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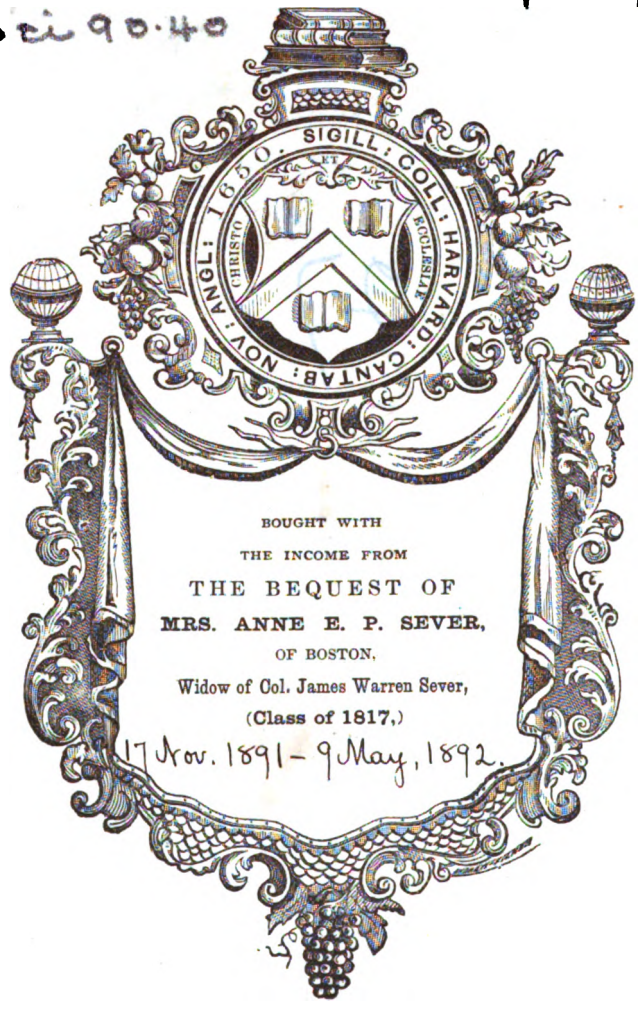

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Supplement to Nature,
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Nature

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ILLUSTRATED JOURNAL OF SCIENCE

VOLUME XLV

NOVEMBER 1891 to APRIL 1892

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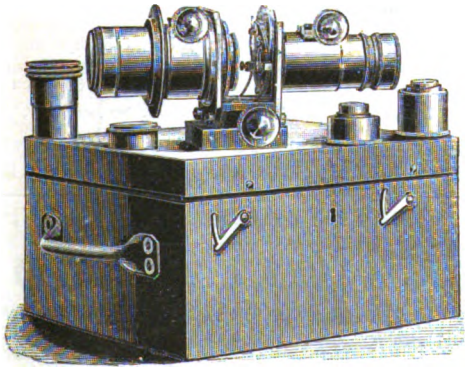

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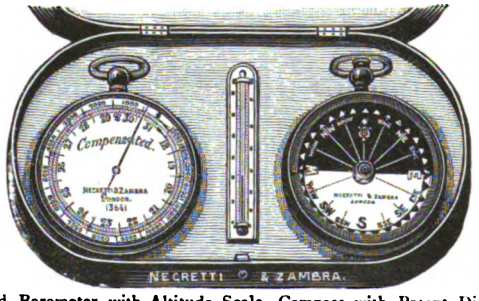

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ELECTRICITY AND MAGNETISM.

Electricity and Magnetism. Translated from the French of Amédée Guillemin. Revised and Edited by Silvanus P. Thompson, D.Sc., F.R.S. (London: Macmillan and Co., 1891.)

THIS work is an English translation of M. Amédée Guillemin's popular treatise of electricity. We are informed, in the preface, that the translation has been in great part executed by Mr. Colman C. Starling and Prof. Walmsley, under the editorship of Dr. Silvanus P. Thompson. It is a splendidly illustrated and beautifully got-up book, designed, so the editor says, rather for the table of the drawing-room than for the desk of the student.

We doubt whether, in fashionable drawing-rooms at any rate, scientific curiosity exists to any great extent; but now that large houses are very frequently lighted with electricity there may be a minority of people who are willing to spend any spare time left over from more absorbing drawing-room occupations in learning something of how the light is produced and of other applications of electricity. For such a public the present work seems exceedingly well adapted. It is popularly and attractively written, so far as a translation from a foreign tongue, supplemented, and to some extent corrected, by editorial paragraphs, can well be; it is profusely illustrated, and comprehensive to an extent which has made the book almost too bulky for convenient perusal.

Still, the remnant of people by whom popular scientific treatises such as this are welcomed, though numerous in itself, is, alas, only a very small minority of that great and influential section of the British public who are brought directly into contact every hour of their lives with the wonderful practical results of the progress of science. The great majority converse through telephones, consult their watches, and send telegrams, and know no more than a Hottentot does how a telephone acts, a watch goes, or a telegraph message is transmitted.

The book is divided into two parts, dealing respectively with phenomena and their laws, and practical applications; or, speaking briefly, theory and practice. In the theoretical part, magnetism is first treated, then electricity, in the order statical electricity, electro-chemistry, and electro-magnetism. In the practical part are comprised telegraphy and telephony, electric lighting and transmission of power, and a number of minor, but in themselves important, applications, such as clockwork-driving and regulation, electricity in warfare, and electroplating. Of the treatment of these subjects we can give here only the merest sketch, noting as we do so a few points in which the book seems to call for modification or improvement in a new edition.

The theoretical part begins with a brief account of the natural history of magnetism, then passes to a discussion of the polar theory of magnetism, starting with the notion of Thales that a magnet had a soul, and ending with the experiments of Coulomb and their results. An excellent description of Coulomb's torsion-balance experiments is given, and then follow the methods devised by Coulomb and Jamin for the determination of the distribution of magnetism in magnets. It is hardly correct to say, as is done on p. 33, that Coulomb's method "enabled him to study the distribution of magnetism in magnets; that is to say, how the magnetism at the surface varies along the magnet between one end and the other." Apart from the objection that the field at any point external to the surface of the bar depends really upon the whole distribution of magnetism, and not merely on that supposed to be near the point, and the further objection (which also does not seem to be stated here) that the vibrating needle itself affects the magnetization of the magnet, it is quite certain that this method, like others devised for the same purpose, cannot be made to give any definite information except as to the surface-distribution of magnetism, which, as Gauss showed, can be made to replace the magnet so far as the external field is concerned. By none of these methods can any information whatever be obtained as to the actual magnetization of a bar of finite cross-section.

It would have been well also if the editor had here appended a note as to the essential inaccuracy of Jamin's method "of placing on the point that we wish to study a

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small contact-piece of soft iron, and of measuring by means of a graduated spring that gradually extended, the force requisite to detach the iron," and given a description of the much more satisfactory method adopted by Rowland and others.

After a chapter on methods of magnetization, in which all the ancient and now discarded methods of "touch" are described, we have an excellent popular discussion of terrestrial magnetism, ending with a splendidly illustrated account of auroræ. The introduction of the subject of auroræ at this point is justified on the ground that they are electrical phenomena connected with the magnetism of the earth, and a sketch is given of the various theories which have been proposed.

Passing now to the subject of electricity, we have the same wealth of illustration, though many of the smaller cuts, like some of those in the section on magnetism, are old familiar friends. Electrical machines are described, from Otto Guericke's down to Wimshurst's. Nothing impresses us as more indicative of the enormous advance of electrical science in recent times than a comparison of Plates V. and XIII. of this book. The former, a well-known picture, represents an electrical machine "according to the model in fashion about 1754"; the latter, a large Edison steam-dynamo. In the former a bevy of ladies and gentlemen in the costume of last century are grouped round a sulphur ball machine, which a gentleman in powdered wig and ruffles is vigorously turning by means of a crank attached to a large and much ornamented driving-wheel of wood. Evidently we have here "electricity in the drawing-room," as practised in the middle of last century. On the other plate we see a large modern steam-engine, in all its array of steam-pipes, balanced cranks, and connecting-rods, resting on a massive bed-plate of iron bolted to a base of masonry, and driving an enormous dynamo. The somewhat *dilettante* group of men and women have disappeared, and in their place stands a typical Yankee engineer, oil-can in hand, and coatless, intently regarding the bearings of the engine. Here there is no unnecessary ornamentation, no suggestion of elegant trifling, everything is sternly suggestive of work and nothing else. Nevertheless, in the contrast, the real dignity and beauty is with the present, not with the past; with modern science in the laboratory, the workshop, or the factory; with work carried on in the deepest earnest, with plain duty-doing, irrespective of sensation or applause.

Next comes an account of batteries, which (like several other parts of the book) we think might very well have been lightened by ignoring old and obsolete pieces of apparatus; after that, we have a discussion of the production of electric currents. In a book of this size, in which a considerable amount of space is devoted to things relatively unimportant, the subject of electrolysis might have been more fully treated; for example, there are matters connected with electrolytic theories to which, since such a theory as that of Clausius is introduced, a few pages might very well have been devoted. The absolute measurement of currents by means of electrolysis from the known electro-chemical equivalents of different substances is not referred to; indeed, an electro-chemical equivalent does not seem to be anywhere defined. But what strikes one as strange indeed is that in the chapter on thermo-electricity Peltier's name is only mentioned in connection with an illustration showing what is called his "thermo-electric pince." Not a word is said on the subject of the Peltier effect, or the Thomson effect, not to speak of the bearing of these on thermo-electric theory! Again, no mention appears to be made of any form of secondary cell except that of Planté: surely some of the modern forms now so largely in use in practice for electric lighting, traction, &c., might have been figured and described.

The next section of theory, electro-magnetism, has three chapters devoted to it. The main phenomena are well described, and excellently illustrated by diagrams. Here the only forms of tangent and sine galvanometer figured are those of Pouillet (one of these (p. 337) has an enormous needle). Some of the splendid instruments which have been made for absolute measurements (for example, Fitzgerald's tangent galvanometer) ought surely to find a place in a work like the present, published as it is at a time when currents, &c., are no longer measured in arbitrary units, and their determinations are as far as possible divested of errors arising from instrumental peculiarities and accidents of place. A definition might also have been given here of the electro-magnetic unit of current, with some indication, where the constant of a galvanometer is referred to, of how it is possible to measure currents in absolute units, and the importance in this respect of electro-magnetic instruments, the constants of which can be determined from their dimensions and arrangement. At p. 333 a current of so many amperes is referred to as producing a certain force at the needle, but we have not anywhere, so far as we have been able to discover, a definition of an ampere.

The following passage (p. 369) apparently quoted from Faraday's "Researches," was at first sight rather startling: "In this state of circumstance(s) the force of the electro-magnet was developed by sending an electric current through its coils, and immediately the *image of the lamp-flame continued magnetic.*" It is almost needless to say that a reference to the "Researches" showed that the copyist had dropped out a line from Faraday's account of the actual phenomenon, which was not exactly that asserted in the quotation. After "flame" supply the words "became visible, and continued so as long as the arrangement."

The second part of the book is most excellent. All applications of electricity of any importance are fully described, and magnificent cuts, without stint, illustrate in the clearest manner the marvellous and complex contrivances and arrangements now in use in the various systems of telegraphy and telephony, electric lighting, &c., &c. Full-page plates of the illumination of Tunis by the search-lights of the French fleet, the electric light in use in the erection of a great Parisian *magasin*, the head-light of a locomotive illuminating the track, the interior of one of the Paris forts during the siege, and other subjects, serve to show the great part now played by electricity in all branches of industry and the arts, even including warfare, slow as that is in some respects to profit by the latest results of scientific invention. No book could form a more attractive and useful present for a boy with a taste for mechanics and practical electrical

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science, and it is sure to be no less popular among older people who appreciate a sound and easy guide to the mysteries of practical electricity.

In taking leave of this work, we have only to say, what has already been indicated above, that an extension of the editorial remarks, and their absorption into the general current of the text, with consequent re-writing of some of the chapters, would render it more homogeneous, and throughout more in accordance with the electrical spirit of the age. Still, the clearness of its arrangement and style more than compensate for the disadvantages necessarily attending an edited English edition of a foreign scientific treatise, however popular. As a whole, it reflects credit on all concerned—translators, editor, and publishers alike. Its publication may even do something towards arousing an interest in electricity in circles, even in this proverbially practical country, where the light of science can hardly be said to have yet penetrated.

A. GRAY.

BIOLOGY OF SEASIDE PLANTS.

Die indo-malayische Strandflora. Von A. F. W. Schimper. Mit 7 Textfiguren, einer Karte, und 7 Tafeln. (Jena: Gustav Fischer, 1891.)

Ueber die Mangrove-Vegetation im malayischen Archipel. Von G. Karsten. *Bibliotheca Botanica*, Heft 22. (Cassel: Theodor Fischer, 1891.)

THESE two essays are exceedingly interesting contributions to our knowledge of plant-life on tropical sea-shores. They partly cover the same ground, partly supplement each other, and to some extent review and summarize the work of previous observers. Schimper treats of the salt-loving plants of the sea-shore generally, whilst Karsten's investigations are limited to the purely mangrove vegetation. Karsten also enters more fully into the formation of seeds—that is to say, into the development of the embryo-sac, the endosperm, and the embryo; and he follows up their germination and subsequent growth.

But the object of this notice is to give some general idea of the subject rather than a critical exposition of the writings of the authors named, for they are the first attempts at a connected description of the vegetation of tropical sea-shores.

The mangrove[1] vegetation—that is, the vegetation of the tidal forests—exhibits comparatively little variety, though the components belong to several different natural orders. First come the Rhizophoreæ—genera Rhizophora (both in the Old World and in America), Bruguiera, Ceriops, and Kandelia; Combretaceæ—Lumnitzera (Laguncularia in America); Lythraceæ—Sonneratia; Meliaceæ—Carapa (*Carapa guianensis*, a native of tropical America and west tropical Africa, does not appear to inhabit the tidal forests); Myrsineæ—Ægiceras; Rubiaceæ—Scyphiphora; Verbenaceæ—Avicennia (both in the Old World and in America); Acanthaceæ—*Acanthus ilicifolius*; Palmæ—*Nipa fruticans*.

The foregoing are the principal and widely-spread trees and shrubs of the mangrove girdle of muddy tropical shores; but this list might be largely augmented if we included those forming the tidal forests of the Bay of Bengal, and similar situations. Thus, in the Sunderbun, as Mr. C. B. Clarke informs me, the Sundra tree (*Heritiera Fomes*) abounds to such an extent that a railway is almost entirely devoted to carrying the wood to Calcutta, of which city it is the fire-wood. Among other common trees and shrubs are *Hibiscus tiliaceus*, *Sapindus Danura*, *Dalbergia monosperma*, *Derris uliginosa*, *Oxystelma esculentum*, *Dolichandrone Rheedei*, *Premna integrifolia*, *Clerodendron inerme*, *Pandani*, *Phœnix paludosa*, and *Cocos nucifera*. Mr. Clarke further informs me that the milk of the coco-nut in the Sunderbun is so salt as to be undrinkable. This is a very remarkable fact, and scarcely in harmony with the observations of Schimper, Karsten, and others, so far as mangrove plants are concerned generally.

In this connection it may be mentioned that mangrove plants have mostly very thick leaves, with few, very deeply seated stomata, so that transpiration is reduced to as low a minimum as in true xerophytes. As it is obvious that transpiration is not checked in halophytes because of a lack of water, it must be accounted for in some other way; and, as it has been found that the accumulation of salt in the tissues of the leaves beyond a certain quantity, varying in different plants, prevents the formation of starch and glucose, it is assumed that it is of a protective character; that, in short, smallness of transpiration means smallness of absorption, and thus no more salt is taken into the tissues of the plant than it is capable of assimilating. The correctness of this view is strongly supported by the fact that mangroves, grown in soil free, or practically free, from chloride of sodium, develop foliage of less substance, furnished with a larger number of stomata.

Turning to another phase in the life-history of mangroves—namely, reproduction—we find special provisions, suitable to the exceptional conditions, to insure the propagation of the species. Most of the members of the Rhizophoreæ, for instance, are, in a sense, viviparous—that is to say, the seed germinates on the parent plant. Only one ovule is developed, the rest being aborted; and when the seed is ripe, the radicle, or primary root, grows through the apex of the fruit, assuming a slender club-shaped form, with the centre of gravity nearest the organic base, so that, when it eventually separates from the parent, it falls in such a manner that the radicle penetrates the mud, and usually sufficiently to withstand the ebb and flow of the water. The size and length of the viviparous radicle varies considerably in different genera, and even in different species, of the same genus, attaining its greatest development in *Rhizophora mucronata*, the forepost of the Asiatic mangroves, and perhaps the only one that sometimes grows where the soil is always submerged. In this the viviparous radicle is

[1] The word mangrove looks quite English, but it appears to be a corruption or modification of *mangro* or *mungo*, the name commonly applied, according to Rumpf (1750), and Blume (*Museum Botanicum*, i. p. 132), in Dutch Guiana to *Rhizophora Mangle*. However, it was employed in its present form by Dampier, Sloane, and other writers of the seventeenth century, and it is now applied to a number of different trees and shrubs that constitute the outermost fringe of vegetation on tropical coasts. It is also used to designate these shrubs and trees collectively. *Mangi-mangi* is the generic term in the Malay Islands for these trees and shrubs, and the different kinds are distinguished by affixes. In Brazil, *Rhizophora Mangle* is called *mangle* and *mangue*; and in Panama, on the authority of Seemann ("Die Volksnamen der amerikanischen Pflanzen"), the former name is current, with various qualifying affixes. In Grisebach's list of colonial names of plants ("Flora of the British West Indian Islands," p. 785), we find mangrove (*Rhizophora mangle*); black mangrove (*Avicennia nitida*); white mangrove (*Laguncularia racemosa*); and Zaragoza mangrove (*Conocarpus erectus*).

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usually from twenty to twenty-four inches long, and occasionally as much as forty; and it is capable of growing even should it fall where it is wholly under water in the early stage of its further development. When the young plantlet is ready to separate from the parent, the aperture made by the growing radicle is sufficiently large to allow the inclosed or apical end to slip out, leaving the empty fruit still attached to the branch. And when this happens, there is a fully-formed leaf-bud at the top, from which the stem is developed. The primary root does not grow much after falling, but stout secondary roots are thrown out from this axis, successively, one above the other; and as they assume an arched form, and are produced in all directions, the plant becomes very firmly fixed. The American *Rhizophora Mangle* is very closely allied to the Asiatic and African *R. mucronata;* but whereas there is only one genus and one species of the order in the New World, there are several in the Old.

Singular to say, the only herbaceous plant of the Asiatic mangroves, *Acanthus ilicifolius*, is supported by similar stilt-roots. Most of the other trees and shrubs of the mangrove vegetation have horizontal roots, often of enormous length and strength, and some of them produce the so-called knee-roots in great abundance. These roots grow out of the ground, at an angle of about 45°, to a height of a foot or two, or perhaps more, and return to the ground at about the same angle, forming an anchor-like attachment. But their function is not merely to hold the plant. They are abundantly furnished with lenticels, through which the interchange of gases takes place—at least, such is the opinion of several eminent physiologists. Indeed, Karsten designates them breathing roots. Schimper figures negative geotropic roots of *Avicennia tomentosa*, which grow quite erect, from a thicker horizontal root, to a height of about a foot, and are either undivided or forked, and taper to a point. They are thickly studded with lenticels, as are the stilt-roots of *Rhizophora*. Another modification of root-production is exhibited by some of the mangrove-trees. Like *Rhizophora*, they produce aërial roots; but, instead of their remaining free, they eventually grow to the stem and outwards, forming plate-like buttresses.

Many other interesting facts might be extracted from the papers cited; but enough has been said to give an idea of the nature and value of their contents.

W. BOTTING HEMSLEY.

RICARDO'S "POLITICAL ECONOMY."

Principles of Political Economy and Taxation. By David Ricardo. Edited, with Introductory Essay, Notes, and Appendices, by E. C. K. Gonner, M.A., Lecturer on Economic Science, University College, Liverpool. (London: George Bell and Sons, 1891.)

THIS edition of Ricardo's "Principles" will be found useful to students of political economy. In addition to a large number of footnotes, the editor contributes an introductory essay of forty pages, and two short appendices—(1) on Ricardo and his critics, (2) on the effect upon rent of improvements in the fertility of land. The introductory essay gives a general account and brief critical estimate of Ricardo's work. It is characterized by judicial moderation and impartiality; and many ambiguities and obscurities, due to the defects of Ricardo's style, are cleared away. Naturally, the abstract theory of value is treated first; and here the editor acknowledges that Ricardo did not attach sufficient importance to the influence of demand in determining value. But, on the serious question of the relation of capital to labour, he hardly seems to make Ricardo's position clear. He says (p. xxxix.):—

"Of course, the mere fact that capital is subject to such replacements enables us to assert that, in the long run, there is a tendency to some equality of reward between indirect labour (*i.e.* labour embodied in capital) and direct labour. Thus in a somewhat abstract and general way we may renew our previous statement that commodities exchange in the ratio of their cost of production."

This passage, in which the editor concludes his general criticism of Ricardo's theory of cost of production, appears to involve the very fallacy that some Socialists have committed in their reasonings based on Ricardo. For it suggests their doctrine that capital is nothing but labour applied *indirectly* to production. Now Ricardo most explicitly avoided this fallacy. He wrote (p. 27):—

"On account of the time which must elapse before one set of commodities can be brought to market, they will be valuable, not exactly in proportion to the quantity of labour bestowed on them, . . . but something more to compensate for the greater length of time which must elapse before the most valuable can be brought to market."

In short, Ricardo distinctly points out that an additional value arises when the same quantum of labour is extended over a larger period of time.

On the question of the distribution of reward between capital and labour, the editor remarks (p. xxxviii.):—

"The two great agents in production—labour and capital—so divide total value between them that an increase in the value obtained by the one implies a diminution in the share of value falling to the other."

This apparently harmless truism is elaborated with painful prolixity. But the form in which Ricardo applied it was always "Profits depend on wages"—never "Wages depend on profits." With Ricardo, profits were the residue of production remaining over and above the value of the standard of comfort; and he did not enter too closely into the question of the forces determining variations in this standard. This crucial error shows itself throughout all Ricardo's reasonings—notably in his theory of taxation.

In Appendix B, the treatment of the effects upon rent of improvements in the fertility of land is very unsatisfactory. The editor says that Ricardo made two assumptions—one implicitly and the other explicitly. But if he had properly interpreted the assumption explicitly made, he would have seen that the other was unnecessary. Ricardo explains quite clearly that the contemplated improvement is assumed not to disturb "the *difference* between the productive powers of the successive portions of capital." The editor most gratuitously interprets *difference* to mean *ratio*, in the face of the fact that *all* Ricardo's illustrations assume constancy of difference, not constancy of ratio. Now Prof. Marshall has shown

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that, with Ricardo's premiss, his conclusion is absolutely correct without any further assumption. If, on the other hand, we adopt constancy of *ratio* (instead of constancy of *difference*)—which was Mill's (not Ricardo's) supposition—then some further assumption must be made in order to demonstrate that improvement in fertility produces diminution of rent. In proving this point, the editor uses an unnecessarily complicated piece of mathematical reasoning.

Without further dwelling on these defects, it is only necessary to say that the explanatory footnotes are everywhere extremely helpful, and that the frequent references to Ricardo's "Letters to Malthus" will be found especially useful in further elucidating the great economist's doctrines. W. E. J.

OUR BOOK SHELF.

Photographic Pastimes: a Hand-book for Amateurs. By Hermann Schnauss. Translated from the Second German Edition. (London: Iliffe and Son, 1891.)

MANY and varied are the effects that can be produced with the aid of the camera, and the present work gives a plain and popular account of the methods that have been adopted in producing them. The five chapters are headed, respectively—specialities, curiosities, photography by peculiar arrangements, photographic optical entertainments, and entertainments with photographic prints.

In carrying out the experiments contained under the first two headings, amateurs will find their time fully occupied, while the novel effects that can be obtained will afford both instruction and amusement. With reference to taking pictures by moonlight, we can quite agree with the author when he says that "if the moon is included in the picture, its track will make a straight band of light nearly half-way across the photograph, which, besides the peculiar illumination of the landscape, gives a *most characteristic effect*." The characteristic effect, we should think, would be very decided.

An excellent and easy method of producing ghosts, which may prove useful to amateurs, and which is not wholly described in this book, is as follows:—The ghost consists of a person completely covered over with a sheet, the latter being so adjusted as to give a dim outline of the head; when in position, a short exposure of about half an inch of magnesium is given: then, as soon afterwards as possible, without moving anything with the exception of the ghost (which now is no longer required), another exposure is made, by means of a magnesium flash light, of the other figures that are required for the picture. In this manner excellent results have been obtained, the pattern on the wall appearing through the ghost, giving it quite a realistic appearance.

In these and the remaining chapters, descriptions of many novelties too numerous to mention are given, of which the following may serve as types—caricature, composite, and pin-hole photographs, statuary portraits, kaleidoscopic and stroboscopic pictures, &c.

Altogether, amateurs will find in this hand-book much that will occupy them during the winter months, when out-door photography is more or less at a standstill.

On Surrey Hills. By a "Son of the Marshes." (Edinburgh and London: W. Blackwood and Sons, 1891.)

THE Surrey hills are so well known that an ordinary writer would find it hard to say anything fresh about them. The "Son of the Marshes," however, has an exceptionally good power of observation, and even familiar facts he is able to present in a way that seems to give them new vitality. In all his books he is especially interesting in passages dealing with the habits of animals, and there are many such passages in the present volume. No secondhand information is offered; the author tells us only of things which he himself has had opportunities of noting. Most of the chapters have already appeared in *Blackwood's Magazine*, but many who read them there will be glad to possess them in their present form. The manuscripts of the "Son of the Marshes" have, as usual, been edited by Mr. J. A. Owen, who does not say precisely how much his editorial work includes.

Heroes of the Telegraph By J. Munro. (London: Religious Tract Society, 1891.)

THE author of this book desires that it shall be regarded as in some respects a sequel to his volume on "Pioneers of Electricity." He begins with a short account of the origin of the telegraph, and then sketches the lives and principal achievements of those discoverers and inventors to whom we owe the electric telegraph and the telephone—Charles Wheatstone, Samuel Morse, Sir William Thomson, Sir William Siemens, Fleeming Jenkin, J. P. Reis, Graham Bell, Thomas Alva Edison, and D. E. Hughes. In an appendix, Mr. Munro gives brief accounts of various other investigators whose names are intimately connected with his subject. He has a plain, straightforward style, and the book will give much pleasure to young readers who take interest in the practical applications of science.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Koh-i-Nur.

DR. BALL, in his reply (NATURE, vol. xliv. p. 592) to my criticisms on his "true history" of the Koh-i-Nur, feels aggrieved that I "smite him in season and out of season," and considers me in the light of a partisan for doing so. I can assure him that my criticisms were absolutely impersonal, as I have never, to my knowledge, seen him in my life, and bear no kind of ill-natured feeling towards him; indeed, I said whatever I was able honestly to do in favour of his work. But of course, where I considered his arguments to be groundless or illogical, I met them. If he has read into my remarks an asperity I did not desire to impart to them, surely he should blame himself somewhat for the style of his attacks on those who went before him, and of whom I have shown that they knew not less, but more, of the subject than he did himself.

I have pleasure in withdrawing my expression of an accusation that Prof. H. H. Wilson was one of those against whom Dr. Ball threw out a sneer in relation to the earlier history and traditions attaching to the Koh-i-Nur. I supposed that, as he has laboured to make his knowledge of the authorities on the subject complete, he would certainly have known what was of common knowledge at the time as to the authorship of by far the most interesting notice ever penned on the Koh-i-Nur. But that was long ago. It was that notice, however, that brought me into such contact as I have had with the subject. As a young Professor at Oxford, I had the honour of knowing the great master of Sanskrit and of Indian lore: and as I had been interested in Indian history I ventured to approach him now some thirty-five or thirty-six years ago on the subject of the values assigned by him to certain weights referred to in his article. I drew his attention to Babar's valuation of the mishkal in ratis, and I further pointed out the probability of the retention by Shah Jahan of the Mogul diamondin his captivity. He received my suggestions in the kindest spirit and offered me every help in further inquiry; and at the East India Company's Library he placed all the documents before me.

I shall not weary your readers with thrashing out and again winnowing the various statements involved in this controversy.

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I could say more about the Garcias-De Boot matter, but I am satisfied with having shown that it was not Dr. Ball, but Mr. King, who, twenty-five years ago, explained the misprints in De Boot, and declared the very great improbability of the 140 mangelin diamond of Garcias, estimated by De Boot at a weight of 187½ carats, not being the Koh-i-Nur. Dr. Ball alludes to inaccurate figures in Mr. King's treatise. That Mr. King was inaccurate, was hasty, no one knows better than I. Nor did any of his many warm friends lament more than I did the unhappy infliction of advancing blindness which explains so much of the former demerit, as no one admired more than I the boy-like enthusiasm which often gilded in his imagination what seemed to others metal of a less precious order than gold. He had a splendid memory, and he trusted too much to it in drawing out from it, rather than throwing on his impaired eyesight, the verifying the records of his enormous reading and varied knowledge. I had controversies with him over a thousand subjects, but while he kept singularly isolated, and let no one come between him and his printer, he never resented a friend's criticism or difference from him.

As regards the scene before the throne of Aurungzebe, it can never, perhaps, be determined whether the view first put forward by Prof. H. H. Wilson, that Tavernier weighed the diamond, but with weights and scales supplied by Akil Khan its custodian, is the correct one; or the view I have held—namely, that Tavernier's account of the transaction given in his tenth chapter was barely compatible with his having weighed the stone, as he asserts he did in the twenty-second chapter of his book, which was avowedly a retrospective one written long afterwards, and near the end of his life. That I have reason for adhering by preference to the latter view is confirmed by what Dr. Ball himself says of another passage referring to the Great Mogul diamond. Dr. Ball condemns the passage as "in part spurious if not altogether so, . . . as the statements are in contradiction with others made elsewhere in the 'Travels'; and there is the strongest reason for attributing them to an erroneous editorial interpretation, and not to Tavernier himself." The delinquent he supposes to have been a M. Samuel Chappuzeau, the reputed editor of Tavernier's works.

As a fact, the travelled Frenchman seems to have been a person somewhat illiterate, as he had to call in extraneous aid in putting his memoirs into shape. He must be supposed to have picked up some colloquial Persian, but otherwise seems to have been dependent on interpreters throughout his travels. The treatment Chappuzeau received during a year of editorial service at the hands of Tavernier and his wife is recounted by Dr. Ball as a sort of "mortification, if not martyrdom." Chappuzeau appears to have described the notes of the traveller, on which he had to depend, as a chaos, and to have attributed the only written part of them to the penmanship of one Father Gabriel. I think I am justified, then, in asking whether the account of the weighing in the later chapter may not have been an editorial afterthought; but whether it were so or was historical, in the sense assigned to it by Prof. Wilson, really very little affects the question.

The logical issue of this discussion is involved in the acceptance of one of two alternatives, the one a series of astounding coincidences and improbabilities, the other one of simple probabilities. Garcias saw a diamond weighing 140 mangelins; Le Cluze estimated its weight at 700 apothecary grains (= 573·8 grains troy, or 180 carats). De Boot assigned to it a weight of 187½ carats. The Koh-i-Nur weighed 589½ grains, or 186 carats. Misinterpreting a note of Le Cluze, Dr. Ball throws scorn on this having anything to do with the Koh-i-Nur.

Tavernier sees a diamond to which a weight is assigned of 319·5 ratis. Babar's diamond (the Koh-i-Nur) weighed 8 mishkals, or 320 ratis, equivalent to about 186 carats. Dr. Ball says this diamond was that known as the Great Mogul, that it is the Queen's Koh-i-Nur, but that it was whittled down by necessitous princes—to find them, in fact, in pocket-money—from 280 carats to 589½ grains, or 186 carats, the identical weight of Babar's diamond and of the Koh-i-Nur. Dr. Ball finally declares the Darya-i-Nur to have this same weight of 186 carats.

In opposition to this impossible recurrence of coincidences I have endeavoured to show that the stone Garcias saw may have been the Koh-i-Nur, that the one Tavernier handled was in all probability—I believe was certainly—the Koh-i-Nur. I say there is no evidence whatever of the Koh-i-Nur having been whittled down by cleavage, accidental or intentional; that its form in 1851 was more probably its original form rudely facetted (and I think, perhaps, I may not be without a mineralogist's experience when I say this); I further say that the Darya-i-Nur is undoubtedly the "Golconda table" diamond.

Finally, I assert the probability that the Great Mogul, unwhittled down and entire, is in the jewel chamber of the Shah of Persia to this day.

Of the great diamond which I would identify with this stone I append a tracing, in which it is seen in its carcanet of ruby-enamel. In the original drawing it is accompanied to right and left by two large diamonds, similarly girdled; while, above and below, is a row of three enormous rubies encircled by emerald-enamel. Ten pearls above and ten below, some of them ¾ of an inch in diameter, form a fringe to this gorgeous ornament. It

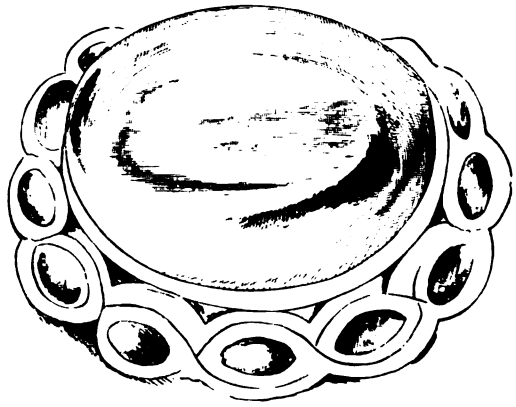

FIG. 1.—Great Mogul.

is, however, only one half of a cylindrical cap the corresponding half of which is its counterpart in splendour and wealth of stones, only the Darya-i-Nur is in, that other half, the central ornament.

I leave the great stone to speak for itself in the tracing, and I furthermore for comparison give a tracing from a drawing of the Koh-i-Nur, taken from a somewhat similar point of view—that is to say, looking down on it.

That the Koh-i-Nur was valued beyond these greater stones I believe to have been in consequence of its being the reputed talisman of Indian empire. It was probably that last relic of his treasure surrendered by the miserable Muhammad Shah when he exchanged caps with Nadir, and the conqueror saluted this most historic of his spoils by the name it has since borne—

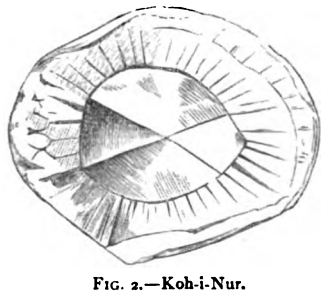

FIG. 2.—Koh-i-Nur.

the Koh-i-Nur. It was certainly the diamond that Shah Rukh, after yielding up all his wealth of jewellery, held to through every torture till he gave it to Ahmad Shah. Shah Zaman carried it to his prison, and secreted it in a crevice; whence Shah Shuja recovered it on information from his blinded brother.

Shah Shuja again clung to the old talisman not less fiercely than those who had preceded him, till he surrendered it to Runjit Singh under pressure which amounted to compulsion; and memorable was the answer of Shah Shuja to Fakeer Nur-ud-dín, who had been sent by Runjit to ask in what its value consisted. It is "good luck," said Shah Shuja, "for he who has possessed it has done so by overpowering his enemies."

I have put, I hope clearly, to my readers, the alternative and

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conflicting interpretations of the portion of the accounts of the Koh-i-nur from Babar's time onward. There are still some interesting questions of a difficult kind regarding its history antecedent to the days of the Mogul Empire. But I believe I have said now my last word regarding the later history, and leave to my readers the decision as to the side in this little controversy on which the truth is more likely to lie.

N. STORY MASKELYNE.

Basset Down House, October 26.

A Rare Phenomenon.

AURORAS were visible at Lyons, New York, on September 9, 10, and 11. That on September 9 was very fine, flickering streamers and arches forming at intervals from 8 o'clock to 10 o'clock p.m. A peculiar feature of this aurora was an arch similar to that described in NATURE of September 17 (vol. xliv. p. 475), as having been seen by Mr. Tuckwell at Loughrigg, Ambleside, on September 11. The arch seen at Lyons on September 9 was visible shortly after sunset, and remained in the same position throughout the evening. It consisted of a narrow band of light, which arose vertically from a point on the horizon nearly due west, and passed through the constellations of the Northern Crown and the Lyre, and just south of the zenith down to the eastern horizon. When it was brightest, at about 10 p.m., a few small streamers formed in connection with it nearly in the zenith; otherwise it consisted simply of a narrow band of white light separated by a wide interval from the auroral coruscations and streamers in the northern heavens. This seems to have been very similar to the band seen by Mr. Tuckwell. Other instances have been noted by the writer in which some peculiarity of form or colour characteristic of an outbreak of the aurora has attended its appearance in localities remote from each other.

M. A. VEEDER.

Lyons, N.Y., October 17.

Two instances of the occurrence of the rare phenomenon referred to in your issue of September 24 (vol. xliv. p. 494), by Prof. R. Copeland and Mr. W. E. Wilson, will be found recorded in the Transactions of the Nova Scotian Institute of Natural Science, vol. vi. p. 100. The dates of these occurrences were July 31 and September 5, 1883. The general appearance and position of the luminous arch were the same in both cases as in those described by Prof. Copeland and Mr. Wilson. Two additional points were noted, however, which are worthy of mention, viz. (1) that the arch of September 5 had a slightly marked rayed structure, which, when first observed, was in the direction of its length, but which gradually changed to a direction inclined about 45° to the longitudinal, and (2) that the spectrum of this arch, as determined by one of Hilger's pocket spectroscopes, consisted of two lines in the green, one quite bright and the other faint.

On Tuesday, September 1 of this year, I again observed the same phenomenon at Halifax, N.S. I was unable to make accurate observations, but noted the following facts:—The luminous arch was quite bright when first observed, at 11.30 p.m., and extended from horizon to horizon. Fifteen minutes later it had completely faded away. It was about 4° or 5° in width throughout its whole length. It met the horizon at points about 10° or 15° to the north of the east and west points, and passed through a point a few degrees south of the zenith. When first observed, it was approximately uniformly bright throughout, except at the edges, where its brightness diminished rapidly outwards. To the eye its light seemed to be white, and stars were visible through it. In fading away, the east and west ends disappeared first, and the main body of the arch became gradually fainter, wider, and more variable in width. The night was bright and clear, and the temperature lower than it usually is in the beginning of September, and there was no appearance of aurora in other parts of the sky.

Except on this occasion I have neither observed this phenomenon nor heard of its occurrence since 1883. But as it might readily occur without my either seeing it or hearing of it, I cannot say that I know it to be rare.

J. G. MACGREGOR.

Dalhousie College, Halifax, N.S., October 14.

IT has twice been my good fortune to observe phenomena similar to that described in NATURE of September 24 (vol. xliv. p. 494). My recollections of the first occasion are somewhat indistinct, but at all events the luminous band extended east and west almost through the zenith, and was preceded by an auroral display. It occurred in August or September of 1883 or 1884.

My attention was again directed to a similar appearance on the evening of September 9 of the present year, while near Toronto. The narrow band of light, as before, extended from the eastern almost to the western horizon, passing through the zenith, and was accompanied by an aurora.

It is worthy of note that I saw the phenomenon at Toronto on the evening of September 9, not September 11.

R. N. HUDSPETH.

Bishop's College, Lennoxville, P.Q.

Apparent Size of Objects near the Horizon.

SOME years ago there appeared an account of an investigation into the cause of the sun and moon looking larger when low down than when high up in the sky. The theory advanced as the result of the investigation attributed the effect to a physiological cause. One could not expect an explanation of this kind to be applicable to all individuals, but rather that with different persons there would be different results; so I have made observations—81 in number—to find out what law applies to my own case. These observations were made by taking notice of two stars near the horizon, and then looking up near the zenith to see what stars in that situation appeared to be the same distance apart as those near the horizon. I took a great variety of different cases, the length of the compared arcs varying from 1°·4 to 100°. I observed them also in various angles of position, from horizontal to vertical; and sometimes had the two arcs at the same angle of position upon the retina, and at other times at different angles.

The result of this investigation is an unexpected one, showing that the length of the observed arc greatly affects the result of the estimation—short arcs appearing longer when near the horizon than when high up, and long ones appearing shorter.

The comparisons were made in either of two ways; according to one method, after I had carefully taken note of the apparent length of the arc near the horizon, and had fixed an idea of it in my mind, I then took a single glance at the stars near the zenith and fixed in a moment upon an arc that appeared to be of the same length; whereas in the other plan I made as deliberate and careful an estimation of the arc near the zenith as of that near the horizon with which it was compared, looking to and fro from one to the other till I was satisfied as to their apparent equality.

One would naturally expect that the instantaneous estimations would be less accurate than the careful ones, and this is found to be the case. Taking all the observations, I find the average deviation from the truth of a single estimation is 7·7 per cent. in the case of careful comparisons, and 10·3 per cent. in the case of the instantaneous ones. The following formula is based upon the *careful* comparisons—

$$L = l\left\{1 + \frac{A^\circ - a^\circ}{74^\circ}(\cdot085 - \cdot00321l)\right\},$$

where l and L are the lengths (in degrees) of apparently equal arcs at a°, the lower altitude, and at A°, the higher altitude, respectively. According to this formula, an arc 26°·48 long appears the same length at whatever altitude it is situated, but an arc shorter than 26°·48 appears longer at the horizon than at the zenith, and an arc in excess of 26°·48 would actually appear longer near the zenith than near the horizon: an arc 1°·4 long (the shortest in my observations), when at the horizon, would appear equal to an arc in the zenith 109·85 per cent. of its length; while an arc 100° (about the longest in my observations) at the horizon would appear equal to an arc of 71°·30 only in the zenith (*i.e.* with its middle point in the zenith). When the above formula is applied to all the observations, the average deviation (of the observed lengths from the computed) is reduced to 4·2 per cent. in the case of the careful comparisons, and 7·0 per cent. in the case of the instantaneous ones. If this formula can rightly be applied to objects of such small dimensions as the sun and moon, it (as will be seen) allows only a small increase for their apparent size near the horizon upon that when they are seen at a considerable altitude.

It would be easy to find a more complex formula which would satisfy the observations still better, but these are not sufficiently numerous to warrant the doing so.

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It might be supposed that the estimations would agree better when the angles of position are the same for both arcs compared together, than when they are different. But this supposition is not borne out by my observations; for after correcting them by the above formula, the average deviation from the truth in the case of the careful comparisons is 4·4 per cent. when the angle of position of both arcs on the retina is the same, or within 10° of the same; and 4·1 per cent. when it differs more than 10°; while in the case of the instantaneous comparisons these numbers are 7·9 and 6·3 respectively.

When the *lower* arc is horizontal, or nearly so, it is (on the average) estimated as being shorter than when in a vertical position, but the difference is so slight that it is doubtful whether it would not disappear with a larger number of observations. The best correction formula I have obtained for this is to apply the factor

$$(1\cdot04 - \cdot048 \cos d)$$

to the result already obtained: d being the deviation of the lower arc from the horizontal. But the application of this factor only reduces the sum of the squares of the differences between calculation and observation in the case of the careful comparisons from 1163 to 1111.

The angle of position of the *upper* arc seems to make no difference in the results. T. W. BACKHOUSE.

West Hendon House, Sunderland, October 24.

Proper Motions of the Stars.

MISS CLERKE, in her very interesting article (NATURE, vol. xliv. p. 572) on the motion of the sun in space, seems to think that we have only the two alternatives of supposing that the brightness of a star is independent of its distance, or that the motions of the stars increase with their distance. I suspect that, when the proper motions of all stars down to the 9th magnitude have been tabulated, the necessity of adopting either alternative will disappear. My object in writing this letter, however, is to call the attention of spectroscopists to the question thus raised. The spectroscope, when used in connection with a powerful telescope, ought to be able to show whether the fainter stars as a rule move more rapidly in the line of sight than the brighter ones; for if the average motion in the line of sight is the same in both cases, astronomers will be slow to accept an explanation of phenomena which supposes a different average velocity on the whole. But even instruments incapable of deciding this question may throw light on the subject. It now appears certain that if a Sirian and a solar star of the same mass were placed at the same distance from us, the Sirian star would appear more than one magnitude brighter. Hence, before we can use magnitudes as in any sense a test of distance, we must ascertain the relative proportion of Sirian and solar stars in the groups which we are comparing. It would also be very desirable that the magnitudes of the stars employed by Profs. Eastman, Boss, and Stumpe, should be photometrically determined. The photometer has at all events the advantage over the eye that its results are in all cases (allowing for errors of observation) comparable. W. H. S. MONCK.

Dublin, October 17.

California Foxes.

IN NATURE of September 10 (p. 452), there is a paragraph in praise of the intelligence of the (English) fox, with examples in proof. Permit me to say that his California cousin is next door to a fool. My young son has amused himself for the past three summers in trapping (in large box-traps) the small California foxes which infest these mountains, and which live on a mixed diet of Manzanita berries and astronomer's chickens. I pass over the fact that each trap has painted over its door "Danger to all who enter here!", and I proceed to show that our California foxes are barely one remove from idiots. When they are caught, my boy is in the habit of fastening a small leather collar about their necks, and of chaining them with light chains to stakes near the Observatory buildings. Many of them have escaped by parting the chains (by dint of strength, not of intelligence), and have been again caught within two or three days in the same traps! One of them was caught three times in quick succession! I presume an English fox, once caught, would emigrate to North Britain, or at least to the next county. My own ideas of the intelligence of the fox, until I came here, were derived from Goldsmith's "Animated Nature," and, generally, from English writings.

I have now become satisfied that the California fox is *arriéré*. Either the struggle for existence is not sharp here, or he has made up his mind that existence is not worth struggling for.

Lick Observatory, October 8. EDWARD S. HOLDEN.

A Plague of Small Frogs.

MY wine-cellar has been visited during the recent rains with a curious plague of small frogs (*Rana temporaria*) all the same size, about one inch long. There would be nothing surprising in this visitation were it not for the apparent absence of any means of communication from outside, the level of which is six feet above that of the cellar; there is no drain near that part of the house. There is a step up before you go down into the wine-cellar from the adjacent cellar, against which the door closes, leaving no crack any animal so large could squeeze through. The cellar has solid stone walls and a bricked floor. During the recent floods the water stood some three or four inches deep there, apparently oozing through a tiny hole level with the floor on the outside wall, into which the point of a pencil could only penetrate for an inch. Even had it been possible for these little creatures to come in that way they must have burrowed down six feet from the outside level. Only one or two were found in the cellar adjacent, which is lighted by a grating into the garden, whereas in the wine-cellar two or three dozen were caught, many of them drowned by the flood.

Is it not unusual for bats to fly in the day-time? Here one has been doing so for two afternoons, coming out about 2.30, and flying backwards and forwards after insects in most brilliant sunshine. The gardener tells me he has never observed them do so before; and having sometimes caught them in the day-time, always noticed that when thrown into the air they would drop at once, and run instead of flying.

R. HAIG THOMAS.

BOTANY OF THE EMIN RELIEF EXPEDITION.

THE botanical exploration of Tropical Africa leaves so much to desire that it was somewhat disappointing to find that Mr. Stanley brought nothing back which would give any idea of the nature of the dense forests which he traversed. The conditions under which such an expedition is necessarily executed make natural-history-collecting extremely difficult. Travellers, however, often suppose that because they cannot make extensive collections they can do nothing to add to our knowledge. Yet to fill a small portfolio with well-selected and significant specimens is not a very difficult matter. And these may often furnish the basis of useful and important conclusions as to the general nature of the flora. Sir Joseph Hooker was able to give the first account of the vegetation of Kilimanjaro from a small parcel of plants collected by a missionary, the Rev. Mr. New, who was supplied for the purpose by Sir John Kirk, with "a bundle of old *Guardians*." An officer of the Ashanti Expedition brought from Comassi the fruit of what proved to be a new species of *Duboscia*. And quite lately Lord Lamington sent to Kew a small parcel of plants collected by himself in an expedition through the Shan States, which contained good specimens of an interesting plant only known previously from imperfect material collected by Griffith. It has now been worked out and figured in the Kew "Icones Plantarum."

Nor is it so difficult as it might be supposed to do even more than this. And I am not sure that a little careful and intelligent plant-collecting would not be a healthy and useful distraction to the tedium and strain of an arduous journey. Nothing could probably exceed the difficulties under which Joseph Thomson travelled in Masailand; yet he managed, notwithstanding, to get together a tolerably extensive and most valuable botanical collection. Upon this Sir Joseph Hooker was able to base the first attempt at a rational theory of the geographical relations of the high-level flora of Eastern

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Equatorial Africa. Nothing, again, could be more admirable than the collections made by Brigade-Surgeon Aitchison when attached to the Kuram Field Force under Sir Frederick Roberts in Afghanistan. And the Government of India has now arranged—and it is an indication of the sympathy for science which animates its members—that, as part of the organization of the Botanical Survey of India, a botanist shall for the future be attached to all frontier expeditions.

Major Jephson, who accompanied Mr. Stanley, seems, however, to have had his eyes about him. A correspondent has sent me a copy of the October number of the *Mayflower*, a small monthly horticultural periodical published in New York, which contains (pp. 155, 156 a short paper by him on the "Plants of the Ituri Forest African Wilderness." This seems to me worth putting on record in the pages of NATURE, where it will be at least more accessible for future reference. At my request, Mr. Baker, the Keeper of the Kew Herbarium, has had the paper annotated with such critical comments as were possible.

To Major Jephson's paper Mr. Stanley has prefixed a brief introduction, which adds nothing of importance. He remarks:—

"In this branch of science I fancy we were all but amateurs, and considering what very little time any of us could devote from the engrossing business of marching, and seeking for food to sustain life, Mr. Jephson shows what might have been done by him had circumstances been more favourable."

This is, however, erring a little on the side of modesty. As I have already shown, amateurs can do very useful work without much difficulty, if they are content to do only a little, but to do that little carefully. Some further observations are open to more serious criticism:—

"Africa is yet too young and too crude for the scientific botanist. We have only been pioneers to stake the highway to make ready for those who shall come after us. When the rails have been laid in pairs of iron lines across the swamp and desert, and the engined boat cleaves the red bosoms of the great rivers, and furrows the dead green face of the fresh-water seas, then the tender-nurtured botanist, conveyed from point to point without danger to his valuable life, may be trusted, with his enthusiasm and devotion, to bring to us results worthy of science and the age. Of those who have given us an insight into the botanic treasures of the African world, Schivernfuth (*sic*) is by far the best, but he has also laboured under such disadvantages and discomforts that he was not able to do for Equatorial Africa a tenth part of what Bates did for the Amazon."

One cannot but wonder a little at the ignorance of the literature of African travel which this paragraph displays. Men like Grant, Speke, Kirk, Welwitsch, Mann, Vogel, Barter, and Thomson, to mention only a few of those to whom we owe our knowledge of the African flora, would have thought it comical to be described as "tender-nurtured" botanists. The work of Schweinfurth was admirable; yet no one would, I think, be more surprised than that distinguished naturalist, Mr. Bates, to learn that the botanical collections which he never even professed to make, were ten times better.

W. T. THISELTON-DYER.

Royal Gardens, Kew.

"It is difficult to give an accurate idea of the flowers we saw in our march through Africa in a short magazine article, but I here give a short sketch, mentioning some few things which I think may be interesting to my readers.

"The great forest of Central Africa through which we passed is not so rich in variety of flowers and orchids as the forests of Mexico and Brazil, or even the jungles of India and Ceylon. It is chiefly rich in flowering vines, trees, lilies,[1] and Begonias. There is, however, a great wealth of different kinds of ferns, such as I have often seen cultivated in hot-houses in England. In many places the damp ground was covered by a thick growth of filmy ferns and Lycopodium of the most beautiful description.

"Here is a short extract from my journal, which will give some idea of the every-day sights we saw on the banks of the Lower Congo, 1700 feet above the sea, and 250 miles distant from it:—

"'At the bottom of a piece of swampy ground I came to a small stream, on the banks of which were growing *Osmunda regalis*,[2] or Royal fern. It was slightly stunted in growth, being not more than 2 feet in height. It is the first I ever have yet seen in the tropics. Close by the stream was growing a group of beautiful ground orchids,[3] in form like a *Hyacinthus candicans*. There were clusters of great pink flowers with yellow centres; the whole had a very gorgeous effect. Here, also, was a profusion of Lycopodium.[4] It is of a kind I have not yet seen; it creeps up and over everything in great blue-green masses; its long tendrils creep up the tree trunks like ivy, to a height, in some cases, of 4 feet. There were quantities, also, of the ribbon fern, exactly like the *Davallia pentaphylla*,[5] which has been introduced into English hot-houses from the Malayan Archipelago. What would not florists at home have given for an acre of this ground?'

"In the forest there were two kinds of lilies which were common. One, which grew in swampy ground, was in form like an Amaryllis.[6] It was white, with a deep crimson centre, and had a delicious but heavy scent. The other was a lily,[7] which grew everywhere through the whole length of the forest. It was of a brilliant scarlet colour, and was formed of several hundreds of small flowers, forming a round ball like a huge Guelder rose, four inches in diameter. It was of such a brilliant scarlet that it looked almost metallic, growing in the darkest recesses of the forest. One of the commonest and most striking of all the ferns we saw was the *Platycerium alcicorne*.[8] It is an extremely interesting fern, one of a singular genus of epiphytal plants, growing on the branches of trees. Our Zanzibaris called it 'elephant ear,' from its curious shape. There was another of the same family, *Platycerium Stemmaria*, which we found growing upon rocks in the open country. Both these ferns grew at altitudes from 1000 to 5000 feet. Tree-ferns[9] of the ordinary kind we found growing in all the gullies and streams on the slopes of the mountains above the Albert Nyanza. The altitude was from 5000 to 6000 feet above the level of the sea, and I noticed especially that the flora here was remarkably like that in the Central Province of Ceylon, which is an altitude of 2500 to 4000 feet above the sea.

"By far the most common plant which we saw in the jungle was the Amomum, or wild cardamom.[10] It was almost precisely the same in form as the cardamom which is cultivated in Ceylon. It grew almost throughout the whole of Central Africa. It has a large purple flower, which grows in clusters on the ground at the root of the plant, and from it a bright scarlet fruit forms, of a pear shape, and about the size of a small fig. It is divided into four quarters, and contains some white, fleshy pulp, very juicy and acid. This pulp is full of

1 Crinum.

2 *Osmunda regalis* is cosmopolitan, but in tropical zone is high up only.

3 Mr. Rolfe cannot suggest anything better than Lissochilus.

4 *Selaginella scandens*, no doubt.

5 "Ribbon fern" would suggest *Ophioglossum pendulum* or *Vittaria*, but they are not like *Davallia pentaphylla*.

6 *Crinum zeylanicum*. 7 *Brunsvigia toxicaria*.

8 *Platycerium alcicorne* is not African, but *P. Stemmaria* is widely spread.

9 No doubt *Cyathea Thomsoni*, Baker, which is very near *C. Dregei* of the Cape.

10 There are a large number of Amomums in West Tropical Africa. The fruits are 3- not 4-celled. See *A. Danielli*, &c., in Oliver and Hanbury's paper in Journ. Linn Soc., vii. 109."

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small black aromatic tasting seeds like those of the cultivated cardamom. If ever planters go into Africa, the cardamom will be an important product of the soil for commerce, for there are vast tracts of forest with the climate, soil, and checkered shade which are necessary for the cultivation of the cardamon. Orchilla weed should also become a valuable article of commerce; it grows in many parts of the forest. I consider, however, that when the great forest of Central Africa is opened up to civilization, by far the most valuable article of commerce will be india-rubber, the want of which is increasingly felt in the civilized world. Now that electricity is so much used for various purposes, the demand for india-rubber grows larger and larger: the supply which is shut up in the African forest is practically unlimited. There are various trees of the fig tribe which yield this product, but by far the greatest amount is contained in the india-rubber vines[1] which abound in the forest and hang from almost every tree. In cutting our way through the forest in some places, we got covered with the milky glutinous sap, which dropped upon us from the vines we cut through.

"The natives know its value, and use it largely for smearing the inside of their buckets in order to make them hold water. They use it largely also for covering the ends of their drum-sticks. The india-rubber obtained is of a clear, yellowish colour, like glue, and is of the most elastic description.

"In the forest region I saw no water-lilies, but in Emin Pasha's Province, in the Bari country, I saw two kinds.[2] They were both about the size of an ordinary white water-lily, and the leaves and flowers floated on the surface of the water, but the stalks and formation of the leaves and flowers was finer and more slender. One was of a pink coral-like colour, not white like the Zanzibar lily, and the other of a pale bluish lavender. They were growing in small clear pools only a few miles apart in the valley of the Nile, at an altitude of about 3000 feet above the sea.

"One of the most interesting botanical discoveries I made in the forest was the discovery of a wild orange-tree. During our march through the forest I had continually come upon trees varying from 8 to 15 feet high. They had double leaves of a peculiar shape, which had a delicious smell like orange leaves; the branches were covered with long sharp thorns, and I at once pronounced them to be orange-trees. My fellow officers smiled incredulously, and exclaimed: 'Orange-trees[3] in the middle of the forest!' But I held to my opinion, and just before reaching the open country, I came upon a tree with both flowers and fruit upon it. The flowers were exactly the same as the flowers of a cultivated orange-tree. The fruit, which was green, was about the size of a marble. On cutting through it with a knife I found it had the same divisions as an ordinary orange, but each division was full of small seeds, which were very bitter and aromatic. On reaching Emin's Province I told him about it, and he regretted very much that I had not brought a specimen with me, for he was a good botanist, and wished to add it to his collection of dried plants. He told me my discovery was doubly interesting, as many years before a German had penetrated the forest on the west coast of Africa, and reported that he had found wild orange-trees. His story was discredited, and now our discovering the orange-tree in the forest pointed that his report was after all true.

"I have not space to speak much about the flowers we saw in the open country, but will say a few words about those flowers which we found at a high altitude on the slopes of Ruwenzori, or the Mountains of the Moon. Lieutenant Stairs who made the ascent of the mountains, gives the following facts in his report:—

"'The barometer stood at 21·10, thermometer 70° F. Ahead of us and rising in one even slope stood a peak, in altitude 1200 feet higher than we were. This we now started to climb, and after going up a short distance came upon three heaths. Some of these must have been 20 feet high, and as we had to cut our way foot by foot through them our progress was necessarily slow. Here and there were patches of inferior bamboos, almost every stem having holes in it made by some boring insect, and quite destroying its usefulness. Under foot was a thick spongy carpet of wet moss, and the heaths on all sides of us we noticed were covered with "Old Man's Beard" (*Usnea*). We found great numbers of blue violets which had no smell, and from this spot I brought away some specimens of plants for Emin Pasha to classify. The altitude was 8500 feet. We found blueberries and blackberries[1] at an altitude of 10,000 feet. The following[2] are the generic names of the plants collected as named by Emin Pasha:—

Clematis.	Moschosma.
Viola.	Lissochilus.
Hibiscus.	Luzula.
Impatiens.	Carex.
Tephrosia.	Anthistiria.
Glycine.	Adiantum.
Rubus.	Pellæa.
Vaccinium.	*Pteris aquilina.*
Begonia.	Asplenium.
Peucedanum.	Aspidium.
Gnaphalium.	Polypodium.
Helichrysum.	Lycopodium.
Senecio.	Selaginella.
Sonchus.	Marchantia.
Erica arborea.	Parmelia.
Landolphia.	Dracæna.
Heliotropium.	Usnea.
Lantana.	Tree Fern.'

"These were just a few specimens Lieutenant Stairs brought down with him. But the slopes of Ruwenzori will, when properly explored, yield numbers of unknown treasures to be added to the Botanical Encyclopædia.

"For many weeks we drank coffee which we made from the berries of the wild coffee-trees which abound on the highlands round the great lakes of Central Africa. The Arabian coffee was originally supposed to have come from Kaffa, in Abyssinia. That which we found in Karagwe, Ankori, and Uganda is equal in flavour to the finest Arabian coffee, and will, when Central Africa is opened up, be another of the chief articles of commerce.

"I. A. M. JEPHSON."

TOWN FOGS AND THEIR EFFECTS.[3]

UNTIL 1880 the formation of fog was looked upon as arising simply from the separation of liquid water, probably in the form of hollow vesicles, from an atmosphere saturated with aqueous vapour; but in that year Aitken showed that really the determining cause of the separating out of liquid water, and consequent formation of fog, was dust present in the air. He pointed out that a change of state, a gas passing to a liquid, or a liquid to a solid, really always occurred at what he terms a "free

[1] Landolphia.

[2] *Nymphæa stellata* and *N. Lotus* are both plentiful in Upper Nile-land.

[3] This reads like a tree *Citrus*, and if so is an interesting discovery, as no species is hitherto known there.

[1] It would be very interesting to have these identified. The two highest-known species of Rubus are *pinnatus* and *rigidus*, at 5000–6000 feet.

[2] This list is in Stanley's book. The *Viola* is no doubt *abyssinica*, common to the mountains of Madagascar, Abyssinia, the Cameroons, and Fernando Po. There are three heaths known on the high mountains of Central Africa, viz. *Erica arborea*, *Ericinella Mannii*, and *Blaeria spicata*. There is no *Vaccinium* known before in Tropical Africa; though three or four are plentiful in Madagascar, and there is one on the Drakensberg, so that its occurrence is most probable. The ferns of Tropical Africa are nearly all species widely spread in other continents.

[3] The paper by Dr. W. J. Russell, F.R.S., introducing the discussion on Town Fogs at the Hygienic Congress.

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surface"; that as long as a molecule of liquid water is surrounded by like molecules, and the same with gaseous water, we do not know at what temperature, or whether at any temperature, they would change their state; but if in contact with a solid then at the surface, where they meet, the change will occur; if the solid be ice it may become liquid or the liquid may become solid, and the same kind of action occurs when the liquid is in contact with its own vapour; in fact, that what we call the freezing and boiling-points of a body are the temperatures at which these changes take place at such free surfaces. The dust always present in the atmosphere offers this free surface to the gaseous water, and thus induces its condensation. This specific action of dust varies very considerably, first with regard to its composition, and second with regard to the size and abundance of the particles present. Sulphur burnt in the air is a most active fog-producer, so is salt. Many hygroscopic bodies form nuclei having so great an affinity for water that they can cause its condensation from an unsaturated atmosphere. At the same time non-hygroscopic bodies, such as magnesia and many others, are powerful fog-producers; no doubt their activity may in part be attributed to their being good radiators of heat, and thus becoming cooled, induce condensation. Mr. Aitken also shows that the products of combustion, even when the combustion is perfect, are powerful fog-producers, a fact which has important bearing on the production of town fogs. One other point in these experiments I cannot omit mentioning, it is the exceedingly minute amount of matter capable of inducing fog. In his first series of experiments Mr. Aitken showed that $\frac{1}{100}$ of a grain of iron wire, however often it was heated, evolved on each heating sufficient dust to cause a visible fog, and afterwards, with still more delicate apparatus, that $\frac{1}{1000}$ of a grain of either iron or copper, when treated in the same way, gave a similar result, and from these last experiments he infers that even $\frac{1}{100000}$ grain of either wire, if only slightly heated, would yield sufficient nuclei to cause a visible amount of fog. It is of much importance and interest, seeing how small a quantity of dust is required to produce fog, to know that even this small amount may be filtered out of the air by passing it through cotton wool, and thus an air be obtained in which a fog cannot be produced. Mr. Aitken's description of such an atmosphere is at first most alluring, for he says, if there was no dust in the air there would be no fogs, no mists, and probably no rain; but he goes on to state that when the atmosphere became burdened with supersaturated vapour, it would convert everything on the surface of the earth into a condenser; every blade of grass and every branch of a tree would drip with moisture deposited by the passing air; our dresses would become wet and dripping, and umbrellas useless; but our miseries would not end here, for the inside of our houses would become wet, the walls and every object in the room would run down with moisture. I think, if we picture to ourselves this state of things, we may be thankful that there is dust and fog. Dust in its finer forms is invisible to us; but as its delicate particles become loaded with moisture, it becomes a fine mist, dense if the number of particles are many; if, however, the dust-particles are fewer, and the amount of aqueous vapour the same, each particle will have a larger amount of condensed moisture to carry, and it will give rise to a more coarse-grained fog; the moisture, or some of it, will be more feebly attached to its nuclei, producing then what is known as a wet fog, whereas at least a most important fact in the production of a dry fog is the strong affinity between the nuclei and the condensed vapour. As most of you are no doubt aware, Mr. Aitken has invented a most ingenious method for counting the number of dust-particles in air, and has obtained most interesting and valuable results. I can only mention here that some of these results deal with the clearness of air in relation to the number of dust-particles present, and other results show how little effect rain has in diminishing the amount of the finer dust in air. Evidently towns will supply dust of all kinds, and therefore offer every inducement for fogs to form there, and that at least some of the particles will be capable of causing the condensation of moisture even from an atmosphere which is not saturated with aqueous vapour. This condensation of moisture is a very complete process for removing all kinds of impurities from the air. Floating particles are free surfaces, and become weighted by the moisture they condense and tend to sink, and even the gaseous impurities in the air will be dissolved and carried down by the moisture present.

Experiment confirms this, for it has been proved how correctly the impurities of an air can be ascertained by determining the composition of dew, even if it be artificially and locally formed. It is of importance to note that even the purely gaseous emanations from our towns cannot pass away when a fog exists, as is shown by the accumulation of carbonic acid which takes place during a fog. Taking 4 volumes in 10,000 volumes as the normal amount of carbonic acid in London air, some years ago I found that it increased in the case of a dense fog to as much as 14·1 volumes, and often to double the normal amount, which must represent a very serious accumulation of the general impurities in the air.

A fog in this way becomes a useful indicator of the relative purity of the atmosphere in which it forms. If pure aqueous vapour be condensed it gives a white mist—a country fog, a sea fog—and a white light seen through it is not converted into a red light; but in town fogs the whiteness of pure mist disappears and becomes dark, in some cases almost black in colour, the change being produced by the foreign matters floating in the air, and by far the most abundant colouring matters of our town fogs are the products generated by the imperfect combustion of coal; but in addition to these bodies, many others must obviously find their way into the air over a town. Especially will there be dust from the universal grinding and pounding going on in street traffic and many mechanical operations, from the general disintegration of substances and the decomposition of perishable materials—all will add something to the air, and it will become an integral part of the fog. However, although it is often said that a town fog is so dense that it may be cut with a knife, still it is difficult to condense so much of it that it can be subjected to a searching chemical analysis. In 1885, by washing foggy air, I was able to determine the amount of sulphates and chlorides present, and as indicators of organic matter the quantity of carbon and nitrogen in the fog. The results showed strikingly how largely the amounts of organic matter and ammonia salts in the air varied with the weather; no case of dense fog occurred when the experiments were being made; but the mean of several experiments clearly showed that in foggy weather the amount of organic matter was double as much as existed in the air in merely dull weather, and that the amount of sulphates and chlorides increased under like conditions, but not to the same extent. Fog may, however, be made to give its own account of its constituents, for we have only to collect and analyze the deposit which it leaves to learn what its more stable constituents are, and we have to thank the air-analysis committee of the Manchester Field Naturalists' Society for the most complete analysis of such a deposit which has yet been made. The deposit analysed occurred during the last fortnight in February of this year (1891), and was obtained from the previously-washed glass roofs of the plant-houses at Kew, and Messrs. Veitch's orchid-houses at Chelsea. At Kew 20 square yards of roof yielded 30 grammes of deposit. At Chelsea the same area gave 40 grammes, which represents 22 lbs. to the acre or 6 tons

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to the square mile, and the composition of these deposits is as follows:—

	Chelsea. Per cent.	Kew. Per cent.
Carbon	39·0	42·5
Hydrocarbons	12·3	4·8
Organic bases (pyridines, &c.)	2·0	
Sulphuric acid (SO_3)... ...	4·3	4·0
Hydrochloric acid (HCl) ...	1·4	0·8
Ammonia	1·4	1·1
Metallic iron and magnetic oxide of iron...	2·6	
Mineral matter (chiefly silica and ferric oxide)	31·2	41·5
Water, not determined (say difference)	5·8	5·3
	100·0	100·0

These analyses give, I believe, for the first time, a definite account of the composition of fog-deposit. Soot and dust are by far its principal constituents, rendered sticky and coherent by hydrocarbons, but I should like to give you the striking description which Prof. Thiselton Dyer has sent me of this deposit, collected at Kew. He says: "It was like a brown paint, it would not wash off with water, and could only be scraped off with a knife. It thickly coated all the leaves of the evergreens, and upon what have not yet been shed it still remains." In the above analysis it is curious to note the large amount of metallic iron and magnetic oxide of iron.

The details with regard to these very interesting analyses we shall hear from a member of the Manchester Committee, and I will only ask you to note how large a proportion of these deposits arises from the imperfect combustion of coal. We also learn from the Manchester Committee some interesting facts with regard to fog-deposits which occurred last winter in their city. This deposit which was collected from Aucuba leaves contained as much as 6 to 9 per cent. of sulphuric acid, and 5 to 7 per cent. of hydrochloric acid, mostly, of course, in a state of combination, but the deposit was, they say, "actually acid to the taste." Also, that three days' fog deposited per square mile of surface, in by no means the worst part of Manchester, 1½ cwt. of sulphuric acid, and even as far out of the city as the Owens College, on the same area, over 1 cwt. of acid and 13 cwt. of blacks.

There is still one other point characteristic of town fogs to be noted: it is their persistency in an atmosphere considerably above the dew point. A country fog under such circumstances directly passes away; a town fog apparently does not do so. There seem to me to be two reasons for this: one is that the moisture is protected, and its evaporation to a large extent hindered, by the presence of oily matter; and secondly, when the moisture has really gone, the soot and dust remain, and produce a haze.

The great distance to which fogs will travel is also remarkable, for they have on many occasions been traced to a distance of at least 25 to 35 miles from London, and I believe I might say to 50 miles.

I have so far discussed the production and composition of town fogs, and before considering their effects, would say a word on the question of whether in London they are increasing in frequency and density. A complete and accurate record of fogs in London is not kept; several stations are required, and a correct method of registering the density and distinguishing the difference between haze and fog is necessary; but fortunately there is a fair approximation to this complete registration of London fogs published by the Meteorological Office in their daily reports. The observations are made every morning at Brixton, and every afternoon at Victoria Street, and from a paper by Mr. Brodie, on "Some Remarkable Features in the Winter of 1890-91," published in the Journal of the Royal Meteorological Society, I learn that the number of fogs thus registered which have occurred each winter since 1870 is as follows, winter being represented by the months December, January, and February. I have divided these 20 years into four groups of 5 years each:—

Between 1870 and 1875, 93 fogs occurred.
,, 1875 and 1880, 119 ,, ,,
,, 1880 and 1885, 131 ,, ,,
,, 1885 and 1890, 156 ,, ,,

It appears, then, that during the last twenty years there has been a steady increase in the number of winter fogs. I am not aware of any data to prove whether the density of these fogs has increased, but it is probable that the increase of number of fogs largely depends upon an increase of atmospheric impurity, and the conversion of haze and mist into obvious fog; and as the great colouring matter of fogs arises from the combustion of coal, I have drawn up the following table from information which has been kindly furnished to me by Mr. G. Livesey and Mr. J. B. Scott, of the Coal Exchange. It gives the amount of coal really consumed annually in London; it does not include the coal used by the different gas companies. For the first five years, the amount given in the table is rather too high, as the quantity used by the suburban gas companies could not be ascertained and deducted. The quantities apply to what is known as the London district—an area, on an average, of 15 miles round London. The table shows an absolute increase, during the last fifteen years, of 2,000,000 tons of coal—that is, half as much again is now burnt as was burnt in 1875.

Coal consumed in London (that used by Gas Companies deducted).

Year.	Tons.	Year.	Tons.
1875 ...	4,882,233	1883 ...	5,872,310
1876 ...	4,988,280	1884 ..	5,669,281
1877 ...	4,143,909	1885 ...	6,026,063
1878 ...	4,973,147	1886 ...	6,096,732
1879 ...	5,833,891	1887 ..	6,231,956
1880 ...	5,334,823	1888 ...	6,463,498
1881 ...	5,598,281	1889 ...	6,390,850
1882 ...	5,343,974		

Supposing only 1 per cent. of sulphur in this last yearly amount is converted into sulphuric acid (H_2SO_4) and passes into the air; this would give 195,720 tons of this acid.

The five years' averages of winter fogs, we have seen, give a steady increase, but obviously the number each winter will vary much with the atmospheric conditions: for instance, last winter was remarkably favourable for the development of fog; for, again taking the last twenty years, the average number of days of fog during the winter is 25, but last winter the actual number was 50.

The general atmospheric conditions which induce fogs are a still and moist air and a high barometer—a state of the air most usual under anticyclonic conditions. The immediate determining cause, however, of a fog is usually a sudden and considerable fall of temperature. Mr. Brodie also points out that last winter was a time of calms; the percentage of such days on the average for the last twenty years is 9·7, but last winter the number was 22. Emphatically, he says, it was an anticyclonic winter.

A form of fog, well termed a "high fog," now frequently occurs in London. The lights in a street during this form of fog are often as visible as on clear nights, but above hangs a fog so dense that the darkness of night may prevail during the day. This particular form of fog appears

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.o have become much more frequent of late years, and, in fact, it is doubtful whether in former times it ever occurred. The immediate cause of this new form of fog is difficult to explain.

London has always been the head-quarters of town fogs, but now all the large towns appear to be emulating it in this respect, and this is what we must expect; an increase of population means an increase of combustion of coal, and that implies a pouring into the atmosphere of more and more carbon, hydrocarbons, and sulphuric acid. In dry and windy weather all these bodies may be scattered so as not to produce appreciable effects; but let the air be still, and even approach a state of aqueous saturation—then, we have seen, every particle of dust and dirt becomes a centre for moisture to deposit on, and we shall have a fog imprisoning all impurities and offering them to us for inhalation. To burn coal so that only products of complete combustion shall escape is a problem of much difficulty, and is comparatively rarely done. Certainly the domestic fireplace does not do it, but, on the contrary, is the principal cause of the dark colour of our fogs. Many manufacturers, however, liberally contribute to produce the same effect.

I turn now from the constitution and production of fog to note some of the effects it produces. First, with regard to health, details on this point I leave to those who are more a'le to describe them than I am, but I have a few words to say with regard to the effect of London fogs on the death-rate in general. There are many people who feel so strongly the unpleasantness of fog that it induces them to magnify its results, and make extraordinary statements with regard to the mortality it produces. It has even by some been likened in deadliness to the Great Plague of London, and to other great epidemics. To ascertain how far such views were correct, I studied the Registrar-General's reports for the times of fogs; but, as I found it difficult to interpret the figures, I have expressed them by the curves upon these somewhat lengthy diagrams (Figs. 1, 2, and 3). I have selected times of fog, viz. the winters of 1879–80, 1889–90, and 1890–91, and have represented graphically the temperature, the amount of fog, and the death-rate for each day.

The results are, I think, worthy of careful study. The first thing we learn from these diagrams is that by far the greater number of fogs occur when there is a great fall of temperature; and clearly this is closely followed after a few days by a great increase in the death-rate; but how much of this increase is to be attributed to the fog and how much to the fall in temperature may be difficult to determine; but we have evidence that when fogs occur without fall of temperature they do not appear to be followed by any remarkable increase of death-rate; for, on December 15, 1889, there was a dense fog, and the temperature was even above the average: under these conditions the death-rate remained far below the average. On December 13 and 14 in the same year, again, there is a dense fog, an average temperature, and only an average death-rate; and the same thing happens on February 4 in 1890, when, notwithstanding a dense fog, the death-rate remained remarkably low; and last winter, on November 13 and 14, there was again a dense fog, a high temperature, and an average death-rate. With these four exceptions depression of temperature goes with fog. There is no case of depression of temperature not followed by increase of death-rate.

That many people suffer much, both physically and mentally, from the effects of fog, there can be no doubt; but, as far as I can interpret these returns of the Registrar-

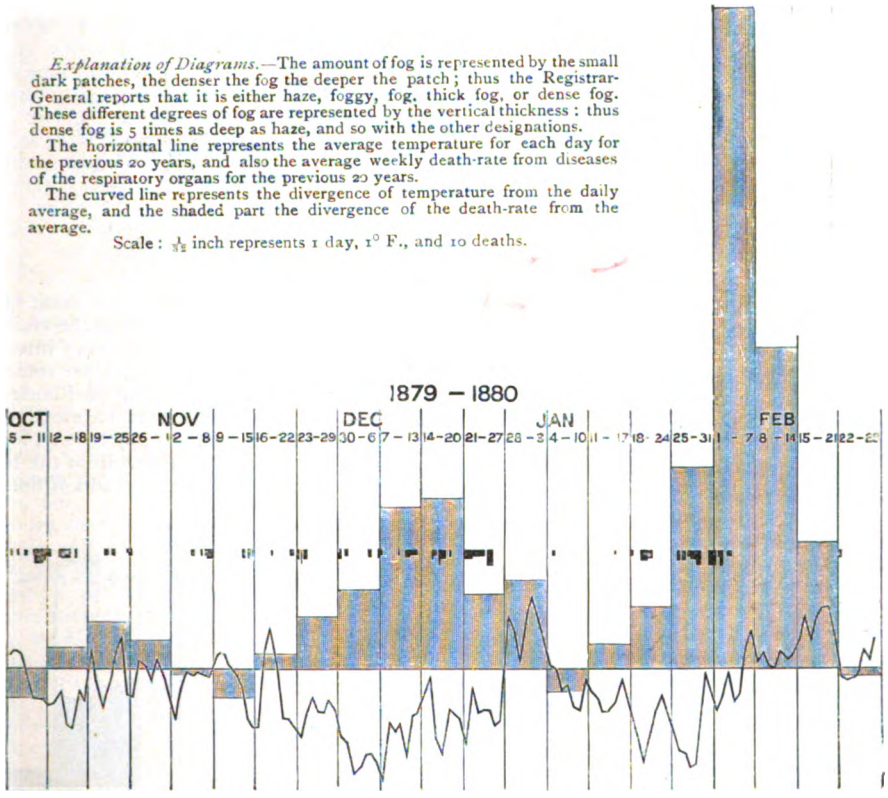
Explanation of Diagrams.—The amount of fog is represented by the small dark patches, the denser the fog the deeper the patch; thus the Registrar-General reports that it is either haze, foggy, fog, thick fog, or dense fog. These different degrees of fog are represented by the vertical thickness: thus dense fog is 5 times as deep as haze, and so with the other designations.
The horizontal line represents the average temperature for each day for the previous 20 years, and also the average weekly death-rate from diseases of the respiratory organs for the previous 20 years.
The curved line represents the divergence of temperature from the daily average, and the shaded part the divergence of the death-rate from the average.
Scale: ½ inch represents 1 day, 1° F., and 10 deaths.
1879 – 1880
OCT
NOV
DEC
JAN
FEB

FIG. 1.

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General, they do not confirm the popular impression that fog is a deadly scourge; at the same time, it is beyond doubt that an atmosphere charged with soot, dust, and empyreumatic products is an unwholesome atmosphere to breathe; but I think that the principal cause of the great increase of death when fogs occur is attributable rather to the sudden fall of temperature which usually accompanies fog, than to the fog itself.

So many toxic effects are now traced to the action, direct or indirect, of bacteria, that it is satisfactory to learn, from the experiments of Dr. Percy Frankland, that fogs do not tend to concentrate and nurture them, for he found there were remarkably few bacteria in London air during a time of fog. The deleterious action of town fogs on plants is more marked and more easy to investigate than its effect on animals. Nurserymen have long known from experience that a town fog will penetrate even their heated greenhouses, and with certainty will kill many of their plants, specially their orchids, tomatoes, and, in fact, most tender and soft-wooded plants; but on this point, I cannot do better than read to you what the Director of Kew Gardens, Prof. Thiselton Dyer, says in a letter to me:—"With regard to plants under glass, the effect of fog is of two kinds—(1) By diminishing light. This checks transpiration. The plants are therefore in the condition of being over-watered. A well-known consequence of this is to make them shed their leaves wholesale. Many valuable plants which ought to be well furnished with foliage become perfectly bare, and it is impossible ever again to recover them into sightly specimens. (2) The toxic influence of the fog. This is most striking. It is illustrated in the most forcible way by the inclosed memorandum. I attribute it in the main to sulphurous acid, though I cannot help suspecting that some hydrocarbon may also have something to do with it. The toxic effect varies from one plant to another, some are scarcely injured, others are practically killed." He adds:—"I hope you will be able to arouse some interest in this horrible plague. If the visitation of last year is annually repeated, it must in time make all refined horticulture impossible in the vicinity of London."

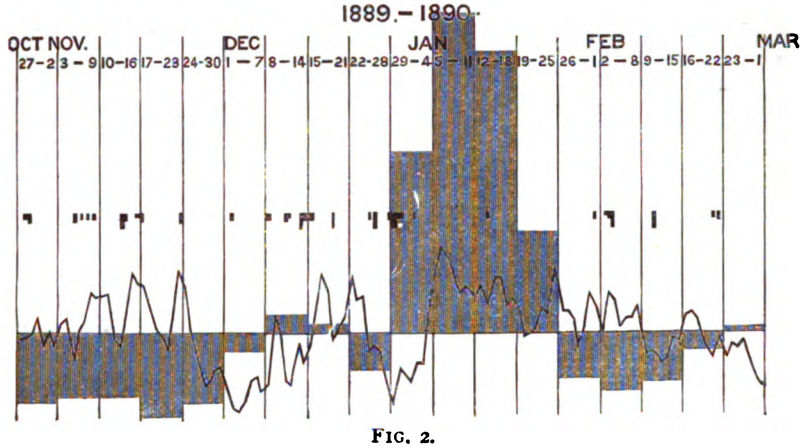

FIG. 2.

I append to this paper the very interesting and important report to which Prof. Dyer refers, from Mr. W. Watson, "On the Effect of Fog on Plants grown at Kew." This fog action on plants is so clearly marked, and so deadly, that it has, I am happy to say, led the Horticultural Society, aided by a grant from the Royal Society, to undertake a scientific investigation of the matter. Plants are so much more easily dealt with than people, all the circumstances of their attack by the fog and its immediate results so much more easily noted and traced, that the investigation has already yielded important results, and we shall, I hope, hear from Prof. Oliver—who is devoting himself specially to the investigation—some account of his latest results. A marked and admitted difference between town and country fog is, that while a country fog is harmless in a greenhouse, a town fog will produce most destructive results.

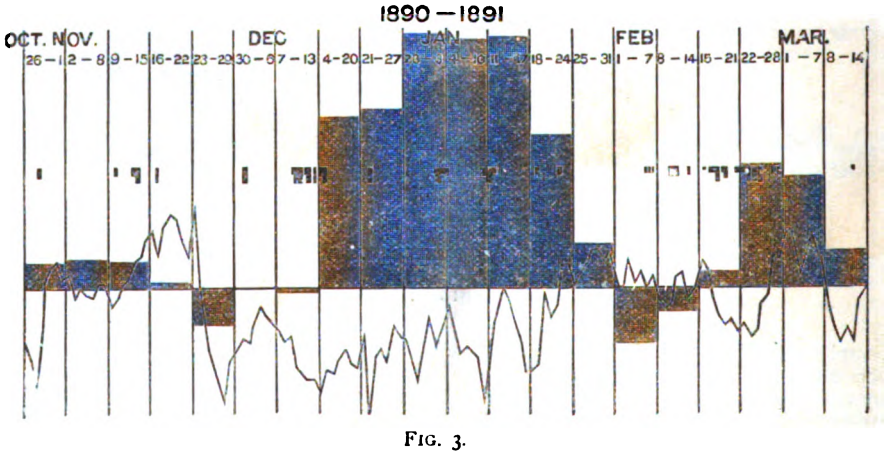

FIG. 3.

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There is still another action of town fogs, and one which I believe is of great importance. I mean its power of absorbing light. This power of abstracting light depends principally on the amount of coal products which the fog contains. The slower-vibrating red rays can struggle through a fog which is absolutely impervious to the more refrangible ones. Even a mist but slightly tinged with smoke is opaque to the blue rays, and thus screens us from their action but as Aitken has lately shown, the heat rays can pass readily through. This opacity of town fog to light is, I believe, one of its most serious and detrimental characters. Animals can no more thrive in semi-darkness than can plants; and, important as the red rays may be, still it is undoubtedly the blue rays which are most active in producing the principal chemical charges going on around us. Experiments lately made have strongly impressed me with the wonderful activity which light confers on a mixture of air and moisture, oxidations which in dullness and darkness are impossible are easily and rapidly effected by aid of a gleam of sunshine, or even a bright diffused light. It is not possible, I believe, for people to remain healthy where this source of chemical activity is cut off, or even seriously diminished. In addition to the loss of physical energy, mental depression is induced by the absence of light, the whole tone of the system becomes lowered, and may be a prey to actions which, under brighter conditions, it would have been able to resist.

There is another action of light which is potent for good. I mean its destructive action on many forms of bacteria. Prof. Koch, at the last meeting of this Congress, pointed out how his tubercle bacilli are killed by even a short exposure to sunlight, and it is now well established how inimical light is to the growth and development of most kinds of bacteria. I wish I could show you in some perspicuous way the enormous power which town fog has of absorbing light, and bring forcibly before you the great difference which exists between the amount of light which reaches the inhabitants and buildings of a town, as compared to the amount on an equal area free from smoke. A simple actinometer is much required, and I hope the want will soon be supplied; but at present the only records bearing on this point are the observations of direct sunshine made at various stations, by the Meteorological Society and Meteorological Office, with the Campbell-Stokes instrument, and some interesting observations, by Mr. H. Raffles, on the distance at which objects were visible during a London winter.

First, with regard to the sunshine experiments. One station is situated in the heart of the City, in Bunhill Row, and it is of much interest to compare the amount of sunshine there with, first, the amount in the immediate neighbourhood of London, where we are not beyond the effect of town fogs, viz. at Greenwich on one side, and Kew on the other, and also with a place not far from London, which is beyond the influence of its smoke, viz. Apsley Guise, near Woburn. I have also noted the results obtained at Eastbourne, which is about as far distant from London as Apsley Guise, but in the opposite direction, and is one of the sunniest places in England.

Hours of Sunshine during the Year 1890.

	Bunhill Row.	Greenwich.	Kew.	Aspley Guise.	Eastbourne.
January	29·9	44·0	56·0	57·3	56·9
February ...	42·4	62·8	57·8	70·5	106·5
March	71·3	90·8	109·3	110·4	133·5
April	127·4	141·5	144·8	137·3	170·1
May	215·7	223·9	223·9	214·3	267·9
June	128·0	125·2	141·4	119·1	165·3
July	134·1	120·6	139·9	141·3	185·6
August	164·0	153·1	182·5	189·5	200·2
September ...	131·6	153·2	169·5	166·1	207·4
October ...	89·6	96·9	121·6	135·6	125·3
November ...	23·4	40·8	57·6	64·7	66·9
December ...	0·1	2·4	0·3	13·4	38·0
Total ...	1157·5	1255·2	1404·6	1419·5	1723·6

Taking the totals of last year, the table shows that the hours of sunshine registered at Bunhill Row were 1158, at Greenwich 1255, at Kew 1405, at Apsley Guise 1420, and at Eastbourne 1724; but for our present purpose we must compare the amounts of sunshine at these places during the winter months—November, December, January, and February—and we find that at Bunhill Row there were 95·8, Greenwich 150, Kew 171·7, Apsley Guise 205·9, and at Eastbourne 268·3 hours of sunshine; that is, if Apsley Guise be taken as giving the normal amount, Bunhill Row received only half its due amount, and at Eastbourne there was nearly three times as much sunshine as in the City. Now, on comparing the two other periods of 4 months, which are comparatively free from fogs, the amount of sunshine is far more nearly the same at all stations.

	Bunhill Row.	Greenwich.	Kew.	Aspley Guise.	Eastbourne.
March till June	542·4	581·4	619·4	581·1	736·8
July till October	519·3	523·8	613·5	632·5	718·5

Mr. Raffles, during the winter of 1887-88, which it should be noted was remarkably free from fogs, made a series of observations of the distances to which he could see from Primrose Hill, and found that looking south on the 152 consecutive days from November to March, only on 78 days could he see a quarter of a mile, and only on 83 days could he see to the same distance in a south-westerly direction: this conveys a good idea of the opacity of our London atmosphere.

We attempt to compensate for the darkness which fogs cause by the use of artificial light, and I have again to thank my friend Mr. Livesey for the information he has given me with regard to the extra quantity of gas burnt in London during a day of fog. He tells me that if a dense fog covered the whole of London, and lasted all day, the additional amount of gas consumed would be 30 million cubic feet; but since so extensive a fog as this probably never exists, and certainly never lasts all day, the actual amount consumed may be correctly reckoned at 25 million cubic feet; and if the cost of this be calculated at 2*s.* 6*d.* per 1000 cubic feet, which is rather below than above the actual cost, it amounts to £3125; but after all, it is not the single days of dense fog that measure the extra amount and cost of artificial light used on account of fog—it is rather the continually occurring dull days and local transitory fogs which demand an extra supply of gas, and this is often 5 to 15 million cubic feet in a day, and gives a total by the end of the winter which is very considerable. As a standard of comparison, I should state that the total consumption of gas in the London district in a day of 24 hours, during the depth of winter, is 140 million cubic feet.

Such, then, is an imperfect outline of the chief features and effects of town fogs; and now what is to be said with regard to the possibility of getting rid of such fogs? This question, it seems to me, resolves itself into this: fogs cannot be prevented from forming over towns; there are, and probably ever will be, special inducements, in the way of dust particles and products of combustion, for fogs to form there; but whether they must always be dark in

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colour, and loaded with soot and tarry matter, is another question. The answer involves not only chemical but also social considerations. With regard to the first, my answer is that as long as coal is burnt you will have dense fogs; grates, kitcheners, furnaces, may be, and probably will be, much improved, and fires may be stoked in a better way, but that the improvements will be so great that all imperfect combustion will cease I think is improbable; if this be so, there is only one other alternative, as long as coal is our source of heat: it is to alter our form of fuel and adopt gas and coke; the soot and tarry matters will be then done away with; the question of sulphuric acid in the air would remain, but our fogs would at least be white. There is still the social part of the question, which is not without serious difficulty—namely, how to induce or compel people to give up the use of coal. At the present day it would not be possible to do as it is recorded was done in the reign of Edward I., try, condemn, and execute a man for burning coal in the City of London. W. J. RUSSELL.

Effects of Fog on Plants Grown in the Houses at Kew.

The heavy fogs experienced in the last two or three winters injured many plants in the houses at Kew. When thick fog occurred almost daily, the injury it did to many plants amounted practically to destruction. The leaves fell off, the growing point withered, and in some cases, such as Begonias and Acanthads, the stems also were affected. Flowers, as a rule, fell off as soon as they opened, or whilst in bud. Almost all flowers which expanded were less in size than when there was no fog. The flower-buds of Phalænopsis, Angræcum, some Begonias, Camellias, &c., changed colour and fell off as if they had been dipped in hot water.

In the Palm-house bushels of healthy-looking leaves, which had fallen from the plants, were gathered almost every morning. Plants which appeared to be perfectly healthy, when shaken would drop almost every leaf. Herbaceous plants suffered most, *i.e.* Begonias, Poinsettias, Bouvardias, Acanthads, &c. Some herbaceous plants, however, did not suffer at all, nor were their flowers injured, as, for instance, Cyclamen, Primula, Hyacinth, &c. Many hard-wooded plants lost their leaves and were otherwise damaged, viz. Boronias, some Heaths, Grevilleas, Acacias, &c. *Protea cynaroides*, a Cape plant with large laurel-like leaves, was much injured in the temperate house (minimum temperature 40°), the leaves turning black as though scalded. The same species, however, in another house where the atmosphere is drier and the temperature a few degrees higher, was scarcely affected by fog.

As a rule, the plants that were in active growth suffered most. Monocotyledonous plants and ferns for the most part were not appreciably affected by the fogs, the injury they suffered, especially last winter, being clearly due to low temperature. The effect of fog on flowers is remarkable. Generally, white flowers are destroyed, but there are some notable exceptions—viz. *Masdevalia tovarensis*, *Odontoglossum crispum*, and Angræcum amongst Orchids, and Crinums, white Cyclamen, white Hyacinths, white Chrysanthemums, &c.

The green leaves of *Poinsettia pulcherrima* all fell off, whilst the red ones (bracts) remained, as also did the flowers. All Calanthes, of whatever colour, lost their flowers. The buds of the white-flowered *Angræcum sesquipedale* turned black as if boiled, whilst those of *A. eburneum*, also white-flowered, were not injured, and developed properly. These two plants are grown in the same house under identical conditions, and they come into bloom about the same time.

The conditions most conducive to rest from growth—viz. a low temperature and moderately dry atmosphere, together with diminished light, unavoidable during the prevalency of fog—were proved at Kew to be the safest for all plants during the prevalence of heavy fogs.

July 29. W. WATSON.

THE ANATOMY OF THE DOG.[1]

THE dog has played by far the most important part in the elucidation of the difficult problems of physiology and pathology presented by the higher animal organism. It is by a firm reliance on the results of experimental researches, conducted largely upon this animal, that the modern physician is enabled to form some idea as to the causation of the symptoms of disease in man, and the mode of action of the remedies which he employs; while the modern surgeon, after a preliminary testing of an operation upon the dog, fearlessly proceeds to attack the most deeply-seated tumour, and to explore the most hidden recesses of the human organization. What, after all, are the services of friendship and companionship, or the more menial duties which are often laid upon the dog, compared with the alleviation of human suffering and the advancement of human knowledge for which he has served as the passive instrument, and this (*pace* the mendacious asseverations of fanatical essayists) at the expense of the least possible amount of suffering to himself?

For these reasons, to the physiologist, the pathologist, the pharmacologist, and the scientific surgeon, a book which, like the one before us, endeavours to deal with the anatomy of the dog in the same detailed and systematic manner in which the structure of man is dealt with in text-books of human anatomy cannot fail to be of the utmost value. To the comparative anatomist it will prove an important addition to the limited existing series of monographs dealing in detail with vertebrate types, while to the veterinarian it will be an indispensable *vade mecum*, both in study and in practice.

For the work is done excellently well, a result which might be anticipated from the manner in which it has been set about. Not only has it been carried on under the auspices of a scientific anatomist so well known as Prof. Ellenberger and in a veterinary school where an unlimited supply of subjects was available for dissection, but with a far-sighted liberality, for which the Saxon Government is much to be congratulated, all the expenses for material and instruments have been defrayed by the State, and one of the collaborators has been enabled to devote his whole time during a period of two years entirely to the labour incident upon the preparation of this work.

The book is a large octavo of 650 pages, containing 208 woodcuts, a few examples of which are here reproduced. There is, in addition, an appendix of 37 lithographed plates, representing in outline frozen sections through the trunk and limbs. A study of these is in itself sufficient to make out the relations of the organs to one another, and the authors have accordingly burdened the text as little as possible with topographical details. Histological and developmental references are entirely avoided, partly for the reason that the facts are not materially different from those which are found in other mammals, partly because they have been dealt with, especially for the dog, in other works, and largely because it was obviously desirable not to increase the bulk of the work. References to literature are also for the most part omitted, for although other works have been consulted, it is claimed by the authors that the present account is

[1] "Systematische u. topographische Anatomie des Hundes." Bearbeitet von Dr. W. Ellenberger, Professor an der tierärztlichen Hochschule in Dresden, und Dr. H. Baum, Prosektor an der tierärztlichen Hochschule in Dresden. (Berlin: Paul Parey, 189

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based almost exclusively upon original dissections and preparations.

It might be supposed that the striking differences, both in size and in shape, which are presented by dogs

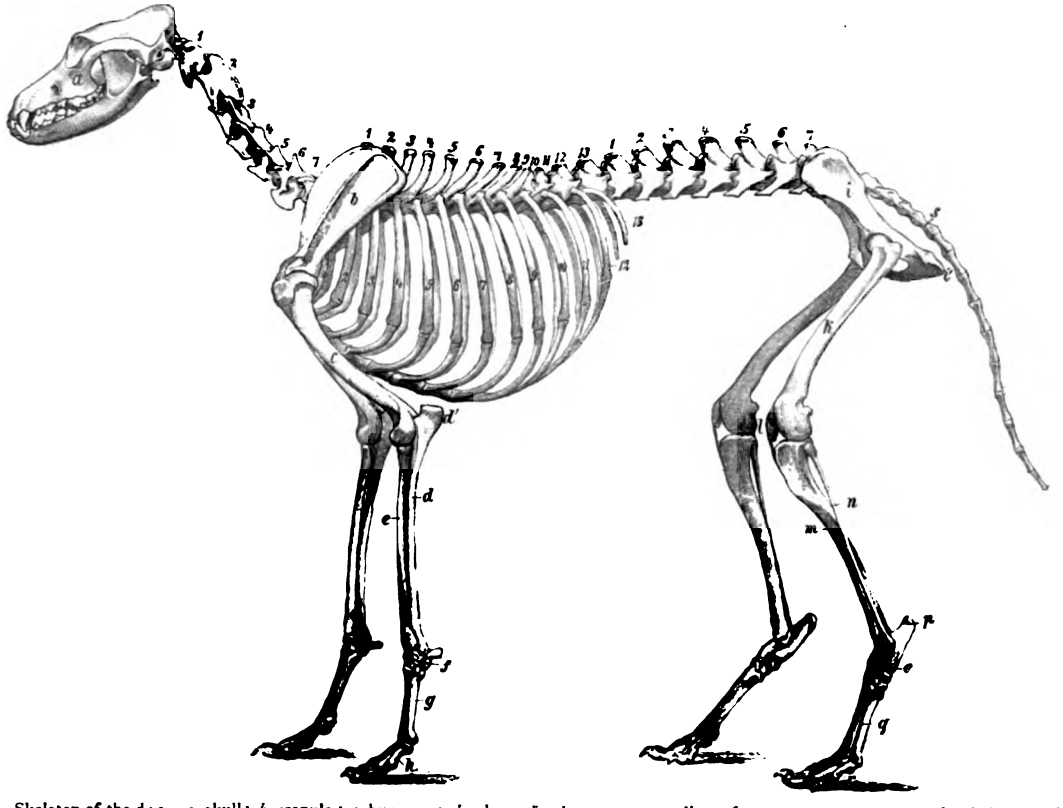

FIG. 1.—Skeleton of the dog. *a*, skull; *b*, scapula; *c*, humerus; *d*, ulna; *d'*, olecranon; *e*, radius; *f*, carpus; *g*, metacarpus; *h*, phalanges of fore-foot; *i*, pelvis; *i'*, tuber ischii; *k*, femur; *m*, tibia; *n*, fibula; *o*, tarsus; *p*, tuber calcanei; *q*, metatarsus; *r*, phalanges of hind-foot; *s*, coccygeal vertebræ. The cervical, thoracic, and lumbar vertebræ and the ribs are respectively numbered consecutively.

of races so different from one another as, to take extreme cases, the greyhound and the pug, would be accompanied by such structural peculiarities as to render a general anatomical account of the dog of less value than that of animals in which racial characteristics are less exaggerated. The differences which are found, and the difficulties which result therefrom, are, as might be supposed, not ignored by the authors of this book. But they remark hereon that apart from differences in size, rendering absolute measurements of little value, the racial differences are almost entirely confined to the skeleton and to certain parts of the muscular system, no important differences being manifest in the position of the muscles, vessels, nerves, and viscera; and even in crook-legged dogs, such as the dachshund, in spite of the twisting of the extremities, the topographical relations of the muscles

FIG. 2.—Fore-foot of the dog. *a*, carpal ball; *b*, ball of the sole: c_1 to c_5, balls of the toes.

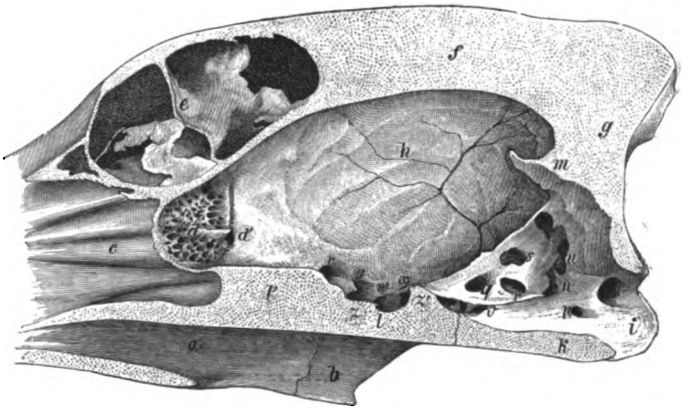

FIG. 3.—Section of skull, displaying the interior of the cranial cavity, and the frontal sinuses. The foramina of exit of the cranial nerves, and the impressions of the cerebral convolutions on the inner surface of the cranium are well shown.

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to one another, as well as to the vessels, nerves, and bones, remain completely unaltered. The only racial characteristics, therefore, which are dwelt upon are those of the skeleton, and especially of the skull, in illustration of which the authors reproduce some of the excellent figures of Nathusius.

Classifying them with regard to their racial peculiarities, the skulls of dogs are divided into two large groups, viz. (1) *Dolichocephalic*, to which belong such dogs as the greyhound, collie, poodle, St. Bernard, and Newfoundland; and (2) *Brachycephalic*, including, amongst others, the pug and bulldog. These groups, however, do not include all dogs, some varieties being intermediate. The difference depends upon the relative development of the face as compared with the brain-capsule, for in the dolichocephalic the face is about two-thirds as long as the brain-capsule, while in the brachycephalic it is only about one-third as long. The former have a strongly marked, bony sagittal crest and a narrow brain-capsule; in the latter the sagittal crest is absent, and the brain-capsule wide. The jaws and dental arches are straight and extended in the dolichocephalic; short and rapidly con-

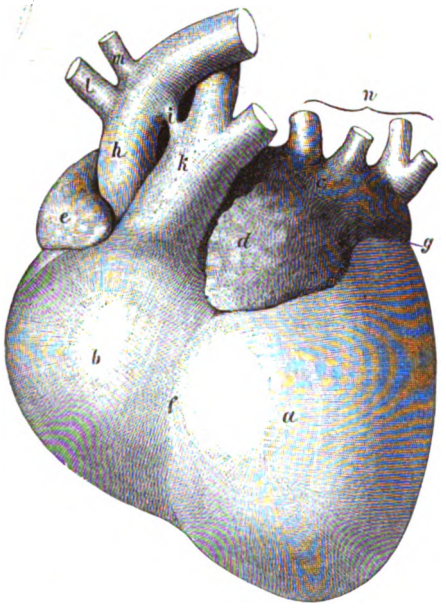

FIG. 4.—Dog's heart, viewed from the left side. *a*, left ventricle; *b*, right ventricle; *c*, left auricle; *d*, its auricular appendage; *e*, right auricle; *f*, groove between right and left ventricles; *g*, coronary groove; *h*, aorta; *i*, ligamentum Botalli; *k*, pulmonary artery; *l*, innominate artery; *m*, left subclavian; *n*, pulmonary veins.

verging in the brachycephalic; in the former the premolars are set straight, with well-marked intervals; in the latter they are closely packed, and set obliquely. The racial peculiarities of all the several bones of the skeleton are referred to, and a comprehensive table of pelvic measurements of the different races is given.

It would carry us too far to draw attention to all the details of a work like this, but there are certain points which deserve special mention. Amongst these may be enumerated the exact manner in which each individual bone is described and illustrated; the descriptions of the teeth, short but sufficient, including their dates of eruption; the account of the individual muscles and groups of muscles, with their action; the descriptions of the viscera and of the vascular and nervous systems; and last, but not least, the general excellence of the illustrations, in which the muscles, the blood-vessels, and the nerves are shown up by the aid of colours and differences of shading in a manner which gives a diagrammatic clearness to what appear to be drawings made from actual dissections. Special mention may also be made of the section devoted to the cerebral hemispheres, the convolutions and fissures of which are minutely described and illustrated both by diagrams and artistic representations. The diagrams which are used to elucidate the distribution of the vagus and sympathetic nerves are a model of clearness; a reference to recent observations on the distribution of white and grey fibres in these and other nerves would, however, have added much to the physiological value of this section. It is also to be remarked that the sense-organs are somewhat lightly touched upon; but in the case of the eye and its connections with the brain, the student is enabled to supplement the account given by the authors by a bibliography of the subject extending over the last twenty-five years.

A table showing the arterial and nervous supply of all the organs of the body, including each muscle and the several parts of the skin, occupies about twenty pages at the end of the book, and will add greatly to its value. An excellent index must also be mentioned, especially as an index is often conspicuous by its absence in German scientific works. In its printing and general get up the book is worthy of the pains which have been bestowed upon it by its authors and of the distinguished physiologist, Prof. Carl Ludwig, to whom they have inscribed a dedication. It is to be hoped that we may soon be able to welcome this work in an English form.

NOTES.

A COMMITTEE has been formed at Cambridge to raise a fund to obtain a portrait of Prof. Michael Foster. The portrait will be presented either to the University or to Trinity College, as the subscribers may decide. Among the members of the committee are the Vice-Chancellor, the Provost of King's, the Masters of Trinity, Jesus, and Downing Colleges, Sir George Stokes, M.P., Sir George Paget, Sir George Humphry, Prof. Jebb, M.P., Prof. Darwin, Prof. Newton, Prof. Roy, Prof. Stanford, Prof. Stanton, and Prof. Thomson. Dr. Lea, of Gonville and Caius College, is the treasurer of the fund.

THE celebration of Prof. von Helmholtz's seventieth birthday, deferred from August 31, was held on Monday last at Berlin. He was congratulated in the warmest terms by the Minister of Education, and by representatives of many scientific Societies. Prof. du Bois Reymond, acting on behalf of the Helmholtz Medal Committee, handed to Dr. von Helmholtz the first medal, and said that numerous contributions to the Helmholtz Fund had flowed in from all parts of the world, and that the Berlin Academy of Science, with the Emperor's permission, had undertaken the trusteeship. In the evening over 500 guests attended a banquet at the Kaiserhof Hotel.

WE regret to have to record the death of Dr. H. K. H. Hoffmann, one of the most distinguished German botanists. He died on October 27. He had been for many years Professor of Botany at Giessen and Director of the Botanic Institution there. Prof. Hoffmann was in his seventy-third year.

WITH reference to the article on "Existing Schools of Science and Art" in NATURE of October 8 (vol. xliv. p. 547), Mr. O. S. Dawson writes:—"It was stated at the meeting that the St. Martin's School of Art 'had closed its doors.' I find this to be incorrect. Certain changes have been made, but I am glad to be able to state that this school (one of the oldest and best known in the country) is flourishing under the new head-master, Mr. Allen."

THE interest excited by the question of the compulsory study of Greek brought to Cambridge on Thursday, last week, the largest number of members of the Senate ever gathered in the Senate House. The proposal that the question should be made a subject of official inquiry was rejected by 525 votes against 185.

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THE Museums and Lecture Rooms Syndicate, Cambridge, have accepted on behalf of the University a cast of the model executed by the late Sir J. E. Boehm, R.A., for his statue of Mr. Charles Darwin. The cast has been presented by Mr. Darwin's family, and is now placed in the lecture-room of comparative anatomy.

A SOCIETY for the encouragement of the study of natural science has recently been formed at the University of Edinburgh. In commemoration of the fact that Darwin was once a student of the University and a member of a similar society, it has been named the Darwinian Society. The inaugural address is to be delivered by the President, Prof. J. Cossar Ewart. Mr. J. Graham Kerr (late naturalist to the Pilcomayo Expedition) is chairman.

THE anniversary meeting of the Mineralogical Society will be held on Tuesday, November 10, at 8 o'clock. After the election of officers and Council, the following papers will be read:—analysis of aragonite from Scotland, by J. Stuart Thomson; on minerals from the apatite mines near Risör, Norway, by R. H. Solly; notes on the minerals from the hematite depo its of West Cumberland, by the same; mineralogical notes from Torreon, Chihuahua, by Henry F. Collins; on the pinite of Breage in Cornwall, by J. H. Collins; on the occurrence of danalite, by H. A. Miers and G. T. Prior.

ON Tuesday, Dr. Burdon Sanderson delivered the first of the Croonian Lectures before the College of Physicians in the new lecture-room at the Examination Hall. The remaining lectures will be given on the next three Tuesdays of November. The subjects are the etiology of inflammation and of the acute specific diseases, and natural and acquired immunity.

SIR DOUGLAS GALTON, F.R.S., has been asked to investigate and report upon the sanitary state of Florence. He is to make any recommendations and suggestions that he may deem necessary.

AT a meeting of the Senate of the University of Sydney on September 21, it was resolved that Prof. Thorpe and Prof. Ramsay should be asked to select and appoint a Demonstrator of Chemistry to take office at the Sydney University on March 1 next, the salary to be at the rate of £350 per annum, and £60 to be allowed for passage money, such sum to be refunded if the Demonstrator should resign his office before the expiration of two years from his appointment. The appointment of a new Demonstrator has been rendered necessary by the resignation of Mr. F. B. Guthrie, who has been made Analyst to the Department of Agriculture.

THE Society of Arts has completed its arrangements for the approaching session. The first meeting will be held on Wednesday, November 18, when the opening address will be delivered by the Attorney-General, Chairman of the Council. At subsequent ordinary meetings (four of which, in addition to the opening meeting, will be held before Christmas) the following lectures will be delivered:—Measurement of lenses, by Prof. Silvanus P. Thompson, F.R.S.; secondary batteries, by G. H. Robertson; the World's Fair at Chicago, 1893, by James Dredge; spontaneous ignition of coal, and its prevention, by Prof. Vivian B. Lewes; burning oils for lighthouses and lightships, by E. Price Edwards; dust, and how to shut it out, by T. Pridgin Teale; typological museums, by General Pitt Rivers; Iceland, by T. Anderson; artistic treatment of jewellery and personal ornament, by J. W. Tonks; agricultural banks for India, by Sir William Wedderburn. The following Cantor Lectures will be given on Monday evenings:—The pigments and vehicles of the old masters, by A. F. Laurie (three lectures, November 30, December 7, 14); developments of electrical distribution, by Prof. George Forbes, F.R.S. (four lectures, January 25, February 1, 8, 15); the uses of petroleum in prime movers, by Prof. William Robinson (four lectures, February 29, March 7, 14, 21); mine surveying, by Bennett H. Brough (three lectures, March 28, April 4, 11); recent contributions to the chemistry and bacteriology of the fermentation industries, by Dr. Percy Frankland (four lectures, May 2, 9, 16, 23). A special course of six lectures, under the Howard Bequest, will be delivered on Friday evenings:—The development and transmission of power from central stations, by Prof. W. Cawthorne Unwin, F.R.S. (February 5, 12, 19, 26, March 4, 11).

LAST week the Speaker of the House of Commons, responding to a toast at the annual Mayoral banquet at Warwick, gave some sensible advice about technical education. He was afraid, he said, that there was great danger of the sums granted for the promotion of technical education being frittered away. What they wanted to teach was not a trade, not the particular manipulation of the article students might have to deal with in after life, but the principles of science as applicable to the art. Their object should be to elevate the students above the mere manual dexterity of the special professions to which they were to belong.

ON November 12, Mr. E. J. Humphery will read a paper before the Camera Club on a new method of photography by artificial light. According to the Journal of the Camera Club, Mr. Humphery promises a process of considerable novelty and value in practical work.

DR. ELISHA GRAY lately read before the Chicago Electric Club a paper in which he urged the importance of the International Congress of Electricians which is to be held in connection with the World's Fair at Chicago in 1893. The Congress, he thinks, should be divided into sections according to the various interests represented, one section being devoted to the purely scientific aspects of the subject. "Success," he said, "will be assured from the beginning if all our interested friends act harmoniously, and are actuated by one common desire that the best thing shall be done, without regard to geographical boundaries or local prejudices." Commenting on the paper, Mr. Parker pointed out that, owing to the supremacy which America enjoys in the practical development of industrial electricity, the electrical department would be the most interesting and attractive feature of the Exhibition. He held, therefore, that the directors of the Exhibition should give priority to this department in all arrangements, and should do all in their power to render the Electrical Congress a successful gathering.

PROF. WARD, the mineralogist, of Rochester, New York, has offered to send his collection of geological specimens to the Chicago Exhibition. It is said to be one of the most valuable collections in the United States.

ON Wednesday, October 28, a terrible earthquake visited Nipon, the island which forms the larger part of the Japanese Empire. The area over which the shocks were felt was wider than was at first supposed. It extended inland to the region of the lakes. The principal shock lasted less than two minutes, but was of extreme violence. The subsequent shocks were not strong enough to have done damage in ordinary circumstances, but they sufficed to shake down walls already cracked, and added immensely to the terrors of the night. The *Times* correspondent, telegraphing from Hiogo on November 2, says that great fissures had appeared in the ground at many points, rendering roads impassable and travelling dangerous; and that there had been a remarkable subsidence of the land to some depth over large tracts of country. The volcanic mountain Nakusan belched forth enormous masses of stones and continuous streams of sand and mud, and the contour of the mountain has been completely changed by the eruption. The greatest havoc

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seems to have been caused at Ogaki, where at least 1000 persons were killed, chiefly by falling buildings. Both there and at Gifu the earthquake was followed by fires, in which many perished. At Kitagata, Ichinomiya, Tiraguna, Kiyonsu, Kamatsu, and other places, chiefly along the coast, great damage was done. The city of Nagoya suffered to a less extent, although seriously. Much distress prevails in the ruined towns, and the Government is embarrassed in its efforts by the prevailing panic, and the absence of means of communication, telegraph lines and many miles of railway having been destroyed. Exact details as to the extent of the calamity will probably not be obtained for some time. On November 2 the following was the official estimate: killed, 4000 persons; injured, fully 5000; houses destroyed, 50,000.

A GREAT rush of migratory birds seems to have passed over Dublin during the night of May 4 last, evidently on the way to their northern breeding-haunts. An account of the matter is given by Mr. Allan Ellison in the new number of the *Zoologist*. "While sitting in our rooms in Trinity College, about 11 p.m.," he says, "we were attracted by the loud call-notes of birds passing overhead. The night was calm and cloudy, not very dark. We listened at the open window until about 1 a.m., when they seemed to be still passing over in undiminished numbers. They were mostly golden plovers and dunlins, easily recognized by their notes, but we frequently heard the cry of the whimbrel, or the shrill call of the common sandpiper. It was most curious to hear these notes, at first far away towards the south-west, gradually becoming louder as the flocks drew nearer and passed overhead, and then rapidly passing away to the northward. Sometimes the whole air seemed full of their clear whistling notes: in one direction the loud, short pipe of the golden plover, in another the shrill wheezing cry of the dunlin, reminding one of the sound made by a whistle with a pea in it. Sometimes a bird or two would fly quite close over the house-tops, uttering its loud whistle close to the open window, but they seemed for the most part to fly at a great height."

ONE large meteorite and two fragments were lately received by the Government Central Museum, Madras, through the Board of Revenue. Mr. Edward Thurston, the Superintendent of the Museum, quotes in his report for 1890–91 the following statement, by the Tahsildar of Tirupatúr, in the Salem district, as to the conditions under which these stones fell:—"On June 4, 1890, about 8 a.m., there was a sudden clap of thunder, accompanied by an unusual rumbling noise. At this time two stones are said to have fallen in the village of Kakangarai. The fall of both the stones occurred at the same time in adjacent fields, and was witnessed by rayats, who were ploughing close by at the time. One stone appears to have been broken up and divided among the rayats, while the other was taken charge of by the village munsif. The large specimen weighs 11¼ ounces, and the fragments weigh about 1 ounce and ¼ ounce respectively."

THE sponge trade of the Bahama Islands forms the subject of an excellent report by the U.S. Consul at Nassau. The number of persons engaged in this industry in the Bahamas is from 5000 to 6000, all of whom, except the shipowners, brokers, and skippers are coloured people. The sponges are gathered by means of iron hooks attached to long poles. By using a water-glass the fisherman can readily discover the sponges at the bottom, and then with his pole and hook he will bring up those he may select as fit for his purpose, leaving the smaller ones untouched. Some sponges adhere firmly to the bed of the sea, while others—known as "rollers"—are not attached at all. About ten years ago an attempt was made to introduce dredges, but it seemed likely that they would ruin the beds, and a law was passed forbidding their use. The vessels are provided and fitted out, as a rule, for a voyage of about six weeks, and generally from six to eight voyages are made in the year. It is difficult to estimate the average catch per trip, as the cargoes vary greatly in size and value. Of the larger sponges a catch of 5000, or of the smaller ones 7500, would be considered a fair lot. Occasionally a cargo of from 12,000 to 15,000 large sponges has been brought in, but this success is exceptional. Contradictory statements are made as to the time taken by sponges to grow to the size at which they are wanted. It seems probable, however, that under ordinary conditions a healthy sponge will reach a marketable size in from twelve to eighteen months.

EXTENSIVE excavations of the prehistoric mounds in Ohio and Indiana have lately been carried on under the supervision of Prof. Putnam. In one mound, near Anderson Station, Indiana, 7232 flint spear-heads and knives have been discovered. They were found in a layer one foot thick, extending over a space of twenty by thirty feet. They are made of grey flint found only in Indiana. The largest find of flint implements previously made in America did not include more than 1800 specimens.

STATISTICS published by the French Ministry of Public Instruction show that there are in France 525 learned Societies, of which 135 have been officially recognized as of national importance. Of these 525 Societies, 95 are historical and social; 95 agricultural and horticultural; 57 medical and pharmaceutical; 45 scientific; 41 artistic; 37 geographical; and the rest miscellaneous, including photographic, statistical, and ballooning associations.

PROF. KIKUCHI, of Tokyo, whose Japanese treatise on geometry we noticed briefly a year or two since, has now published a translation of his work into English. In the first Japanese Parliament Prof. Kikuchi had the honour to be made a life member of the House of Peers by the Emperor ("this does not constitute peerage as in England"), and at the request of the Department of Agriculture and Commerce he was one of the original framers of the Weights and Measures Bill.

THE Cambridge University Press has published a second edition of Mr. S. L. Loney's "Treatise on Elementary Dynamics." The book is intended for beginners, the author having dealt only with those parts of dynamics which can be treated without the use of the infinitesimal calculus. In the present edition the work has been carefully revised and somewhat enlarged.

THE first part has now been issued of the *Zeitschrift für Pflanzenkrankheiten*, edited by Dr. Paul Sorauer, with the assistance of an "International Phytopathological Committee." The journal is intended to be published bi-monthly, at a subscription of 15 marks per annum; and will contain original articles, reviews, and news, extending over the whole subject of the diseases of plants and the remedies for these diseases.

MESSRS. CASSELL AND CO. have issued Part 37 of their "New Popular Educator." Besides many illustrations in the text, there is a coloured plate representing sea-jellies and sea-stars.

FREE hydroxylamine, NH_2OH, has been isolated by M. Lobry de Bruyn, and a preliminary account of its mode of preparation and properties is published by him in the current number of the *Recueil des travaux chimiques des Pays-Bas* (1891, 10, 101). The manner in which the free base was obtained was briefly as follows. About a hundred grams of hydroxylamine hydrochloride, $NH_2OH.HCl$, were dissolved in six hundred cubic centimetres of warm methyl alcohol. To this solution a quantity of sodium dissolved in methyl alcohol was added, in such proportion that the hydrochloride of hydroxylamine was present in slight excess over and above that required to convert it to sodium chloride. After deposition of the separated sodium

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chloride the solution was decanted and filtered. The greater portion of the methyl alcohol was next removed by distillation under the reduced pressure of 160–200 mm. The remainder was then treated with anhydrous ether, in order to completely precipitate the last traces of dissolved sodium chloride. The liquid eventually separated into two layers, an upper ethereal layer containing about 5 per cent. of hydroxylamine, and a lower layer containing over 50 per cent. of hydroxylamine, the remainder of the methyl alcohol, and a little dissolved salt. By subjecting this lower layer to fractional distillation under 60 mm. pressure, it was separated into three fractions, of which the first contained 27 per cent. of hydroxylamine, the second 60 per cent., and the third crystallized in the ice-cooled receiver in long needles. This third fraction consisted of free solid NH_2OH. Hydroxylamine as thus isolated in the free state is a very hygroscopic substance, which rapidly liquefies when exposed to air, owing to the absorption of water. The crystals melt at 33°, and the fused substance appears to possess the capability of readily dissolving metallic salts. Sodium chloride is very largely soluble in the liquid; powdered nitre melts at once in contact with it, and the two liquids then mix. Free hydroxylamine is without odour. It is heavier than water. When rapidly heated upon platinum foil it suddenly decomposes in a most violent manner, with production of a large sheet of bright-yellow flame. It is only very slightly soluble in liquid carbon compounds such as chloroform, benzene, ether, acetic ether, and carbon bisulphide. The vapour attacks corks, so that the solid requires to be preserved in glass-stoppered bottles. The free base appears also to act upon cellulose, for, upon placing a few drops of the melted substance upon filter paper, a considerable amount of heat is evolved. The pure crystals are very stable, the base in the free state appearing to possess much greater stability than when dissolved in water. The instability of the solution appears, however, to be influenced to a considerable extent by the alkalinity of the glass of the containing vessel, for concentrated solutions free from dissolved alkali are found to be perfectly stable. Bromine and iodine react in a remarkable manner with free hydroxylamine. Crystals of iodine dissolve instantly in contact with it, with evolution of a gas and considerable rise of temperature. Bromine reacts with violence, a gas again being explosively evolved and hydrobromic acid formed. The nature of the gas evolved is now undergoing investigation. A letter from M. Lobry de Bruyn appears in the number of the *Chemiker Zeitung* for October 31, warning those who may attempt to prepare free hydroxylamine by the above method that it is a dangerously explosive substance when warmed to a temperature of 80°–100°. Upon warming a flask containing the free solid base upon a water-bath a most violent explosion occurs. A spontaneous decomposition appears to set in about 80°, and even in open vessels the explosion is very violent. Care must also be taken during the fractional distillation of the concentrated solution in methyl alcohol to cool the apparatus before changing the receiver, as if air is admitted while the retort is heated the experiment ends with an explosion.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus* ♀), two Macaque Monkeys (*Macacus cynomolgus* ♂ ♀), a Bonnet Monkey (*Macacus sinicus* ♀) from India, two Roseate Cockatoos (*Cacatua roseicapilla*), a Greater Sulphur-crested Cockatoo (*Cacatua galerita*), two Cockateels (*Calopsitta novæ-hollandiæ*) from Australia, presented by the Rev. Sidney Vatcher; two Rhesus Monkeys (*Macacus rhesus* ♂ ♂) from India, presented by Mr. John H. Taylor; a Macaque Monkey *Macacus cynomolgus* ♂) from India, presented by Mr. K. A. Williams; a Yak (*Poëphagus grunniens* ♂) from Tibet, presented by Mr. M. E. C. Ingram; a Corn Crake (*Crex pratensis*), British, presented by Mr. E. Hart, F.Z.S.; two Woodcocks (*Scolopax rusticola*), British, presented respectively by Mr. Hamon Le Strange, F.Z.S., and Mr. William Bellamy; two Water Vipers (*Cenchris piscivorus*), a Water Rattlesnake (*Crotalus adamanteus*) from Florida, presented by the Natural History Society of Toronto; a Small-scaled Mastigure (*Uromastix microlepis*) from Persia, presented by Mrs. Howell; an Alligator (*Alligator mississippiensis*) from the Mississippi, presented by Mr. W. Chattaway; two Bearded Vultures (*Gypaëtus barbatus*), European, deposited; a Molucca Deer (*Cervus moluccensis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE TELLURIC SPECTRUM.—Dr. Müller has made some observations of the telluric spectrum on the summit of the Säntis, and his results are given in a recent publication of Potsdam Observatory (vol. viii., No. 27). The observing station was situated at a height of 2500 metres above sea-level. It was found that when the sun had a mean altitude, about 40 per cent. of the lines due to the water vapour in the atmosphere were quite invisible, and the remainder were very weak. Whilst the zenith distance of the sun was less than 60°, the appearance of the spectrum remained unchanged. At greater zenith distances the weak lines increased in intensity and the missing ones gradually appeared. Measurements of the intensities of single lines observed on different days and at different zenith distances indicate a variation roughly proportional to the thickness of atmosphere traversed. The aspect of the portion of spectrum observed was on the whole strikingly similar to that seen when observations were made near sea-level on dry and cold days. This is in agreement with the fact that the vapour pressure on mountains in summer is approximately equal to that on the plains in winter. Careful estimations of the intensities of the atmospheric lines at C and D might therefore be utilized to determine the decrease of the amount of vapour present in the air at different elevations. With regard to other atmospheric lines, Dr. Müller observed changes in the α-group. The whole of the lines of this group, however, were easily seen at the mountain station when the sun had a high altitude, and the difference of intensity there and in the plains was considerably less than in the groups C and D. Two maps are given illustrating the appearance of the lines in the neighbourhood of C and D for different zenith distances of the sun.

TEMPEL-SWIFT'S PERIODIC COMET.—The following ephemeris is given by M. Bossert in *Astronomische Nachrichten*, No. 3063:—

Ephemeris for Paris Midnight.

1891.	Right Ascension.	Declination.	Brightness.
	h. m. s.	° ′	
Nov. 3	21 31 22	+ 7 49·6	10·1
,, 5	21 37 6	8 37·5	
,, 7	21 43 17	9 27·7	
,, 9	21 49 59	10 19·3	
,, 11	21 57 13	11 15·1	11·9
,, 13	22 4 59	12 12·1	
,, 15	22 13 19	13 11·2	
,, 17	22 22 14	14 12·0	
,, 19	22 31 45	15 14·4	13·5
,, 21	22 41 54	16 18·0	
,, 23	22 52 41	17 22·2	
,, 25	23 4 6	18 26·6	
,, 27	23 16 9	19 30·5	14·4
,, 29	23 28 48	20 33·2	
Dec. 1	23 42 1	21 33·9	
,, 3	23 55 46	22 31·8	
,, 5	0 9 57	23 26·2	14·0
,, 7	0 24 30	24 16·3	
,, 9	0 39 18	25 1·4	
,, 11	0 54 15	25 41·0	
,, 13	1 9 13	26 14·9	12·0

The comet is moving north at the rate of 1° per day. It will be in Pegasus all this month, and will pass about 4° north of α Pegasus (Marcab) near the 23rd inst. The maximum brightness is reached at the end of the month.

CATALOGUE OF RUTHERFURD'S PHOTOGRAPHS.—A year ago Dr. Lewis Rutherfurd presented to the Observatory of Columbia College all his photographic negatives taken between

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the years 1858 and 1878, and thirty quarto volumes containing the measures of many of them. The *Annals of the New York Academy of Sciences*, vol. vi., June 1891, contains a catalogue of these negatives. There are 139 negatives of the sun taken between 1860 and 1874, each of which has the time of exposure marked upon it. Several negatives were taken of the eclipses of 1860, 1865, and 1869. The solar spectrum is the subject of 160 negatives and 14 positives. The list of lunar negatives numbers 408, 40 of which are covered for protection. Mars was photographed in 1877, and the transit of Mercury in the following year. It is hoped soon to issue reductions of the measures of the numerous negatives of stars and clusters.

THE INSTITUTION OF MECHANICAL ENGINEERS.

A GENERAL meeting of the Institution of Mechanical Engineers was held on Wednesday and Thursday evenings of last week, the 28th and 29th ultimo. The meeting took place at the Institution of Civil Engineers, Great George Street, the theatre having been lent by the Council of the latter Society for the purpose. The President, Mr. Joseph Tomlinson, occupied the chair, and there were two papers on the agenda. The first of these, taken at the Wednesday's sitting, was "On some Details in the Construction of Modern Lancashire Boilers," by Mr. Samuel Boswell, of Manchester. The evening of Thursday was occupied with the reading of "The Report to the Alloys Research Committee," made by Prof. W. C. Roberts-Austen, C.B., F.R.S.

The first paper does not call for much attention at our hands. It dealt exclusively with boiler-making practice, and can hardly be of much interest outside the boiler-shop and draughting office. Within these limits the paper is one of great value, and therefore will occupy a most fitting place in the Proceedings of the Institution. The contribution of Prof. Roberts-Austen was of a very different description; and although it may not appeal so directly to the majority of mechanical engineers, it can hardly fail to improve the practice of engine construction, and advance the science of the production of mechanical energy many steps nearer that ideal of efficiency which is the goal all good engineers should keep in view. We have on previous occasions dwelt upon the excellent work done by the various Research Committees appointed by the Council of this Institution, and we can think of no better way in which the surplus funds of the Institution could be spent. Of all these Research Committees, it may be said that that appointed to consider the question of alloys is the most comprehensive and important, for we appear to be fast coming to a period when engines will consist almost wholly of two alloys—namely, brass and steel. Cast-iron will naturally continue to be used for massive parts where comparatively great weight is of small importance, but wrought-iron is every day giving place to steel, and steel castings have already almost entirely superseded those of iron in positions where it is desirable to combine lightness and strength.

Prof. Roberts-Austen's report is a long document occupying twenty-four pages of the Proceedings, and illustrated by several diagrams. We shall therefore, with the space at our command, be able to do little more than give an outline of its scope, or at any rate we can do no more than dwell on a few of the more salient features. In dealing with the question of iron and its alloys, the author assumed the reader to have an acquaintance with the work of the talented French physicist Osmond, of whom, as is well known, Roberts-Austen is a great admirer. Osmond holds that the results of his experiments show that there are two distinct varieties of pure iron—namely, the α or soft form, and the β or hard form. M. Osmond, it will be remembered, set forth his views in a paper read at the meeting of the Iron and Steel Institute, held in 1890.[1] Mr. Roberts-Austen had previously commenced an investigation upon the application of the "periodic law" of Newlands and Mendeleeff to the mechanical properties of metals, and the Research Committee requested him to carry his work in this direction still further. This law, as originally expressed, states that "the properties of the elements are a periodic function of their atomic weights." It has been shown that the effect of impurities added to gold is nearly proportional to their atomic volume, the larger the volume of the atom the greater being its effect.[2] It became interesting to determine, therefore, whether this holds good for other metals. Osmond had determined that the action of impurities on iron does appear to be in accordance with the periodic law; and he had arranged the elements in the following order in accordance with their atomic volumes, found by dividing their atomic weight by their specific gravity:—

I.		II.	
Carbon	3·6	Chromium	7·7
Boron	4·1	Tungsten	9·6
Nickel	6·7	Silicon	11·2
Manganese	6·9	Arsenic	13·2
Copper	7·1	Phosphorus	13·5
		Sulphur	13·7

Osmond pointed out that the elements in column I., whose atomic volumes are smaller than that of iron (7·2), delay during cooling, *cæteris paribus*, the change of hard iron into soft iron, as well as that of "hardening carbon" into "carbide carbon." For these two reasons they tend to increase, with equal rates of cooling, the proportion of hard iron that is present in the cooled iron or steel, and consequently the hardness of the metal. The elements in column II. tend to raise, or maintain at its normal position during cooling, the temperature at which the change of hard to soft iron takes place. Further, they render the inverse change during heating more or less incomplete, and usually hasten the change of dissolved or hardening carbon to carbide carbon. Thus they maintain iron in the soft state at high temperatures, and must therefore have the same effect in the cooled metal. In this way they would act on iron as annealing does, rendering it soft and malleable, did not their individual properties, or those of their compounds, mask this natural consequence of their presence. The essential part played by foreign elements alloyed with iron is therefore either to hasten or to delay the passage of iron during cooling to an allotropic state; and to render the change more or less incomplete in one direction or the other, according to whether the atomic volume of the added impurity is greater or less than that of iron. In other words, foreign elements of low atomic volume tend to make iron itself assume or retain the particular molecular form which possesses the lowest atomic volume; whilst elements with large atomic volume produce a reverse effect. The report goes on to point out that the effect of impurities on iron is far more complicated than in the case of gold; the latter being probably more simple in its molecular structure. Also if iron, by itself, can exist in two widely different states, the mechanical properties will be affected by the proportion of each. Lead also, which was one of the metals the Committee selected for investigation, probably exists in more than one modification. The author had made many experiments on the mechanical properties of lead as affected by a small quantity of impurity, but had not brought the results to any concordant or definite conclusion, and the inquiry was laid aside for a time. The fundamental necessity in carrying out the work of the Committee was a trustworthy pyrometer which would measure higher temperatures, and fortunately an instrument which appears to fulfil these conditions is now to be procured. This, we need hardly say, is the Le Chatelier pyrometer. This instrument consists of a thermo-couple of platinum and platinum-rhodium wire, the record being obtained by the measurement of the electric current produced. An autographic record is obtained by means of a spot of light thrown from a mirror attached to the galvanometer which measures the current. This spot of light is thrown on to a sensitized plate (Eastman's film) which is caused to travel by suitable means so as to give the time factor. The amplitude of the deflection naturally gives the temperature of the substance which is supplying the heat to the thermo-couple The calibration has been carefully effected by observations at known temperatures; and the instrument has been tested by observations in connection with the liquation of silver-copper alloys, about which a good deal is known.

The report next proceeds to deal with the effect of small quantities of impurity on the freezing point of gold—a metal which offers special advantages for investigations of this nature, as it may be prepared in a very high degree of purity, and is not liable to contamination by oxidation. Moreover, much is already known of its mechanical and thermal properties as influenced by small quantities of impurity. The effects of certain alloys upon gold are given in the report, and are well worth study on the part of those inquirers who wish to prepare them-

[1] See also *Comptes rendus*, vol. cx., 1890, p. 346.
[2] Philosophical Transactions of the Royal Society, vol. clxxix., 1888, p. 339.

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selves for a better understanding of the alloys of metals which come within the scope of every-day experience.

From the engineer's point of view, as the report states, the most interesting information which the pyrometer has yet afforded is connected with the measurement of the internal stresses in iron and steel. The molecular change which takes place in steel must be of vital importance when the metal is subjected at high temperature to mechanical operations such as rolling or forging. "Do the molecular changes in the iron take place at one moment throughout the mass of metal? that is, is the rate of cooling approximate throughout the mass, or does the external portion of the ingot cool so much more rapidly than the centre as to allow the molecular changes in the iron, and the relation between the carbon and the iron, to become completed near the surface long before they take place in the interior of the mass?" The pyrometer used allows some insight to be gained into this hitherto unassailable problem. A small ingot of mild steel had two holes drilled into it, one near the circumference, and the other at the centre. The ingot was heated, and a thermo-junction was inserted in each hole. In this way curves of temperature were obtained simultaneously. With the mild steel the evidence as to molecular change was but slight. Another ingot of steel, containing 0·799 per cent. of carbon, 0·084 per cent. of silicon, and 0·412 per cent. of manganese, was tried in the same way. The initial temperature at the centre was 1160° C. The curve showed the molecular change at 880° C., and the carbon change at 696° C. At the circumference the carbon change took place no less than four minutes earlier than at the centre, and at the lower temperature of 665° C. This is a most important point, as the rate of cooling, as Osmond has pointed out, has a measurable effect upon the temperatures at which molecular change occurs. The great internal strain which must be set up is evident when it is borne in mind that the carbon change is accompanied by a considerable alteration of volume. It is pointed out in the report that "there can be but little question that such experiments well deserve careful attention, and, in the hands of competent observers, should be fruitful of results."

On the conclusion of the reading of the paper, the President called for a discussion, when Dr. Anderson was the first to rise. He spoke in terms of warm praise as to the value of the work done by Prof. Roberts-Austen. As an instance, he mentioned that the method described in the report, by which the temperatures of an ingot could be obtained simultaneously at the centre and the circumference, would be of the greatest use in dealing with the large pieces of steel used for gun-hoops; and he expected great help from this in the work at the Royal Arsenal.

Mr. R. Hadfield, of Sheffield, followed. He gave a summary of the effect of the most prominent alloys of iron. This table will form a useful appendix to the report when published in the Transactions of the Institution.

Prof. Howe, of Boston, gave an instance in which the Le Chatelier pyrometer had been turned to good practical account. This was in the Rodman system of gun-casting. In that process it was most desirable to know the varying temperatures of different parts of the cast, but naturally this had been hitherto impossible. By inserting a thermo-couple in the mould it was possible to get this information at all times. He thought the Le Chatelier pyrometer the greatest boon that metallurgists had received for very many years.

The next speaker was Prof. Arnold, of Sheffield, who made a certainly vigorous speech. We think, however, that he was rather carried away by his enthusiasm. To say that the work done by the author of the report was "not worth a rush," is rather straining the prerogative of rhetoric; and we failed to see, when Prof. Arnold descended to facts, that he justified the florid language of his exordium. Prof. Roberts-Austen, in his reply, gave an example of forbearance and good temper which it would be well if men of science could often follow. It was satisfactory to notice that the feeling of the meeting was by no means in accordance with Prof. Arnold.

Mr. Stromeyer added to the work done a useful table in which were collated the opinions of various authorities on the effect of alloys upon iron. The table was not read, but will be published in the Proceedings. Such work as this is very acceptable. It involves a great deal of labour and brings but small return in the way of praise and glory, which of course are two things to which a true follower of science is profoundly indifferent.

Mr. Stead, of Middlesborough, protested against Prof. Arnold's remarks, and spoke of the value of the author's work. The testimony of Mr. Stead is valuable, as he combines the position of a practical investigator, working for commercial ends, and a man of science.

The meeting broke up after passing the usual votes of thanks.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The Junior Scientific Club held their first meeting this term in the Physiological Laboratory at the Museum, under the presidency of Mr. R. S. Gunther, of Magdalen.

Mr. W. Pullinger, of Balliol, read a paper on volatile platinum compounds, and exhibited prepared specimens.

Mr. A. F. S. Kent, of Magdalen, indicated improvements in the manipulation of photo-micrography whereby the effect of tremors was excluded, and passed through the lantern some very excellent slides which he had taken from negatives obtained by his new method.

Mr. G. E. C. Pritchard, of Hertford, exhibited specimens of Bacteria, and described the method whereby they had been obtained and prepared for microscopic exhibition.

Dr. Collier read a paper of a very interesting character on the physiology of muscular exercise with special reference to training, in the course of which he traversed some statements recently made by Sir Morell Mackenzie, to the effect that fatigue was due to the cessation of blood flowing to the muscles. Dr. Collier would rather attribute fatigue to the development of waste-products in the muscle, formed too rapidly for the blood to remove them, and quoted experiments carried out on frogs which seemed to support this view.

CAMBRIDGE.—The Agricultural Education Syndicate, in view of a grant of £400 a year from the Cambridgeshire County Council, recommend that a lecturer in agricultural science, who shall also be director of agricultural studies, should be appointed at a stipend of £500. They also propose that a second lecturer be appointed at a stipend of £300. These two lecturers would take between them the subjects of agricultural botany and agricultural chemistry.

The degree of M.A. *honoris causâ* has been conferred on the distinguished entomologist Mr. D. Sharp, F.R.S., Curator in Zoology at the University Museums.

Dr. Sir A. Geikie and Dr. T. G. Bonney have been appointed adjudicators of the Sedgwick Prize of 1895.

At St. John's College, on November 2, the following were elected to the vacant Fellowships: William McFadden Orr, B.A., Senior Wrangler, 1888; Edward Ernest Sikes, B.A., First Class (Division 1), Classical Tripos, 1889, Newton Student in Archæology; Percival Horton-Smith, B.A., First Class Natural Sciences Tripos, 1889-90 (distinguished in physiology), late Hutchinson Student in Physiology.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, October 26.—M. Duchartre in the chair.—On the theory of Hertz-oscillations, by M. H. Poincaré.—On a new mineral—boleite, by MM. Mallard and E. Cumenge. The new mineral occurs with copper in volcanic tuff and conglomerate found near Santa Rosalia, Lower California. It crystallizes in the cubic system, and its composition is represented by the expression $PbCl_2 + CuO.H_2O + \frac{1}{4}AgCl$. Its density is a little greater than that of calcite; cleavage easy parallel to the faces of cube, much less easy parallel to faces of octahedrons. Approximate index of refraction, 2·07.—Vaso-motor action of bacteria, by M. Ch. Bouchard.—Contribution to the botanical history of the truffle (fourth note): *Kama* of Bagdad (*Terfezia Hafizi* and *Terfezia metaxasi*) and of Smyrna (*Terfezia Leonis*), by M. Ad. Chatin.—On a storm observed at the Canary Islands. This is an extract from a memoir by M. de la Monneraye.—On the original causes of cyclones, and on their precursory signs: extract from a memoir by M. Le Goarant de Tromelin.—On the theory of the voltaic pile, by M. P. Duhem.—Experimental researches on a category of capillary phenomena, with an application to the analysis of alcoholic liquids and others, by M. Emile Gossart.—On bromo-

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stannates, by M. Leteur. The author has prepared the following bromostannates, the general method consisting in mixing concentrated solutions of the two bromides, and evaporating the mixture in a vacuum or dry air: $SnBr_4NH_4Br$, $SnBr_4NaBr + 6H_2O$, $MgBr_2 . SnBr_4 + 10H_2O$.—On a new crystalline ferric oxychloride, by M. G. Rousseau. Concentrated solutions containing more than 80 per cent. of Fe_2Cl_6, if kept for some time at a temperature between 160° and 220° C., give rise to crystalline ferric oxychloride, $2Fe_2O_3 . Fe_2Cl_2 . 3H_2O$. The author has studied the decomposition of solutions of ferric chloride at temperatures higher than 220°. Between 225° and 280° anhydrous oxychloride ($2Fe_2O_3 Fe_2Cl_6$) was obtained. At temperatures between 300° and 340° a new oxychloride was formed, having the composition $3Fe_2O_3 . Fe_2Cl_6$.—On the estimation of thallium, by M. H. Baubigny.—On the solution of bismuth chloride in saturated solutions of sodium chloride, and on the basic salicylate of bismuth, by M. H. Causse.—On a characteristic difference between the alcoholic radicles substituted in place of carbon and nitrogen, by M. C. Matignon. From a thermo-chemical investigation the author finds that the substitution of an alcoholic radicle for nitrogen increases the heat of combustion more than the substitution of the same radicle for carbon.—Action of benzoic acid on essence of turpentine, by MM. G. Bouchardat and J. Lafont.—On the formation of quaternary iodides of ammonium by the action of trimethylamine, in concentrated aqueous solutions, or the hydriodic ethers of several primary and one secondary alcohol, by MM. H. and A. Malbot.—On a new albuminoid substance in the blood serum of man, by M. C. Chabrié.—The soluble substances of the pyocyanic bacillus producing fever, by M. A. Charrin.—Experimental progressive muscular atrophy, by M. Roger.—Some anatomical characteristics of *Hyperoodon rostratus*, by M. E. L. Bouvier.—*Apropos* the chromatophores of Cephalopods, by M. Raphael Blanchard.—Physiology of the nerve which enables us to localize sounds, by M. Pierre Bonnier.—On a method for destroying insects injurious to the beetroot and cereals, by M. Decaux.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, NOVEMBER 5.

LINNEAN SOCIETY, at 8.—A Theory of Heredity based on Force instead of Matter: Rev. Prof. Henslow.

CHEMICAL SOCIETY, at 8.—The Disociation of Liquid Nitrogen Peroxide: J. Tudor Cundall.—The Magnetic Rotation of the Ammonium and Sodium Salts of Fatty Acids: Dr. Perkin, F.R.S.—The Vapour-Pressures and Molecular Volumes of Acetic Acid and of Carbon and Tin Tetrachlorides: Prof. S. Young.—The Ortho- and Para-nitro Derivatives of Orthotoluidine: A. G. Green and T. A. Lawson.—Researches on the Gums of Arabin Group, Part II.: C. O'Sullivan, F.R.S.

CAMERA CLUB, at 8.30.—The Action of Light and Heat upon the Haloid Silver Salts: Dr. J. J. Acworth.

FRIDAY, NOVEMBER 6.

PHYSICAL SOCIETY, at 5.—On Corresponding Temperatures, Pressures, and Volumes: Prof. Sydney Young.

GEOLOGISTS' ASSOCIATION, at 8.—*Conversazione.*

SATURDAY, NOVEMBER 7.

ESSEX FIELD CLUB, at 7.—Notes concerning the Distribution of Mollusca in the Thames Estuary: A. J. Jenkins.—Some Remarks upon the Aquatic Plants and Algæ of the Thames Marshes: A. J. Jenkins.—On the Occurrence of Westleton Beds in part of North-Western Essex: J. French.

SUNDAY, NOVEMBER 8

SUNDAY LECTURE SOCIETY, at 4.—The Personal Life of Shakespeare: W. E. Church.

MONDAY, NOVEMBER 9.

CAMERA CLUB, at 8.30.—Lenses, II.: Lyonel Clark.

TUESDAY, NOVEMBER 10.

MINERALOGICAL SOCIETY, at 8.—Anniversary Meeting.

INSTITUTION OF CIVIL ENGINEERS, at 8.—President's Address: George Berkley.—Presentation of Medals, Premiums, and Prizes.

PHOTOGRAPHIC SOCIETY, at 8.

WEDNESDAY, NOVEMBER 11.

GEOLOGICAL SOCIETY, at 8.—On Dacrytherium ovinum from the Isle of Wight and Quercy: R. Lydekker.—Supplementary Remarks on Glen Roy: Thos. F. Jamieson.

THURSDAY, NOVEMBER 12.

MATHEMATICAL SOCIETY, at 8.—On the Classification of Binodal Quartic Curves: H. M. Jeffery, F.R.S.—On Selective and Metallic Reflection: A. B. Basset, F.R.S.—On a Class of Automorphic Functions: Prof. W. Burnside.—The Contacts of Systems of Circles: A. Larmor.—Note on the Identity $4(x^3 - 1)(x - 1) = Y^2 + \beta Z^2$: Prof. G. B. Mathews.—Note on Finding the G Points of a given Circle with respect to a given Triangle of Reference: J. Griffiths.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—Description of the Standard Volt and Ampere Meter used at the Ferry Works, Thames Ditton: Captain H. R. Sankey (late R.E.) and F. V. Andersen.

CAMERA CLUB, at 8.30.—A New Method of Photography by Artificia Light: E. J. Humphery.

FRIDAY, NOVEMBER 13.

ROYAL ASTRONOMICAL SOCIETY, at 8.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—Description of the Works on the Barking and Pitsea Extension Railway: Henry E. Stilgoe.—Rail Pile Bridges in Ceylon: Harry Bucknall.

CAMERA CLUB, at 8.—Retouching: Redmond Barrett.

SATURDAY, NOVEMBER 14.

ROYAL BOTANIC SOCIETY, at 3.45.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Natural Theology: Sir G. G. Stokes (Black).—Elementary Trigonometry: J. M. Dyer and Rev. R. H. Whitcombe (Bell).—Fundamental Problems: Dr. P Carus, 2nd edition (Chicago).—L'Amateur d'Oiseaux de Volière: H. Moreau (Paris, Baillière).—Les Coquilles Marines: A. Locard (Paris, Baillière).—Colour-Blindness and Colour-Perception: Dr. F. W. Edridge-Green (Paul).—Handleiding tot de Kennis der Flora van Nederlandsch Indië: Dr. J. G. Boerlage, Tweede Deel, Eerste Stuk (Leiden, Brill).—Star Groups: J. E. Gore (Lockwood).—Elementary Thermodynamics: J. Parker (Cambridge University Press).—Report on the Meteorology of India in 1889: J. Eliot (Calcutta).—Copernic et la Découverte du Système du Monde: C. Flammarion (Paris, Marpon and Flammarion).—Moral Teachings of Science: A. B. Buckley (Stanford).—Further Reliques of Constance Naden: edited by G. M. McCrie (Bickers).—The Wire and the Wave: J. Munro (R.T.S.).—Ytterligare om Gadolinit-Jordens Molekylarvigt: A. E. Nordenskiöld (Stockholm).—Notes on the Recent Geometry of the Triangle: J. Griffiths (Simpkin).—Journal of the Royal Microscopical Society, October (Williams and Norgate).—Illustrations of the Flora of Japan, vol. i. Nos. 7, 8, 9 (Tokyo).

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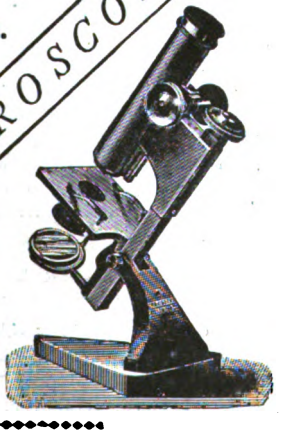

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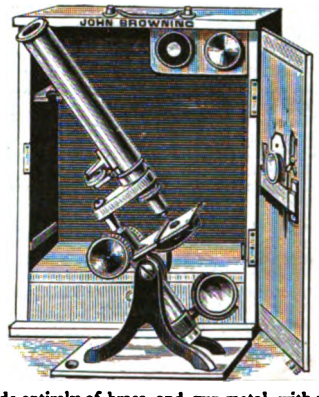

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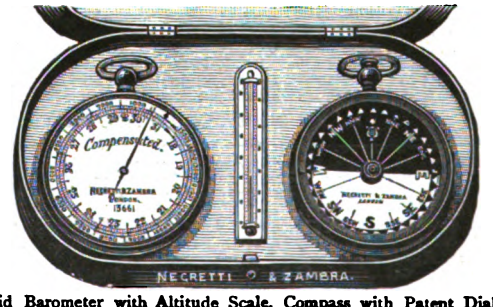

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THE HYGIENE OF WATER-SUPPLY.

An Elementary Hand-book on Potable Water. By Floyd Davis, M.Sc, Ph.D. (Boston, U.S.: Silver, Burdett, and Co., 1891.)

THE aphorism that "history repeats itself" is being very strikingly illustrated in the matter of hygiene at the present day. Questions respecting water-supply and the public health generally, which in this country were absorbing much of scientific attention some fifteen or twenty years ago, have only within the last decade begun to be seriously dealt with even in the most civilized of Continental countries and in the United States. Indeed, although we are indebted for much of the recent progress which has been made in what may be called the theory of hygiene to our Continental neighbours, yet in matters of actual practice we still hold, undisputedly, the first place among nations. The practice of hygienic principles cannot be introduced by Act of Parliament or Imperial ukase; it is the growth of years, or rather generations, and is quite independent of the establishment of hygienic institutes and bacteriological laboratories. In a few hours of Continental travel, it is possible to visit University towns provided with hygienic laboratories, munificently equipped, in which food-stuffs are daily submitted to elaborate analysis, whilst water and milk are searchingly interrogated as to the micro-organisms which they contain; and yet side by side with these refinements we find sanitary conditions, even in the houses of the well-to-do, which would hardly be found in the alleys and purlieus of one of our manufacturing centres. It is far from my wish or purpose to deprecate the establishment of institutions for the prosecution of hygienic inquiries on a scientific basis; on the contrary, such places are calculated to enormously accelerate the achievement of sanitary improvements, and to economize time, money, and human life, which are ruthlessly wasted when these improvements are attained as the result of empiricism and the operation of natural forces. Our position of supremacy in practical sanitation is mainly due to the long period of domestic repose and prosperity which we have enjoyed, and which has led us to turn our attention to the prevention of the unnecessary sacrifice of human beings even in civil life; but who can doubt that this position would have been much more rapidly gained if these endeavours had been always guided by scientific knowledge and systematic experimental inquiry? Even as it is, the path to our present position has been much shortened, and has been rendered less costly both as regards life and money, by the time and attention which have been bestowed upon sanitary matters by men of high scientific attainments. It is earnestly to be hoped that the recent Hygienic Congress held in our midst will have convinced those who control the purse of this country that a national effort must be made to maintain our position in the scientific as well as the practical progress of the century. We have not to deplore any shortcoming in the quality of the scientific work which emanates from us; in originality and as pioneers in all departments of science we are second to none; but quantitatively we are lamentably deficient, and in consequence, it is only too frequently the case that we have to leave to others the cultivation of those fields which we have ourselves had a large share in discovering. This is most conspicuously the case in the matter of hygiene; and after the highly discreditable obstruction, with which the foundation of our National Institute of Preventive Medicine was recently harassed, has now happily been swept away, we trust that public if not Government support will be forthcoming in the immediate future, to render that Institution, with its tremendous potentiality for benefiting mankind, second in usefulness and dignity to none in the civilized world. The State organization of science in the New World has made great strides during recent years, and scientific men in this country cannot fail to be impressed with the immense volume of work—more especially in applied science—which annually flows from the laboratories of the United States. The appearance of the book before us is, presumably, evidence of this great activity, showing as it does that there is a considerable body of men anxious to have presented to them in a concise and handy form all the main facts which have been accumulated—and which are dispersed in innumerable reports, blue-books, journals, and other forms of literature—concerning potable water. The difficulty of access to the original sources of this information renders such a work of great importance at the present time, but one which it is extremely difficult to do justice to. The present volume, we regret, does not come up to what we could wish for in a work of the kind. The questions which have to be discussed are in many cases necessarily more or less matters of opinion, in which conflicting evidence ought to be balanced and submitted to careful and critical analysis; unfortunately, however, for the exercise of this judicial power the author exhibits but little aptitude or inclination. The pages are sometimes filled with authoritative statements made by their respective authors on insufficient data, which statements have been copied, often not even from the original sources, without a word of elucidation or criticism. Such material, placed in the hands of the unwary reader, may lead to very serious consequences. Of this character is the statement that "the power of certain samples of water to dissolve lead is directly proportional to the number of micro-organisms that the samples respectively contain," which might well have been omitted from this work; and its introduction as almost the only piece of information concerning the action of water on lead is singularly inappropriate. Again, on another page, we are categorically informed that "even milk is sometimes the agent of this disease (typhoid fever), in which case the typhoid poison remains undestroyed in passing from the polluted water from which the cows drink, to the milk-secreting glands"; whilst no mention is made of the real mode of transmission by the watering of milk and the rinsing of cans with contaminated water. In most cases the principles laid down are sound and reasonable; but the author has permitted himself to be carried some-

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what too far in his advocacy of pure water, when he says that "scientific investigation also reveals the fact that, as a community is supplied with pure water, there is not only a decrease in the disease and death-rate, but often a most surprisingly rapid increase in thrift, morality, and degree of civilization." We should be glad indeed if he were correct in his statement that since the introduction of an efficient health administration in England, the prevalence of typhoid has been reduced to such an extent that "for weeks and even months not a single case now occurs in the city of London." We can readily understand that our rivers must appear insignificant enough to the inhabitants of a country containing such mighty streams as the St. Lawrence, the Mississippi, and the Ohio; and although we are fully alive to some of them being disgracefully fouled, we certainly are somewhat startled to have our watercourses, which are dear to many of us, disposed of in the following sentences:—"The pollution of English streams is carried to such an enormous extent that the waters of many, where city sewage enters them, are actually offensive, and during the summer months, owing to the stench, the passenger traffic is forced to the railroads. In some of these streams the whole surface of the water, for some distance below sewage entrance, is in a state of commotion, owing to the evolution of gas bubbles, and the water is so foul that it cannot be used in the boilers of the little steamers that ply across the rivers. Immediately below the entrance of sewage no life can exist in the water, on account of the presence of ferrous sulphate (*sic*), which is a disinfectant." In dealing with the much vexed subject of the apparent self-purification of streams, the author shows a very just appreciation of the matter when he points out that there "is no guarantee that running water is perfectly wholesome at any distance below a point where it is certainly polluted with the contents of sewers and privy-vaults, or the decomposition of vegetable and animal matter. The question as to what extent must impure water be diluted or oxidized to render it safe for domestic purposes, cannot be answered. Mere dilution of polluted water does not render inoperative the action of living bacteria. . . ." We are glad to see that the author points out the importance of boiling all drinking-water which is open to suspicion, for it cannot be too frequently reiterated that perhaps the two most effective measures which the private individual can take in avoiding zymotic disease consist in boiling the water and milk that are used for drinking. The largely increasing consumption of ice, which in America has assumed enormous proportions, is a matter which also calls for very careful attention, since recent experiments have shown that, although the living bacteria in ice are considerably less numerous than in the water from which the ice has been derived, still the process of freezing, even if long continued, affords no sort of guarantee that the dangerous forms originally present in the water shall have been destroyed. Thus the bacillus of typhoid fever has been found still alive in ice which had remained continuously frozen for a period of 103 days.

PERCY F. FRANKLAND.

CAUSATION OF SLEEP.

The Intracranial Circulation and its Relation to the Physiology of the Brain. By James Cappie, M.D. (Edinburgh: James Thin, 1890.)

THE factors concerned in the production of sleep have from time to time engaged the attention of physiologists, and various theories have been advanced to explain the phenomena. The author of the work now before us, so far back as 1854, published a short essay on "The Immediate Cause of Sleep," which he subsequently expanded into a volume entitled "The Causation of Sleep" (Edinburgh, 1882). In the work now under consideration, although with a different title, the author travels over much the same ground as that surveyed in his previous writings on this subject, and adds to it some additional chapters.

In his successive publications Dr. Cappie accepts the position usually taken up by physiologists, that the state of sleep is accompanied by a diminished brain circulation; but he combats the view that sleep is due to a diminution of the whole mass of blood within the cranial cavity, and that the compensation for this diminution is got by an increase in the amount of cerebro-spinal fluid in the ventricular and sub-arachnoid spaces of the brain. His objection to this opinion is based upon its not being reconcilable with either the physics or the physiology of the parts situated within the cranium. As regards the physics, he adopts the view advocated by Drs. Alexander Monro (*secundus*), Abercrombie, and Kellie, that, inasmuch as the brain lies within a closed cavity, which possesses rigid bony walls, the contents cannot be affected directly by the pressure of the atmosphere, which can only influence the interior of the cranium through the blood-vessels, so that a force is constantly in operation to maintain the amount of blood within the intracranial vessels. The author believes that the effect of the pressure on the blood-vessels, say of the neck and head, is opposed to the movement of the blood in the veins, and that the tendency of the pressure is to keep the blood within the veins which ramify in the vascular membrane enveloping the brain, called the pia mater. At the same time, however, the arterial stream drives the blood onwards into the capillaries and the veins, which tends to dilate the latter vessels, and, in conjunction with the backward pressure on the great veins, to retard the flow of blood through the veins of the pia mater, and consequently through the great venous sinuses of the head, into the jugular veins. In this way he infers that, whilst the brain itself becomes less vascular, the mass of blood within the cranial cavity continues the same, but its mode of distribution is altered: a less proportion is within the arteries and capillaries, whilst an increase takes place in the contents of the veins of the pia mater.

The author acknowledges, in connection with the nutrition of the brain, that molecular actions of a subtile kind take place between the blood and blood-vessels and the nervous tissues, and that these are much less active during sleep than when awake. The lessened activity in the nutrition of the nerve protoplasm diminishes the activity of the capillary circulation. He regards, however, the change in the balance of the circulation

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between the arteries and capillaries on the one hand, and the veins on the other, as the key-stone of the theory of the causation of sleep. The altered balance of the circulation occasions a change in the balance of active pressure, which is not so much within the brain substance as on the surface. It is less expansive and more compressing, and with this compression consciousness is suspended.

In proof of his theory, the author adduces observations made by Dr. Hughlings Jackson and himself on the retina—the blood-vessels of which are so intimately connected with those of the brain—both during sleep and in a state of coma, from which it would appear that in these conditions the retina was paler, its arteries smaller, but its veins were larger, more tortuous, and distended. In another case recorded by Dr. Kennedy, where a portion of the skull and dura mater had been removed, and the pia mater consequently exposed, it was noticed that the veins in the latter were during sleep congested and assumed a dark hue.

In a concluding chapter, entitled "Some Points in Mental Physiology," which was not contained in his previous work "On the Immediate Cause of Sleep," the author considers how far the peculiarities of the encephalic circulation may affect the functional activity of the different parts of the brain. Starting from the position that the brain is a composite organ, and that distinct portions are put into a state of functional activity in connection with the discharge of their respective duties, the question of balance of the circulation has again to be considered. For the part which is more immediately concerned in the production of the particular cerebral operation must become the seat of vascular excitement, and the amount of blood flowing through its vessels will be greater than that transmitted through the vessels of those other parts of the brain which are for the time being not so functionally active. Hence a certain tension of the area or centre which is actively working must arise, and the encephalic circulation is focussed in the direction of activity. The parts which surround the operating centre would act as a background of resistance, and would afford such support as will secure the immediate liberating action in the discharging centre.

The author applies his views on the encephalic circulation to the explanation of the phenomena of Hypnotism. The first incident in the hypnotic state is a steady prolonged effort of volition in which the attention is concentrated in a very restricted direction. The immediate consequence is fatigue of the nerve-centres concerned in keeping up the strain. Their molecular motions become enfeebled, the circulation through them is less active, and a condition approaching that of sleep is produced. If then, in the form of a "suggestion" from another, some stimulus calls into activity a part of the brain not fatigued in the effort of attention, the vascular activity in it will be increased, and its function will be intensified. An assertion boldly made to a hypnotized person may influence belief in opposition to former experience, and if it be towards an ideational centre, some particular notion may so monopolize the consciousness that discrimination and judgment may become almost as completely in abeyance as in ordinary dreaming.

OUR BOOK SHELF.

Physiography: Elementary Stage. By J. Spencer, B.Sc., F.C.S. (London: Percival and Co., 1891.)

As an introductory science, physiography is one requiring very careful treatment. Its range is undoubtedly wide, but it is correspondingly shallow, and should rather lead up to scientific thought than aim at giving an incoherent collection of facts. In the book before us, the author, who is head master of one of our most successful technical colleges, begins well by recommending that teachers should endeavour to make the subject a practical one, by the performance of experiments, excursions into the neighbouring country, and the collection of specimens of rocks, minerals, and fossils. The experimental shape which the book has consequently taken is one of its most noticeable features; but it certainly falls short of the expectations raised. The chemical and physical parts appear to be excellent, but there is little to assist either teacher or student in gleaning information from the inspection of natural phenomena. A peat bog, for instance, is a fruitful subject for study under proper guidance; but this is not afforded by the scanty remarks on p. 89. In dealing with a wide subject, brevity is essential, but this does not necessitate the omission of the fundamental points, and looseness of expression. The book, however, shows many signs of a want of care in this respect. In the note on Foucault's pendulum (p. 210), for example, the whole point of the explanation is lost by the absence of a reference to the permanence of the plane of swing. On p. 113, it is stated that "submarine volcanoes produce new land, the erupted matter being piled up sufficiently high to form islands"; the omission of the qualifying word "sometimes" might obviously lead to a misconception. Test questions, original and selected, are given at the end of each chapter.

The book is well illustrated, and, with careful revision, should make a useful addition to existing text-books.

Mayhew's Illustrated Horse Doctor. Revised and Improved by James Irvine Lupton, F.R.C.V.S. (London: Griffith, Farran, and Co.)

THE continued existence of domestic medicine, whether the subjects of its application be human beings or domesticated animals, cannot well be doubted. We may, however, be permitted to doubt whether true economy lies in this direction. Nevertheless, as residents in the colonies, and even in many rural districts of our own country, are often far removed from the qualified veterinarian, and considerable time must necessarily elapse before his services when sought can be forthcoming, it is obviously advisable that whatever measures are taken by the stock-owner to ameliorate the sufferings of the animal during this interval of time should be rational, and follow lines similar to those which the professional man would adopt. Wrong methods of procedure would serve only to handicap and hamper his efforts. The book before us can be recommended to guide the horse-owner in such emergencies; though we note with pleasure that the reviser, on p. 553, points out that "the reader will always best consult his interest and pocket by at once consulting a qualified veterinarian." Agreeing with this proposition, we cannot but consider superfluous the introduction of minute directions for the performance of such delicate and difficult operations as the extirpation of the eye-ball, the division of the plantar nerves (neurotomy) in some forms of foot-lameness, and section of contracted tendons (tenotomy). We can scarcely conceive an owner, however intelligent and courageous he may be, proceeding to cast, chloroform, and perform any of the above-mentioned operations.

Horse-owners will do well to note the stress which is laid upon the construction, ventilation, and drainage of

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stables, and the varied information given on the prevention of disease. The reviser has scant sympathy with the use of the actual cautery; but here he is on debatable ground, and, notwithstanding his strictures, we fancy veterinarians will not lightly lay aside an agent which, rightly or wrongly, most of them believe to be potent for good.

The illustrations form the least satisfactory portion of the work. Many of them are grotesque and ludicrous to the last degree, and ought to have been eliminated from the present revised edition. Some, such as those indicative of the symptoms of colic, are good, and well convey their intended meaning. W. F. G.

Handleiding tot de Kennis der Flora van Nederlandsch Indië. Door Dr. J. G. Boerlage. Tweede Deel, "Dicotyledones Gamopetalæ." Eerste Stuk, "Inferæ—Heteromeræ; Caprifoliaceæ—Styracaceæ." (Leyden: E. J. Brill, 1891.)

PREVIOUS parts of this work have been noticed in these columns. It is more than thirty years since the last part of Miquel's "Flora Indiæ Batavæ" appeared, a work written chiefly in Latin; but the present publication cannot be regarded as replacing it, or as being a successor to it. So far as we have tested this "Manual," it is a Dutch translation of the descriptions of the natural orders and genera in Bentham and Hooker's "Genera Plantarum," followed by a list of the species inhabiting the Dutch Indies. Locally it may be serviceable; but what is wanted by botanists generally is a new descriptive elaboration of the species of the region in question. W. B. H.

By Sea-shore, Wood, and Moorland: Peeps at Nature. By Edward Step. (London: S. W. Partridge and Co., 1891.)

THE author of this book, under his pseudonym "James Weston," published in 1886 "Stories and Pictures of Birds, Beasts, and Fishes"; and, two years later, a companion volume, "Stories and Pictures of Animal Life." Both of these volumes, which were very popular, are now out of print. In the present work they have been amalgamated, and the author has added to them some brief "nature-papers" which he has contributed to various periodicals. The book is intended chiefly for young people, and it is so pleasantly written that children who have a chance of reading it can hardly fail to find it attractive. They will obtain from it much interesting information about all sorts of animals, and it will help them to realize that even the most familiar objects, when properly observed, may be worthy of close study. The book is very carefully illustrated.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Note on the Chromosphere Spectrum.

WITH the new spectroscope of the Halsted Observatory, which has a 5-inch Rowland grating of 20,000 lines to the inch, I have repeatedly observed of late that the bright chromosphere line, Angström 6676·9 (No. 2 in my catalogue of chromosphere lines), is not coincident with the corresponding dark line of the solar spectrum, but is *less refrangible* by about one-third of a unit of Rowland's scale. This chromosphere line, therefore, can no longer be ascribed to iron, but must be due to some other substance as yet undetermined.

I think there can be no doubt as to the non-coincidence. The interval between the bright and dark lines varies to some extent with circumstances, being usually less in the chromosphere spectrum on the sun's eastern limb than on the western, and it is often affected by motions in the line of sight; but nine times out of ten the want of coincidence is perfectly obvious.

I may add that I have also obtained a considerable number of photographs of the ultra-violet spectrum of the chromosphere with the new instrument, and get complete confirmation of almost all Mr. Hale's results. I find not only the constant reversal of the H and K lines, but I have obtained, so far, five of the ultra-violet series of hydrogen lines; the first of them being the well-known "companion" of H (first visually observed by myself in 1880), and the other four in their regular succession above it.

The only point in which my plates fail to confirm Mr. Hale's is that I have not yet succeeded in catching the duplicity of the hydrogen α (3889). Several of his plates show at this point two lines near together; none of mine do so, and I conclude that the companion line makes its appearance only rarely. I first observed this line visually in 1883 (*American Journal of Science*, November 1883), and it has since been often seen here by my as istant, Mr. Reed, as well as by myself.

Of course the opinion is no longer tenable that H and K can be due to hydrogen, since the measures clearly show that the companion to H belongs to the hydrogen series. But I am still sceptical whether they are due to calcium, at least in its terrestrial condition. C. A. YOUNG.

Princeton, N.J., U.S., October 20.

Formation of a Temporary Cyst in the Fresh-water Annelid *Æolosoma*.

AS I am not aware that the formation of a temporary chitinous cyst has been described in any Oligochætous Annelid, the following observations may be of some little interest. A few days ago Mr. Latter, science master at the Charterhouse, kindly forwarded to me three tubes containing a quantity of *Æolosoma quaternarium*. They were so abundant that every drop of water contained several specimens; in the water I found also a still larger number of spherical bodies, which proved to be cysts, each completely filled by a single worm coiled once upon itself. The cysts were perfectly colourless and transparent, and very thin-walled; one cyst was found empty, and had been ruptured by the worm in escaping from it. Twenty-six years ago Maggi (*Mem. Soc. Ital. Sci. Nat.*, 1865) described and figured some bodies, oval in form, which he believed to be the cocoons of *Æolosoma*; embryos were found in these cocoons in different stages of development. Prof. Vejdovsky (*Entwickelungsgesch. Unters.*, Heft 1, Prag, 1888) suggested the possibility that these bodies were really cysts; my own examination of what are, I think, undoubtedly cysts in *Æolosoma* leads me to agree with Vejdovsky's suggested interpretation of Maggi's figures. If the structures which I describe here are really cocoons, their form differs from that of the cocoons of all other Oligochæta, in being spherical and without a narrow process at either pole; moreover, it is—on the hypothesis that they are cocoons—a remarkable coincidence that they should *all* contain completely adult worms; finally, and this is of course conclusive, the intestinal tract of the worms contained vegetable detritus. The cysts, as I believe them to be, are of about the size of a *Volvox globator*. In this encysted condition the worms might perhaps be easily transported from place to place; I have found that they survived the evaporation of the surrounding water upon a glass slide for a considerable period; the worms were in continual movement within their cysts, so that it was quite easy to be sure that they were alive. Now this very species has a wide distribution, which may perhaps be partially accounted for by this habit of encystment; with regard to other fresh-water Annelids which have a wide range, such as *Tubifex*, it is possible that birds may inadvertently transport the cocoons from country to country. It is not known yet whether *Æolosoma* forms a cocoon, though it is probable, as the worm develops a clitellum. But sexual maturity appears to be a comparatively rare occurrence in *Æolosoma*; very few observers appear to have seen the sexually mature worms. If this is so, the encystment of the worm may take the place of one of the secondary uses of the cocoon—namely, to aid in the diffusion of the species. FRANK E. BEDDARD.

Polytechnics and Recreation.

IN Mr. Oliver Dawson's article on Polytechnics in your issue of the 8th inst. (vol. xliv., p. 547) he says: "although

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those institutes which make much of athleticism and such matters attract the largest proportion of students, the attendance *pro ratâ* in the class-rooms would not favourably compare with an institute carrying out a purely educational programme." A Polytechnic is mentioned in which, though only seven students entered the class, "scores of young men could be found in the billiard-room and gymnasium"; and the opinion is expressed that even the excellent work of the Regent Street Polytechnic "would be still better if it could be relieved of the recreative element."

May I say a word for recreation, as the representative of a College which will eventually form part of the South London Polytechnic, and which has not been "started by a teacher," but has grown up out of a purely recreative institution, for the Victoria Hall (the parent of the Morley Memorial College) is nothing more, unless we use the word in a very narrow sense.

It is a commonplace truth that the aim of education should be to develop the whole man, not to make mere intellectual experts any more than mere manual experts. Surely recreation has not only a legitimate but a very important place in this, especially where sedentary workers are concerned. Those whose tastes are naturally studious may with comparative safety be left to take care of themselves. In these days, instruction of some sort may be had by most of those who set their minds on it, and if they miss much which books or the living teacher would have enabled them to gain, at least their energy is not likely to be turned into hurtful channels. Those who desire recreation can also get it, and with little exertion; but of what kind? If the gymnasium spoken of above had not been open, what would its scores of frequenters have been doing? Some, no doubt, might have been in class, improving their minds more than they are likely to do in a gymnasium; but others, whose youthful spirits need an outlet, would not have been much attracted by study not associated with recreation. The music-hall, or some of the many forms of betting now prevalent, would have been more likely to entice them. In the confined life of our towns it is no small good to provide athletic sports (apart from temptation to drink) as a safety-valve for boyish spirits, even if the good stops short there. But it need not stop short. Of course, there is danger lest the recreative side of an institute or Polytechnic should swamp the educational, unless care is taken to prevent it. A very simple rule, however, is sufficient for this. Our members are not allowed to use the gymnasium or recreation rooms unless they are *bonâ fide* students of at least one class. It is not sufficient that they should *take a ticket* for a class. The registers are occasionally looked over, and if frequent absence from class is combined with frequent attendance in the gymnasium or recreation rooms, a warning, suspension, or even expulsion, is the consequence. We have found that only in an insignificant number of cases is it necessary to proceed to the last resort. Our students as a rule receive an excellent character for steady work from such of our teachers as are in a position to compare them with other students. "I am tired of teaching lads who are trying not to learn," said one who held an important position in a large educational establishment; "your fellows mean business; it's a pleasure to teach *them*." And the testimony of others is to the same effect.

If the moving spirits of a Polytechnic love work themselves, and if they are careful to enlist the sympathy of students, so as to lead them by example rather than drive them by rigid rule, then there is little danger of the institution degenerating into a mere place of amusement.

EMMA CONS.

Samuel Morley Memorial College, Waterloo Road, S.E., October 16.

"W = Mg."

I SHOULD like to take exception to Prof. Greenhill's statement in your issue of September 24 (vol. xliv. p. 493) that "when goods are sold in commerce by weight, they are weighed in scales, and the weight is the same wherever the weighing is carried out, whether at the equator, or the poles, or in the Moon, Sun, or Jupiter." In this country it is the commonest thing in the world to see goods sold in commerce weighed in a spring-balance, which is also the universal kitchen weighing apparatus, and I respectfully submit that the weight indicated would *not* be quite the same in the Moon, Sun, or Jupiter.

The appeal to the scales seems to me to be an attempt to throw dust in our eyes, as what Prof. Greenhill really means is that two *equal* weights are *equal* (not each the *same*) wherever the weighing is carried on—a balance telling us nothing about the weight, or pull downwards, of either one.

I was fortunate in getting some of my first notions of dynamical measurements from Thomson and Tait, and hence the appearance of the "blooming g" did not seem unnatural, for after I had learned how to measure a force properly in dynamical units, I was told that a pound's weight = g poundals, or a gram's weight = g dynes, which suffices for reduction to non-absolute units. This, in my opinion, is virtually the same as Prof. Slate's suggestion. I never could see why g should appear in dynamical formulas: measure in absolute units, and at the end reduce to pounds' weight from poundals as above. Of course this involves knowing what an absolute dynamical unit is, and it strikes me that a few more "horizontal" experiments with spring-balances, graduated in poundals or dynes, and a little less thinking about arm balances, would go far to clear up difficulties in the minds of students.

ARTHUR G. WEBSTER.

Clark University, Worcester, Mass., October 14.

[It will be interesting to see if Mr. Webster can devise a horizontal spring dynamometer which will record within 10 per cent. of the true value; also to know what corrections he would apply for the inertia, temperature, and fatigue of the spring, and how he would occasionally test the indications. These difficulties have to be met in Diagrams given by Steam-Engine Indicators. How does the Inspector of Weights and Measures test Spring-Balances in America?—A. G. G.]

Alum Solution.

PERHAPS the following evidence of the *practical* superiority of potash alum solution to distilled water in adiathermancy, *when the electric arc is the radiant* and the "radiometer" a Crookes, may be of interest. The same glass-sided cell was used throughout, and the difference of voltage between the carbons ("Apostle") was kept sensibly constant (40 volts) through the experiments. Between each observation on the liquids the radiation from the arc was observed unimpeded, save by the glass of the radiometer, as recorded below. The time was given by a metronome (previously examined for constancy) beating half-seconds. No lens was used.

	No. of revolutions of radiometer arms.	Time in half-seconds.
1. Unimpeded radiation ...	25	160
2. Through empty cell ...	25	254
3. Unimpeded radiation ...	25	152
4. Cell + water	25	820
5. Unimpeded radiation ...	25	164
6. Cell + water (second experiment)	25	832
7. Unimpeded radiation ...	25	160
8. Cell + alum solution ...	25 (time of two revolutions actually taken.)	43,200
9. Unimpeded radiation ...	25	160

The current throughout was 6·4 amperes, thickness of each glass plate 1·5 mm., thickness of solution 50 mm., distance of radiometer from arc 1 metre.

T. C. PORTER.

Eton College, October 29.

The Salt Lake of Aalia Paakai.

I HAVE recently made an analysis of the water of the salt lake of Aalia Paakai, near Honolulu, and have thought that the results might be of interest to the readers of NATURE. The lake occupies the crater of an immense tufa cone, whose ejecta cover several square miles, and are especially remarkable for containing numerous aggregations of crystalline grains of pure olivine. The lake is just at mean sea-level, and is scarcely a mile distant from the ocean, but there is evidently no free communication with the waters of the sea.

During the dry months crusts of salt are deposited, sometimes six inches or more in thickness, on the bottom of the lake, and the salt has at times been taken out for use. In the rainy season the salt is wholly redissolved. The crust of salt is at the present

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time from one to three inches thick, and the water is, of course, a saturated brine. It is interesting to note, however, that it does not correspond in composition with the water from the ocean. Like the Dead Sea, the lake contains an excessive quantity of calcium salt.

The interior of the crater basin is crusted in many places with deposits of carbonate of calcium, proving that it was at one time occupied by a highly calcareous water, probably of high temperature. I have given in connection with the results of my analysis, which extends only to the constituents present in large amount, an analysis of concentrated sea-water from the salt works of Kakaako, and an average of a number of analyses that have been made of the waters of the Dead Sea. These latter sometimes contain a larger proportion of solids than the average figure, but in no analysis that I have seen has the quantity been as large as that found in the water of Aalia Paakai.

Constituents.	Water of the Salt Lake.	Water of the Dead Sea.	Concentrated sea-water.
	Grains per gallon.	Grains per gallon.	Grains per gallon.
Chloride of sodium ...	6989	5137	13239
Chloride of calcium ...	7742	2077	Absent
Chloride of magnesium	7790	8235	3779
Bromide of magnesium	99	208	57
Sulphate of magnesium	Absent	Absent	2478
Sulphate of calcium ...	34	58	22
Chloride of potassium	156	736	534
Total solids...	22,810	16,451	20,109
Weight of one wine gallon (approximate)	73,044	68,900	72,180

Honolulu, October 16. A. B. LYONS.

Meretrix, Lamarck, 1799, *versus Cytherea*, Lamarck, 1806.

IN the notice of Mr. Newton's "List of Mollusca," in NATURE of October 29 (vol. xliv. p. 610), I read as follows:—"Many old favourites have been thus relegated to obscurity, whilst fresh names, dug up from some forgotten corner, have, by the law of priority, taken their places. Thus, *Meretrix*, Lamarck, 1799, takes the place of his better-known *Cytherea* of 1806, the latter having been applied by Fabricius, in 1805, to a dipterous insect."

The Dipteron *Cytherea obscura*, Fab. 1805, was described nine years later than *Mutio obscurus*, Latreille (1796), which is the same species. Meigen, in his principal work (1820), acknowledged the priority, and the insect has been called *Mutio* ever since. As the typical species is the same for both genera, there is no chance whatever for *Cytherea* to be resuscitated, and it may well remain as the name of the Mollusk. I most heartily agree with the opinion of the reviewer, that "it would be an immense gain if every name proposed to be altered had to pass through a regularly-constituted committee of investigation before it was accepted and allowed to pass current." In such a committee, besides *priority*, two other paramount scientific interests should be consulted, and they are—*continuity* and *authority*. C. R. OSTEN SACKEN.

Heidelberg, November 1.

A Plague of Frogs.

I HAVE just read with great interest the letter in NATURE of the 5th inst. (p. 8), signed R. Haig Thomas, *à propos* of frogs entering his cellar.

During the past seven years I have resided in three separate lodgings (no two being within half a mile of the other), each having a small garden at the back surrounded by a solid wall. The lowest of these was about 5 feet, and in two cases the walls were quite bare. In the third case there were creepers on both sides. But in all three cases has *one* frog suddenly made its appearance, and always during very wet weather. To account for their entrance has completely puzzled me.

B. A. MUIRHEAD.

Pall Mall Club, Waterloo Place, November 8.

Red Light after Sunset.

THERE was at Lyons, N.Y., last evening, a magnificent display of red light similar to the sunset glows which attracted so much attention a few years ago. The entire western sky was of a deep lurid red, resembling a conflagration, for three-quarters of an hour or more after sunset. M. A. VEEDER.

Lyons, N.Y., October 30.

Topical Selection and Mimicry.

WILL you permit me to make a few remarks on Dr. A. R. Wallace's review of my book ("On the Modification of Organisms") which appeared in your journal on April 9 last (vol. xliii. p. 529)? I cannot disguise from myself the fact that in attempting any reply I labour under great disadvantages: first, in having to combat the statements of such a high authority as Dr. Wallace; and secondly, in writing as I am from the Antipodes, my reply cannot reach your readers for at least three months after the publication of the review in question. Nevertheless there are two statements made by him which demand some notice from me.

The first is that I have misrepresented Darwin's views on the question of natural selection. My reply to this is distinct and emphatic. The references to Darwin in my book are absolutely correct: there is no misrepresentation; there is no misquotation. In every reference to Darwin's views I gave the page and the edition from which the quotation was taken. In writing my book I was perfectly aware how important it was to start with a clear understanding of what Darwin meant by the term natural selection, and I was at the utmost pains to quote his exact words in every reference I made to him. It is not my fault if Darwin did not give a clear or consistent definition of natural selection, or that he confounded cause with effect, as when at one time he defined natural selection as "the struggle for existence," and at another time as "the survival of the fittest." I can therefore with the utmost confidence refer your readers to the book itself in confirmation of what I here state.

Dr. Wallace has also been good enough to give, as a sample of my "teaching," a part of a sentence of mine on the subject of mimicry. He says your readers "may estimate the value of Mr. Syme's teaching by his explanation of mimicry, which is, that natural selection has nothing to do with it, but that insects choose environments to match their own colours. He tells us that these extraordinary resemblances only occur among insects that are sluggish, and that 'to account for the likeness to special objects, animate or inanimate, we have only to assume that these defenceless creatures have intelligence enough to perceive that their safety lies in escaping observation.'"

Now I did not state that these extraordinary resemblances occurred only among insects; what I said was that they occurred "chiefly" among insects. I am aware that, judging from Dr. Wallace's stand-point, I may have disposed of the subject of mimicry in a somewhat off-hand way, and for the simple reason that I regarded mimicry as a subordinate branch of the more important subject of protective coloration, which I had treated at some length; and in adopting this course I was taking as my guide Dr. Wallace himself, who has elsewhere stated that "the resemblance of one animal to another is of exactly the same essential nature as the resemblance to a leaf, or to bark, or to desert sand, and answers exactly the same purpose" ("Natural Selection," p. 124, 2nd edition). So far, then, I may presume that I am in good company. To understand what I said about mimicry, therefore, it is necessary to know my views on protective coloration. Protective coloration I regarded as, in certain cases, the result of heat and light acting on the pigment cells, and, in other cases, the result of what, for want of a better name, I may call topical selection—that is, the selection by the animal of its environment. Obviously, this environment would be a cover or background which would enable the animal to escape observation, as by that means many animals, especially such as are not possessed of great speed or great powers of flight, might elude their enemies, or, if Carnivora, might steal upon their prey unawares. No doubt there is something captivating in the idea of a universal cause to which every change in the organic world may be referred; but it is surely contrary to the rules of right reasoning to invoke the aid of a greater force than is necessary to account for a given result. This is what the Darwinist does, however, in order to explain the phenomena of protective coloration and mimicry. It is well known, however, and it has been pointed out by Dr. Wallace himself, that certain

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varieties of protectively coloured insects are frequently confined to very limited areas. Some will only be found on a certain species of tree or plant; others only on rocks or a stone wall of some particular colour; others, again, only on small patches of soil or gravel; while a short distance from these there may be other objects differently marked, which may be frequented by insects altogether different in colour, although belonging to the same or to an allied species. Are we to suppose that every tree, plant, rock, every stone wall, and every distinctive patch of soil or gravel, has been the scene of natural selection? There is no other conclusion open to the Darwinist. But when it is considered that natural selection may take hundreds of thousands or even millions of years, to effect a given result, the strain upon our forbearance must be great when we are asked to believe that this process is the only one we have to reckon with. If the phenomena can be accounted for by a shorter or simpler process, why should the longer and more complex one be insisted on? Is it not more reasonable to suppose that animals have sufficient intelligence to fly to, and remain in, the place where experience has shown they are least exposed to observation? Can anyone doubt that animals possess such knowledge? How otherwise are we to explain the action of the butterfly, for instance, in darting at once when disturbed to some object which resembles itself, and then lying perfectly still, when one might in vain attempt to find it, although within a few inches of it?

This view also receives corroboration from the fact that many unprotected animals render themselves inconspicuous by covering themselves with materials which resemble their environment. Thus certain Lepidopterous larvæ form cases for themselves out of the fragments of the substance on which they feed, the cases of the larvæ of the Psychidæ, for instance, being made of leaves or of brown grass stems; those of the Essex emerald moth of fragments of leaves spun together with silk; certain species of sea-urchins and many Mollusca cover themselves with grains of sand, shell, and bits of stone, while, according to Poulton, certain species of crabs fasten species of seaweed to their bodies for the same purpose.

Topical selection will also explain the protective coloration of certain vertebrates, as rabbits, hares, and deer. Thus Mr. H. A. Brydon, who has an extensive acquaintance with the habits of deer in South Africa, writes ("Kloof and Karoo," p. 298) as follows:—

"In some localities where the 'zuur veldt' clothes the upper parts of the mountains, and the 'rooi' grass the lower portions, the vaal and the rooi rhebok may be found on the same mountain-side, but each adhering to its own peculiar pasturage. When the hunters come upon the ground to shoot, the rooi rhebok immediately fly from their lower slopes to the higher ground of their grey brethren, and the two species are seen galloping in close company over the mountain heights. If the hunter rests quietly after his shot and looks about him, he will presently see the two kinds of antelope, as soon as they think they may safely do so, separating, the rooi rhebok quitting the 'vaal' pastures, and betaking themselves again to their own feeding-grounds. To this habit they invariably adhere, and will not delay their departure an instant longer than their safety admits of. If the vaal rhebok in turn are driven out of their own ground, they pursue exactly the same tactics, and will on no account remain for long in their red brethren's territory."

The occurrence of so many trimorphic and polymorphic varieties of the same species have always been a puzzle to Darwinists, as the numerous varieties which the Darwinian theory postulates would all be killed off by natural selection, except the "fit"; but according to the theory which I have advanced, most variations would find their appropriate environments and live. If this theory of topical selection be correct, its application to the phenomena of mimicry is obvious. We have only to suppose that one animal may find safety in associating with another animal to which it has some resemblance, without invoking the aid of either mimicry or natural selection.

I shall not attempt to reply to the other remarks of your critic further than this, that no one who contents himself with reading Dr. Wallace's review will be able to form the slightest idea of the views put forth in my book. That it has taken a lifetime, as Dr. Wallace correctly enough says it has, to build up "the vast edifice" of Darwinism is surely no guarantee of the truth of that system, and certainly no reason why it should be above criticism, as my reviewer seems to think it should be.

Melbourne, 1891. DAVID SYME.

MR. SYME now says: "The references to Darwin in my book are absolutely correct," and—"In every reference to Darwin's views I gave the page and the edition from which the quotation was taken." Assertions, however, are not proofs; but if Mr. Syme will point out where Darwin defines natural selection as "the struggle for existence," and where Darwin "insists that variations are created by natural selection," statements which occur at p. 8 and p. 15 of Mr. Syme's book, I will acknowledge that I have misrepresented him. Otherwise I see nothing that requires modification in my article. But as Mr. Syme claims to have taken "the utmost pains" to quote Darwin's exact words, I will refer to other cases. At p. 12 he says, "The second assumption is that favourably modified individuals should be few in number, 'two or more';" and for this he refers to "Plants and Animals under Domestication," vol. ii. p. 7. The true reference is to vol. i. p. 7, where Darwin says: "Now, if we suppose a species to produce two or more varieties, and these in course of time to produce other varieties, &c." Here we see that Mr. Syme puts "individuals" in the place of "varieties," and thus makes Darwin appear to say the exact reverse of his main contention, which is, that ordinary variability occurring in large numbers of individuals, not single sports, are the effective agents in the modification of species.

Again, at p. 102, Mr. Syme says, when discussing cross-fertilization and variability: "No doubt self-fertilization is a great factor in producing uniformity of colour. That this uniformity is not due to the plants having been 'subjected to somewhat diversified conditions,' as Darwin intimates, is shown by the fact, &c." But Darwin, as every student knows, said exactly the reverse of this—that the somewhat diversified conditions produced *variability*; and Mr. Syme's great efforts to understand him and to quote him correctly again fail of success.

One more example is to be found at p. 110, where he says: "Darwin has distinctly laid down the principle that if it can be proved, by a single instance, that one organism exists for the benefit of another organism, his whole system would fall to the ground." But the statement made by Darwin was, that if any part of the structure of one species could be proved to have been formed for the *exclusive* good of another species it would annihilate his theory ("Origin," 6th edition, p. 162). Mr. Syme omits the essential word "exclusively," and thus appears to have a strong case against the theory.

As an example of general misrepresentation, I will refer to p. 86, where Mr. Syme states that "the Darwinist" "carefully ignores the facts which point in the opposite direction" (of the necessity for insect fertilization of flowers); and on the next page, after referring to cleistogamic and other self-fertilized flowers, he asks: "Why does the Darwinist omit mention of such structures as these?" But he does not refer us to the Darwinists in question who, while discussing insect fertilization, "carefully ignore" self-fertilization; and as his statement will be taken to include all, or at least the majority of Darwinists, it must be held, by those who are acquainted with the facts, to be a very absurd misrepresentation.

Other examples might be given, but these are sufficient to support my statement that Mr. Syme has both misquoted and misrepresented Darwin.

The exposition of his theory of "topical selection" to explain the phenomena of mimicry, as given above, may be left to the judgment of the readers of NATURE.

ALFRED R. WALLACE.

PROF. PICTET'S LABORATORY AT BERLIN.

IT has often been remarked that purely scientific research frequently bears fruit of practical value. A fresh illustration of this fact is afforded by the work of Prof. Pictet, the eminent man of science of Geneva, who is turning to practical account the apparatus by which, in 1877, he first reduced hydrogen and oxygen to the liquid state. At Berlin, where he now resides, he has established, on the scale of a small factory, what he terms a "laboratoire à basses températures." The following account of the work carried on and the results obtained is taken from papers read by the Professor before different scientific Societies of Berlin.

The refrigerating machinery, driven by several powerful

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steam-engines, is intended to withdraw heat from the objects under observation, and to keep them at any temperature between $-20°$ and $-200°$ C. as long as may be required. Prof. Pictet's experience has led him to the conclusion that among the refrigerating agents known, such as rarefaction of gases, dissolution of salts, evaporation of liquids, the latter is to be preferred. A long course of research has further enabled him to choose the most convenient from amongst the great number of suitable liquids. In order to avoid the great pressure required in handling the highly evaporative substances of lowest boiling-point which serve to produce extreme cold, it is necessary to divide the difference of temperature into several stages. Each stage is fitted with especial apparatus consisting of an air-pump worked by steam, which drains off the vapours of the liquid from the refrigerator, and forces them into a condenser, whence, reduced to the liquid state, they are again offered for evaporation in the refrigerator. Thus the liquid, without any loss beyond leakage, passes through a continuous circuit, and the operations can be carried on for any length of time. The liquid made use of for the first stage is the mixture of sulphurous acid and a small percentage of carbonic acid called "*liquide Pictet*." It is condensed at a pressure of about two atmospheres in a spiral tube merely cooled by running water. Oxide of nitrogen (laughing gas) is the liquid chosen for the second stage. Its vapours are condensed in the same way at a pressure about five or six times as great in a tube maintained at about $-80°$ by the action of the first circuit. As medium for a third stage, in which, however, continuous circulation has not yet been attempted, atmospheric air is employed, which passes into the liquid state at a pressure of no more than about 75 atmospheres, provided the condenser is kept at $-135°$ by the first two circuits. The evaporation of the liquefied air causes the thermometer to fall below $-200°$.

By this combination quite new conditions for investigating the properties of matter are realized. In various branches of science new and surprising facts have already been brought to light. Many laws and observations will have to be re-examined and altered with regard to changes at an extremely low temperature.

For instance, a remarkable difference was noted in the radiation of heat. Material considered a non-conductor of heat does not appear to affect much the passage of heat into a body cooled down to below $-100°$. Or, to state the fact according to Prof. Pictet's view: "The slow oscillations of matter, which constitute the lowest degrees of heat, pass more readily through the obstruction of a so-called non-conductor than those corresponding to a higher temperature, just as the less intense undulations of the red light are better able to penetrate clouds of dust or vapour than those of the blue." If the natural rise of temperature in the refrigerator, starting from $-135°$, is noted in a tracing, and afterwards the same refrigerator carefully packed in a covering of cotton-wool of more than half a yard in thickness, and cooled down afresh, and the rise of temperature again marked, on comparing the tracings hardly any difference will be found in the two curves up to $-100°$, and only a very slight deviation even up to $-50°$. On this ground it is clear that the utmost limit of cold that can possibly be attained is not much lower than that reached in the famous experiment of liquefaction of hydrogen. The quantity of warmth which hourly floods a cylinder 1250 mm. high by 210 mm. wide (the size of the refrigerator) at $-80°$, is no less than 600 calories, and no packing will keep it out. At a lower temperature, the radiation being even greater, the power of the machinery intended to draw off still more heat would have to be enormous. And as $-273°$ is absolute zero, the utmost Prof. Pictet judges to be attainable is about $-255°$.

As an example of the surprising methods which the refrigerating machine permits the investigator to employ, it may be mentioned that, in order to measure the elasticity of mercury, Prof. Paalzow had the metal cast into the shape of a tuning-fork, and frozen hard enough for the purpose in view. On this occasion it appeared that quicksilver can be shown in a crystallized state, the crystals being of a beautiful fern-like appearance.

Glycerine was likewise made to crystallize; and cognac, after having been frozen, was found to possess that peculiar mellowness commonly only attained by long keeping.

But the most important application of the refrigerating machinery has been the purification of chloroform, undertaken by Prof. Pictet at the instance of Prof. Liebreich, of the Pharmacological Institute, Berlin. Chloroform has hitherto been considered a most unstable and easily defiled substance. The action of sunlight, the slight impurities retained from the different processes of manufacture, perhaps the mere settling down during protracted storage, have invariably resulted in a more or less marked decomposition. By the simple process of crystallization this unstableness is got rid of, and a practically unchangeable liquid is produced. The crystals begin to form at $-68°$, first covering the bottom of the vessel, and gradually filling it up to within one-fifth of the whole volume. This residue being drained off, the frozen part is allowed to melt under cover, so as to exclude the atmospheric moisture. Chloroform thus refined has, by way of testing its durability, remained exposed on the roof in a light brown bottle from November till June without the slightest sign of decomposition.

Prof. Pictet has already taken steps to introduce his process into manufacture, and proposes to apply the principle to various other chemical and technical objects. Sulphurous ether, for instance, has by a similar process been produced in a hitherto unknown degree of purity. At the same time, the Professor continues eagerly to pursue the various purely scientific inquiries with which he started.

R. DU BOIS-REYMOND.

RESULTS OF EXPERIMENTS AT ROTHAMSTED ON THE QUESTION OF THE FIXATION OF FREE NITROGEN.[1]

FROM the results of the experiments of Boussingault, and also of those made at Rothamsted under conditions of sterilization and inclosure more than thirty years ago, Sir J. B. Lawes and the author had always concluded that at any rate our agricultural plants did not assimilate free nitrogen. They had also abundant evidence that the Papilionaceæ, as well as other plants, derived much nitrogen from the combined nitrogen in the soil and sub-soil. Still, they had long recognized that the source of the whole of the nitrogen of the Papilionaceæ was not explained; that there was, in fact, "*a missing link*"! They were, therefore, prepared to recognize the importance of the results first announced by Prof. Hellriegel in 1886; and they had hoped to commence experiments on the subject in 1887, but they had not been able to do so until 1888. Those first results showed a considerable formation of nodules on the roots, and coincidently great gain of nitrogen, in plants grown in sand (with the plant-ash) when it was microbe-seeded by a turbid watery extract of a rich soil.

In 1889 and since, they had made a more extended series. The plants were grown in pots in a glass-house. There were four pots of each description of plant, one with sterilized sand and the plant-ash, two with the same sand and ash, but microbe-seeded with watery extract, for some plants from a rich garden soil, for lupins from a sandy soil in which lupins were growing luxuriantly, and

[1] Abstract of a paper read before the Agricultural Chemistry Section of the Naturforscher Versammlung at Halle a.S., by Dr. J. H. Gilbert, F.R.S., September 24, 1891.

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for some other plants from soil where the particular plant was growing. In all, in 1889 and subsequently, they had grown in this way four descriptions of annual plants —namely, peas, beans, vetches, and yellow lupins; and four descriptions of longer life—namely, white clover, red clover, sainfoin, and lucerne. Enlarged photographs of the above ground-growth, and of the roots, of the peas, the vetches, and the lupins, so grown, were exhibited. Without microbe-seeding there was neither nodule-formation nor any gain of nitrogen; but with microbe-seeding there was nodule-formation, and, coincidently, considerable gain of nitrogen.

As, however, in this exact quantitative series, the plants were not taken up until they were nearly ripe, it was obvious that the roots and their nodules could not be examined during growth, but only at the conclusion, when it was to be supposed that the contents of the nodules would be to a great extent exhausted. Another series was, therefore, undertaken, in which the same four annuals, and the same four plants of longer life, were grown in specially made pits, so arranged that some of the plants of each description could be taken up, and their roots and nodules studied, at successive periods of growth: the annuals at three periods—namely, first when active vegetation was well established, secondly when it was supposed that the point of maximum accumulation had been approximately reached, and thirdly when nearly ripe; and the plants of longer life at four periods—namely, at the end of the first year, and in the second year when active vegetation was re-established, when the point of maximum accumulation had been reached, and lastly when the seed was nearly ripe. Each of the eight descriptions of plant was grown in sand (with the plant-ash), watered with the extract from a rich soil; also in a mixture of two parts rich garden soil and one part of sand. In the sand the infection was comparatively local and limited, but some of the nodules developed to a great size on the roots of the weak plants so grown. In the rich soil the infection was much more general over the whole area of the roots, the nodules were much more numerous, but generally very much smaller. Eventually the nodules were picked off the roots, counted, weighed, and the dry substance and the nitrogen in them determined.

Taking the peas as typical of the annuals, and the sainfoin of the plants of longer life, the general result was, that at the third period of growth of the peas in sand the amount of dry matter of the nodules was very much diminished, the percentage of nitrogen in the dry matter was very much reduced, and the actual quantity of nitrogen remaining in the total nodules was also very much reduced. In fact the nitrogen of the nodules was almost exhausted. The peas grown in rich soil, however, maintained much more vegetative activity at the conclusion, and showed a very great increase in the number of nodules from the first to the third period; and with this there was also much more dry substance, and even a greater actual quantity of nitrogen, in the total nodules at the conclusion. Still, as in the peas grown in sand, the percentage of nitrogen in the dry substance of the nodules was very much reduced at the conclusion. In the case of the plant of longer life, the sainfoin, there was, both in sand and in soil, very great increase in the number of nodules, and in the actual amount of dry substance and of nitrogen in them, as the growth progressed. The percentage of nitrogen in the dry substance of the nodules also showed, even in the sand, comparatively little reduction, and in soil even an increase. In fact, separate analyses of nodules of different character, or in different conditions, showed that whilst some were more or less exhausted and contained a less percentage of nitrogen, others contained a high percentage, and were doubtless new and active. Thus, the results pointed to the interesting conclusion, that, in the case of the annual, when the seed is formed, and the plant more or less exhausted, both the actual amount of nitrogen in the nodules, and its percentage in the dry substance, are greatly reduced, but that, with the plant of longer life, although the earlier formed nodules become exhausted, others are constantly produced, thus providing for future growth.

As to the explanation of the fixation of free nitrogen, the facts at command did not favour the conclusion that under the influence of the symbiosis the higher plant itself was enabled to fix the free nitrogen of the air by its leaves. Nor did the evidence point to the conclusion that the nodule-bacteria became distributed through the soil and there fixed free nitrogen, the compounds of nitrogen so produced being taken up by the higher plant. It seemed more consistent, both with experimental results and with general ideas, to suppose that the nodule-bacteria fixed free nitrogen within the plant, and that the higher plant absorbed the nitrogenous compounds produced. In other words, there was no evidence that the chlorophyllous plant itself fixed free nitrogen, or that the fixation takes place within the soil, but it was more probable that the lower organisms fix the free nitrogen. If this should eventually be established, we have to recognize a new power of living organisms—that of assimilating an elementary substance. But this would only be an extension of the fact that lower organisms are capable of performing assimilation-work which the higher cannot accomplish; whilst it would be a further instance of lower organisms serving the higher. Finally, it may here be observed that Loew has suggested that the vegetable cell, with its active protoplasm, if in an alkaline condition, might fix free nitrogen, with the formation of ammonium nitrite. Without passing any judgment on this point, it may be stated that it has frequently been found at Rothamsted that the contents of the nodules have a weak alkaline reaction when in apparently an active condition—that is, whilst still flesh-red and glistening.

As to the importance of the fixation for agriculture, and for vegetation generally, there is also much yet to learn. It is obvious that different Papilionaceæ growing under the same external conditions manifest very different susceptibility to, or power to take advantage of, the symbiosis. The fact, as shown by Prof. Nobbe, that Papilionaceous shrubs and trees, as well as herbaceous plants, are susceptible to the symbiosis, and under its influence may gain much nitrogen, is of interest from a scientific point of view as serving to explain the source of some of the combined nitrogen accumulated through ages on the surface of the globe; and also from a practical point of view, since, especially in tropical countries, such plants yield many important food materials, as well as other industrial products.

In conclusion, it will be seen that the experimental results which have been brought forward constitute only a small proportion of those already obtained or yet to be obtained at Rothamsted, but they have been selected as being to a great extent typical, and illustrative of the lines of investigation which are being carried out.

FOSSIL BIRDS IN THE BRITISH MUSEUM.[1]

IT is always a matter of extreme interest to trace back any group of beings to their first recorded appearance in geological history, and the task becomes the more attractive in proportion to the rarity of the organism sought for.

The fossil remains of birds, which form the subject of Mr. Lydekker's Catalogue, constitute nearly the smallest

[1] "Catalogue of the Fossil Birds in the British Museum (Natural History), Cromwell Road, S.W." By Richard Lydekker, B.A., F.G.S., F.Z.S. Pp. xxviii. and 368. (London: Printed by Order of the Trustees. Longmans and Co.; B. Quaritch; Asher and Co.; Kegan Paul and Co., 1891.)

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group of vertebrate fossils known; indeed, it is only within the last thirty-five years that any considerable number of species had been recorded.

That the existence of birds at the period of the Secondary rocks should have been first intimated by their *foot-prints* may seem strange; but as far back as 1835 a notice appeared in *Silliman's American Journal of Science* stating that Dr. Deane had discovered *impressions resembling the feet of birds* upon some slabs of Triassic sandstone from Connecticut. Dr. Hitchcock, who was the first to submit these tracks to careful scientific examination, concluded that they had been produced by the feet of birds which must have been at least four times larger than an ostrich. The great size of some of these foot-prints, however, presented at the time an obstacle to their acceptance, notwithstanding the fact of their exhibiting the same characteristic number of toe-joints as exist in the feet of living tridactylous birds—namely, three phalangeal bones for the inner toe, four for the middle, and five for the outer one.

The subsequent discovery of the entire skeletons of great wingless birds in New Zealand has, to some extent, destroyed the force of this objection as to their size; nevertheless, it seems more probable that these impressions were made by some of those gigantic Dinosaurs whose remains have been in later years met with in such abundance in the Secondary rocks of the American continent, many of which were bipedal in their method of progression, their fore-limbs being exceedingly short, and but ill adapted for use in walking. The hind-foot in *Iguanodon* and in some others was tridactylous, and agreed in the number of toe-bones with the foot of the *Dinornis* and other flightless birds. But between the discovery of the reputed foot-prints of birds in the Connecticut Valley sandstones, and the finding of true bird-remains in Secondary rocks, a long interval of time has elapsed. Some supposed bird-bones from the Chalk of Burham, near Maidstone, were figured and described as long ago as 1845 by Dr. Bowerbank, under the name of *Cimoliornis*, but these proved to belong to a gigantic Pterodactyle, and not to an albatross. The same fate befell Dr. Mantell's Wealden bird (*Palæornis cliftii*, 1844), now also transferred to the Ornithosauria by Mr. Lydekker.

Passing over some fragmentary remains, discovered in 1858 by Mr. Lucas Barrett in the Greensand of Cambridge, referred to birds, we come in 1861 to the discovery, announced by Dr. H. von Meyer, of the impression of a single feather upon a slab of lithographic stone from Solenhofen, Bavaria, followed in 1862 by the description by Prof. Owen of the skeleton of a remarkable long-tailed bird from the same formation and locality, the *Archæopteryx macrura*. This, which is still the earliest-known avian fossil, is also the most generalized bird known; and the discovery, twenty years later, of a second example only serves to confirm the correctness of the conclusions which had been arrived at from a study of the first-found example.

That it was clothed in feathers serves to prove the true avian character of the fossil, no reptile having been met with possessed of such epidermal structures. The remarkable features are that the jaws were armed with conical enamelled teeth implanted in distinct alveoli (see Fig. 1); the three metacarpals in the manus are separate and the phalanges are free (not anchylosed, as in modern birds), and each of the three digits was armed with a terminal claw; the centra of the vertebræ are amphicœlous; there are twenty free vertebræ in the tail, which is longer than the body, each vertebra bearing a pair of feathers, and the tail does not terminate in a pygostyle, like most modern birds.

From these, and other anatomical characters, *Archæopteryx* has been placed in a distinct order, the SAURURÆ, or lizard-tailed birds.

The next important bird discoveries from the Secondary rocks were those made in North America by Prof. O. C. Marsh, in 1870, from the Upper Cretaceous strata of Kansas, U.S., by which we became acquainted with two most distinct and important types, the *Hesperornis* and the *Ichthyornis*. Both of these birds are remarkable as having their jaws armed with teeth. The former (*Hesperornis*) had the teeth implanted in grooves, it had only rudimentary wings, a flat keel-less sternum, and saddle-shaped vertebræ. It was a huge fish-eating diver, nearly 6 feet high, probably resembling in appearance the loons and grebes (see Fig. 2).

The latter (*Ichthyornis*) was a bird of powerful flight, having well-developed wings and a strongly-keeled sternum; its jaws were armed with teeth in distinct sockets, and the vertebræ were biconcave (see Fig. 3).

By far the greater proportion of avian remains known are of Tertiary age; many are referable to existing birds, but a few of them are of almost as great interest to the ornithologist as those already referred to, either as representing, like them, extinct forms, or because they tell of important changes during Tertiary times in the geographical distribution of many genera of birds. The oldest of these remains have been obtained from the London Clay. A single skull of a large ostrich-like bird was obtained from the Lower Eocene of the Isle of Sheppey, and described by Owen in 1869 under the name of *Dasornis londiniensis*. Two limb-bones of a bird as large as an ostrich, but more robust, and with affinities to the Anserine type, as well as to the Ratitæ, were obtained about six years ago from the Lower Eocene near Croydon, and described by Mr. Newton under the name of *Gastornis klaasseni*. Two other species of *Gastornis* had previously been described from the Eocene of Meudon and Rheims, in France, so that the Ratitæ were doubtless well represented in Western Europe in Tertiary times.

Another remarkable discovery in the London Clay of Sheppey is that of the *Odontopteryx toliapicus*, a bird with a powerfully serrated bill, well adapted for seizing fish, which probably formed its prey.

The interest attaching to the discovery, fifty years ago, of the bones of extinct ostrich-like birds in New Zealand, remains unabated; their former abundance may be imagined from the fact that there is hardly a museum in the world where remains of the "moa" are not to be found, and they still continue to be sent to Europe for sale. The series of skeletons of *Dinornis* set up in the Vienna Museum is even finer than that in the British Museum. In the latter, six almost complete skeletons may be seen, beside an immense series of detached bones (see Fig. 4). The tallest skeleton is probably 10 feet, and the smallest 4 feet in height. Specimens showing the skin and feathers still attached to the bones are also preserved, evidencing the comparatively modern date at which they were exterminated.

Another island, which possessed a now extinct flightless bird, is Madagascar. Bones and eggs of this great bird, the *Æpyornis*, which probably rivalled the *Dinornis* in size, are preserved in the British Museum; but, owing to the lack of exploration in the island, we know as yet of only a few odd bones, where entire skeletons doubtless exist, perhaps as abundantly as in New Zealand. The egg of *Æpyornis* is the largest bird's egg known, its liquid contents being rather more than two gallons.

The close affinity existing between birds and reptiles has long ago been an accepted fact in zoology; the finding, therefore, of such primitive birds as *Archæopteryx*, *Hesperornis*, and *Ichthyornis* on the one hand, and of the numerous bird-like Dinosaurs in Europe and America on the other—indeed, the whole tendency of this branch of modern palæontological discovery—has been to strengthen the relationship of the two, and to confirm their association in one primary group of the Vertebrata, the SAUROPSIDA.

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FIG. 1.

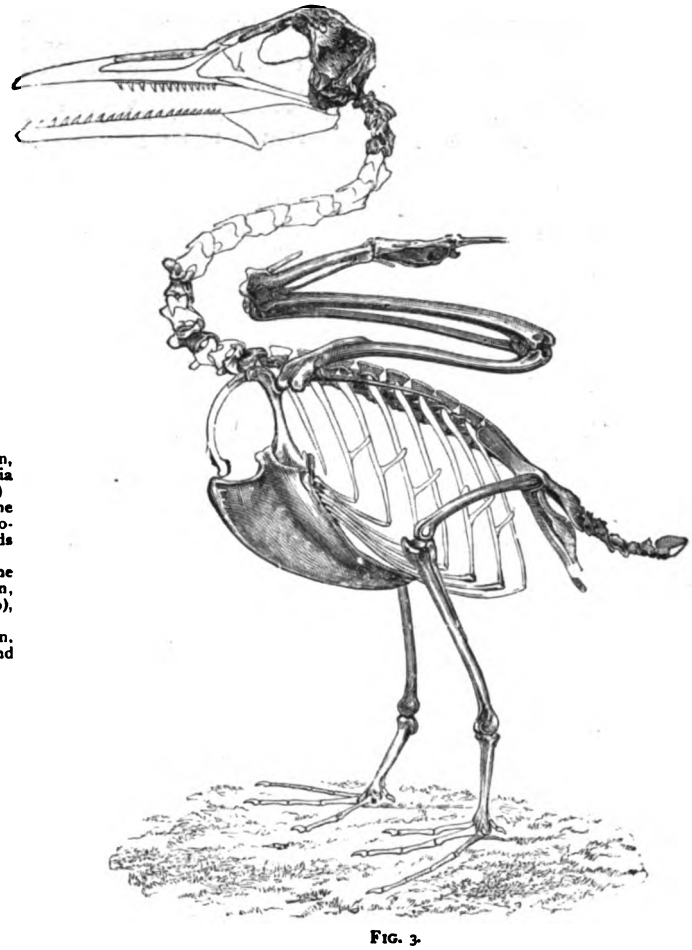

FIG. 3.

FIG. 1.—Right lateral aspect of the skull of *Archæopteryx macrura*, Owen, from the Lithographic stone, Lower Kimmeridgian, Solenhofen, Bavaria (½). (After Dames, "Palæontologische Abhandlungen," vol. ii., 1884.)

FIG. 2.—Restored skeleton of *Hesperornis regalis*, Marsh (1870), from the Cretaceous of Kansas, North America (about ⅛ natural size). (Reproduced, by permission, from Prof. O. C. Marsh's "Extinct Toothed Birds of North America" (folio), New Haven, Conn., U.S., 1880.)

FIG. 3.—Restored skeleton of *Ichthyornis victor*, Marsh (1872), from the Cretaceous of Kansas (⅛ natural size). (Reproduced, by permission, from Prof. Marsh's "Extinct Toothed Birds of North America" (folio), New Haven, Conn., U.S., 1880.)

FIG. 4.—Restored skeleton of *Dinornis* (*Pachyornis*) *elephantopus*, Owen, from Pleistocene deposits, Oamaru Point, South Island, New Zealand (about $\frac{1}{15}$ natural size). (Original in British Museum, N.H.)

FIG. 2.

FIG. 4.

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One very interesting point we may note with regard to the class Aves—namely, that while birds still possessed the teeth which they had inherited from their reptilian ancestors, two very remarkable and distinct types of the class had already made their appearance, and that these two types have persisted on, even to the present day, dividing the class into *Ratitæ* and *Carinatæ*. The characters of the ancient toothed birds indicate undoubtedly a great antiquity for the class, which was probably evolved from the Reptilia in Triassic times, or even earlier.

Although the majority of entries in Mr. Lydekker's Catalogue relate to the *Carinatæ*, the *Ratitæ* are also well represented in the collection, and there is a sufficient number of remarkable extinct forms and figured types to impart to this volume a high scientific interest.

In conclusion, we must express our thanks to Mr. Lydekker for this last contribution to the very useful series of Catalogues which he has prepared for the Trustees of the British Museum, which cannot fail to prove of great service to biological science.

IRON CARBONYL FROM WATER GAS.

AT the meeting of the Chemical Society on Thursday last, November 5, a communication was made by Sir Henry Roscoe, M.P., in the joint names of himself and Mr. Scudder, concerning a new and highly interesting mode of formation of iron carbonyl, $Fe(CO)_4$, the volatile compound of iron and carbon monoxide independently obtained a few months ago by M. Berthelot and by Messrs. Mond and Quincke. During the course of experiments upon the application of water-gas, which contains about 40 per cent. of carbon monoxide and an approximately equal quantity of hydrogen, to the purposes of illumination, it was noticed that the magnesia combs placed over the flame of the burning water-gas rapidly became coated with oxide of iron, which materially lessened the illuminating power. Steatite burners were likewise found to become stained with oxide of iron. The deposit, when allowed to accumulate, took a coralloid tuberous form quite different from accumulations of particles mechanically carried in a stream of gas This led to the supposition that the iron had existed in the water-gas in a volatile form, and was deposited as the result of the decomposition of the volatile compound at the high temperature of the flame. Further experiments were subsequently made with water-gas which had been compressed to eight atmospheres in iron cylinders. After standing for a week in such a cylinder, the gas, which usually burns with a blue non-luminous flame, was found to burn with an intensely yellow flame, and the illuminating power when the magnesia comb was placed over the flame was considerably reduced, owing to the deposition upon the comb of large quantities of oxide of iron. The experiment was repeated before the Fellows of the Society present, and upon depressing the lid of a porcelain crucible upon the flame a black stain was immediately produced, due to the deposition of particles of metallic iron or oxide. Moreover, upon heating the glass tube through which the gas was passing upon its way to the burner, a black mirror of metallic iron was rapidly formed. A thick deposit was also formed upon a plug of cotton-wool inserted in the tube between the heated portion and the burner. A similar tube was exhibited, through which, while heated, one cubic foot of water-gas had been allowed to pass from a cylinder in which it had been stored two weeks; the deposit was strikingly large, both in the portion which had been heated and upon the cotton-wool. After allowing a similar cylinder containing compressed water-gas to stand for five weeks, the flame was found to be smoky, from the large amount of iron liberated during the combustion. The smokiness, and, indeed, the whole luminosity, disappeared upon heating the tube, the gas burning with its ordinary blue flame; a thick mirror was at once deposited, and a large amount of iron retained by the cotton-wool. Thirty litres of gas from this cylinder, burnt during the space of half an hour, gave thirty-two milligrams of metallic iron in the form of a mirror, and forty milligrams were deposited upon the cotton-wool. Upon passing the gas through a U-tube surrounded by ice, a few drops of a turbid liquid were obtained, consisting mainly of iron carbonyl, possessing the properties ascribed to it at the meeting of the British Association at Cardiff by Mr. Mond. The turbidity entirely disappeared upon the addition of hydrochloric acid. From the above experiments it is evident that iron carbonyl is produced in the cold by the action of the carbon monoxide contained in the water-gas upon the iron of the containing cylinder, for the greater the length of time during which it has been stored, the greater is the amount of the compound present It is interesting to learn that the same deposit of metallic iron or oxide is found upon steatite burners from which ordinary coal-gas is burnt, pointing to the existence of iron carbonyl in our common illuminating gas. This conclusion is strengthened by the fact recorded by Dr. Thorne, that coal-gas which has been compressed in iron cylinders and allowed to stand some time is rendered unfit for use for lantern projection, owing to the deep stain of iron formed upon the lime cylinders. It is also interesting, in view of the fact that iron carbonyl is capable of formation in the cold, to note that the nickel compound, $Ni(CO)_4$, described by Messrs. Mond, Langer, and Quincke last year (*vide* NATURE, vol. xlii. p. 370), is also readily formed in the cold, provided the metallic nickel has been previously heated in a current of hydrogen. A. E. TUTTON.

CAPE GUARDAFUI AND THE NEIGHBOURING SEA.[1]

THIS work consists of monthly charts which illustrate the sea surface temperature, the wind, ocean currents, sea disturbance, and weather in the immediate vicinity of Cape Guardafui, extending down the Somali coast so as to include Ras Hafún, and covering the sea to 53° E. Some years ago the Admiralty issued a "Notice to Mariners," indicating the precautions necessary in rounding Cape Guardafui from the southward, in consequence of the Committee of Lloyd's having drawn attention, through the Board of Trade, to the large number of wrecks which had taken place in the neighbourhood. It was pointed out that the wrecks occurred chiefly during the period of the south-west monsoon, which blows from April to September, when the weather on the African coast is stormy and accompanied by a heavy sea; the currents are strong, and the land is generally obscured by a thick haze. The principal recommendation adopted by the Admiralty was the necessity for every precaution in verifying the vessel's position by soundings; and with this precaution it is asserted that the vessel's safety is assured, as the water rapidly deepens northward of the parallel of the cape. Ignorant of the exact position, many seamen have mistaken the high land at the back of Ras Jard Hafún, ten miles south of Cape Guardafui, for the latter, which, being lower and lighter in colour, is often invisible at any considerable distance. Believing the cape to be passed, ships have been steered into the comparatively low bay between the two headlands, and have struck on the sandy beach before any warning has

[1] "Meteorological Charts of the Portion of the Indian Ocean adjacent to Cape Guardafui and Ras Hafún." (London: Published by the authority of the Meteorological Council, 1891.)

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been given. An idea was mooted that a change in the sea temperature could be trusted to indicate the position of the ship in latitude, and some experienced captains in the mercantile marine advocated warmly this test, holding that a sea temperature of 80° F. was never found at this season south of Cape Guardafui. The attention of the Meteorological Office was called to these statements, and it was evident that an investigation into the facts would be of great service to the mariner. A preliminary inquiry threw doubt on the view in question, though it was apparent that the temperature was, generally speaking, lower to the south of Cape Guardafui than to the north. The charts now published are the outcome of the inquiry. So far as the practical bearing of the investigation on navigation is concerned, the result, in brief, is that in every month of the year a sea surface temperature above 80° may be found to the southward of Cape Guardafui; and that, although in the months of June, July, and August, when the south-west monsoon is at its height, this occurrence is rarer than at other seasons, the thermometer would prove a very dangerous guide for the purpose suggested.

The primary object of the discussion undertaken by the Meteorological Office was to show the difference of sea surface temperature near Cape Guardafui in comparison with that over the sea to the southward during the south-west monsoon months, from April to September, but more especially in the months of June, July, and August, when the monsoon is most pronounced. In spite of this being the period of the northern summer, the surface water is coldest at this season, and from June to September are the only months during the year that temperatures below 70° are experienced within the area dealt with. It is clear that during the full strength of the south-west monsoon the cold water of the southern hemisphere is driven north of the equator; but on the other hand, although low temperatures are experienced, readings of 80° and above are met with in these months at a considerable distance to the southward of Cape Guardafui; and for a vessel, making a passage from the southward, to reason that she had passed Cape Guardafui because the thermometer indicated a temperature of 80° would be altogether misleading. The temperatures are without doubt more uniformly high in the vicinity of Cape Guardafui than further to the southward during the months of June to September, and this justifies to a very great extent the opinion formed by many leading captains of the merchant service that a safe course might be shaped by the thermometer; but this view is now proved to be erroneous. The sea surface temperature reaches its highest point in the district discussed during the months of March, April, and May, when nearly the whole area is above 80°.

The winds and ocean currents, which are plotted in position on the charts, give features of especial interest. The change of monsoon is well shown, and the effect produced by the adjacent land on the direction of the wind, also the variations in the strength of the monsoon, especially the intensified force of the south-west wind, which reaches its maximum in July, when the winds frequently blow with the force of a whole gale. The direction during the south-west monsoon is generally more southerly near the land than over the open sea. The surface current during the south-west monsoon almost invariably sets off the land to the eastward and north-eastward, and it sometimes attains the velocity of 80 to 100 miles in the 24 hours. In the north-east monsoon the conditions are generally much quieter, but the monthly charts show interesting and important differences; and the work, embracing, as it does, the whole twelve months, illustrates very fully the changes which occur, and afford very valuable material both for the man of science and the sailor.

NOTES.

THE President and Council of the Royal Society have recommended Prof. Charles Lapworth and Prof. A. W. Rücker for the Royal Medals this year, and the Queen has signified her approval of the award. The other medallists are Prof. Cannizzaro for the Copley Medal, and Prof. Victor Meyer for the Davy Medal.

THE following is the list of names recommended by the President and Council of the Royal Society for election into the Council for the year 1892, at the anniversary meeting on November 30:—President: Sir William Thomson. Treasurer: John Evans. Secretaries: Prof. Michael Foster, Lord Rayleigh. Foreign Secretary: Sir Archibald Geikie. Other Members of the Council: Captain William de Wiveleslie Abney, William Thomas Blanford, Prof. Alexander Crum Brown, Prof. George Carey Foster, James Whitbread Lee Glaisher, Frederick Ducane Godman, John Hopkinson, Prof. George Downing Liveing, Prof. Joseph Norman Lockyer, Prof. Arthur Milnes Marshall, Philip Henry Pye-Smith, William Chandler Roberts-Austen, Prof. Edward Albert Schäfer, Sir George Gabriel Stokes, Prof. Sydney Howard Vines, General James Thomas Walker.

WE are glad to hear of a splendid gift which has just been formally accepted by the Regents of the Smithsonian Institution. It is a gift of 200,000 dollars, which has been presented to the Institution by Mr. Thomas Hodgkins, of Setauket, Long Island. The donation is accompanied with a condition—which, as the New York *Tribune* remarks, "will not be onerous"—that the donor shall have the option of giving another sum of 100,000 dollars within the year. Mr. Hodgkins has arranged that the interest of 100,000 dollars shall be "permanently devoted to the increase and diffusion of more exact knowledge in regard to the nature and properties of atmospheric air."

THE opening meeting of the seventy-fourth session of the Institution of Civil Engineers was held on Tuesday, and was very fully attended. Awards were made for various original communications submitted during the past session, for various papers printed in the Proceedings without being discussed, and for various papers read at the supplemental meetings of students. Mr. George Berkley, the President, delivered an address, taking as his subject the advance of engineering work in relation to social progress.

THE following, briefly stated, are prize subjects recently proposed by the Dutch Academy of Sciences, at Haarlem:—(1) Molecular theory of internal friction of gases departing from Boyle's law, and if possible, of liquids. (2) Determination of the duration of electric vibrations in various conductors. (3) Try inoculation of *Viscum album* on apple, pear, chestnut, and lime trees, and explain its preference for certain species. (4) Criticism of opinions on structure and mode of growth of the cell-wall, having regard to continuity of the protoplasm of the adjacent cells (in some cases). (5) New experiments on the reproductive power of parts of plants, and the polarity observed in it. (6) Study of the low organisms appearing (usually as filaments) in bottles containing solutions of chemical products, after long standing. (7) Significance of peptones for the circulation of nitrogen in plants. (8) Oxidation of ammoniacal salts in the ground, and transformation into nitrates. Do the microbes found by Winogradsky and Frankland exist in the soil of Holland? (9) Researches on the organism concerned in production of marsh gas, or the conditions in which the gas is formed, if life has only an indirect influence on the phenomenon. Liberation of the gas from manure. (10) Study of the

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microbes involved in ensilage of green fodder, and of the variations of sugar and acidity with temperature and time. (11) The development of Triclades. (12) The development of the spleen. The prize offered in each case is a gold medal or a sum of 150 florins. Memoirs may be written in Dutch, French, English, Latin, Italian, or German (not German characters), and they are to be sent in, with sealed packet, to the secretary before January 1, 1893. (Further particulars in the *Revue Scientifique*, October 10, 1891.)

ACCORDING to an official report which reached the Japanese Legation, London, on November 6, the earthquake of October 28 affected the prefectures of Aichi and Gifu. It was calculated that the number of persons killed was 6500, and of persons injured 9000; that 75,000 houses were destroyed, and 12,000 damaged.

IN its review of the weather during October, the U.S. Pilot Chart notes that the latter part of September and nearly the entire month of October were characterized by exceptionally severe weather in the North Atlantic. Tropical hurricanes passed north between Hatteras and Bermuda on October 3, 12, and 18. The heavy weather that prevailed between Newfoundland and the British Channel in the last week of September was followed by comparatively moderate weather during the first two days of October, but a storm that apparently moved eastward in high latitudes on the 2nd and 3rd caused increasing westerly gales in mid-ocean, and the force of these gales was very greatly increased by the formation of a secondary on the 4th, a short distance west of Rockall. This secondary remained central about the same place for three days, the 4th, 5th, and 6th, and during all of this time there was very severe weather almost all the way from the North Sea to the Grand Banks. There were also later storms, and altogether, when the facts are fully known, it will probably be found that the month was one of the most severe on record.

WE take from *Symons's Monthly Meteorological Magazine* for October the following interesting details of the climate of the British Empire during 1890. The tables for the year exhibit some exceptional features. For the first time in the 17 years that the figures have been published, the highest shade-temperature occurred at an East Indian station, 105°·6 at Calcutta, instead of in Australia. The highest sun-temperature was, however, recorded at Adelaide, 163°·9, although it is not exceptional for this to occur at Calcutta, while the mean temperature of the East Indian stations far exceeds that of Australia. The lowest shade-temperature occurred, as usual, at Winnipeg, −39°·4, the extreme rigour of whose winters far exceeds the cold at the other Canadian stations. The greatest range in the year, 135°·9, as well as the greatest mean daily range and the lowest mean temperature, 32°·8, also occurred there; while the least yearly range, 25°·4, and the highest mean temperature, 80°·5, occurred at Colombo, Ceylon. The driest station was Adelaide, mean humidity 62, and the dampest London, mean humidity 80. The greatest rainfall for the places quoted was 82·90 inches at Trinidad, and the least, 19·96 inches at Jamaica. The most cloudy station was Hobart, in Tasmania, and the least cloudy Malta. A large amount of cloud occurs at most insular stations, as well as great humidity, and small range of temperature; and, at one time or another London, Ceylon, Barbados, and Mauritius have recorded the extremes of most of these elements.

AT the distribution of prizes in the Sheffield Technical School last week, Dr. Sorby, the President of the Council of Firth College, spoke vigorously and opportunely on a subject which is likely to become one of increasing importance—the true relation of technical education to the study of pure science. He feared, he said, not that technical education would not succeed, but that the public might forget that there might be something else besides. He hoped that in the efforts that were being made to insure education in everything which was required for the trade of the country they would not forget that there were other things besides that. Some of the greatest discoveries made by Davy, Faraday, and Pasteur, were not made for trade purposes, but in the interests of abstract science. If they did anything to delay the development of science as a whole, they would hinder trade in the long run. Abstract science might sometimes appear at first to be very abstract, but all at once it might turn out to be of the utmost value in connection with trade. He would be very sorry indeed if in the future technical instruction should push other education out of the field altogether. There was a danger of this, because the funds available for education and objects of that kind were limited, and what was devoted to one institution was to some extent taken from others.

THE annual report on the technological examinations of the City and Guilds of London Institute has just been published. It is signed by Mr. G. Matthey, F.R.S., chairman of the examinations committee, and Sir Philip Magnus, superintendent of technological examinations. The facts set forth in the report are, upon the whole, satisfactory. At the recent examination the total number of worked papers was 7416, as against 6781 in 1890, showing an increase of 635, the corresponding increases in the two previous years being 175 and 440. This year, too, there is not only an increase in the number, but also in the proportion of candidates who have succeeded in satisfying the examiners, the number of passes being 4099 as against 3507 in 1890, and the percentage of passes 55·3 as against 51·8. Moreover, the examinations were held this year in 53 as against 49 subjects in the previous year, and in 245 as against 219 different centres throughout the country.

WRITING to the *Times* on the place due to horticulture in technical education, Mr. W. Wilks, honorary secretary of the Royal Horticultural Society, says that that Society is ready to co-operate with the County Councils in any attempt to promote the serious study of the subject. The Society has already entered into arrangements with the County Council of Surrey for holding examinations and awarding certificates, &c., after a series of lectures in various centres; and the County Council of Cambridgeshire is in communication with the Society as to the provision of practical demonstrations of scientific methods applied to orchards, allotments, and cottage gardens. Mr. Wilks is also in correspondence with a gentleman in Somersetshire, with the object of sending round an itinerant instructor and adviser to some of the cider orchard districts of that county.

AT a meeting of the Ashmolean Society in the Museum, Oxford, on Monday, November 9, under the presidency of Prof. Odling, Colonel Swinhoe read a paper on silk-producing moths. The author exhibited specimens of *Bombyx mori* and of their cocoons, showing the changes produced by variation of climate and domestication on members belonging to the Bombycidæ. Several specimens of the tussur silkworm were exhibited, illustrating in some respects the effects of cross-breeding, which, in the opinion of the author, had done much to depreciate the commercial value of the silk produce of India. Much greater care—a care which the Chinese appreciated—was needed on the part of the native breeders of the silkworm to insure in the silk the peculiar qualities which enhance its market value. A discussion followed, in the course of which Prof. Legge described briefly the method of culture of the mulberry-tree in China, and some of the methods employed in winding and securing the silk.

THE Christmas lectures to juveniles, at the Royal Institution, will this year be on "Life in Motion, or the Animal Machine" (experimentally illustrated), and will be delivered by Prof. John G. McKendrick, F.R.S., Professor of Physiology in the University of Glasgow.

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A MAMMALIAN tooth has just been discovered by Mr. Charles Dawson, of Uckfield, in a Wealden bone-bed near Hastings. The fossil much resembles one of the lower molars of *Plagiaulax*, a genus well known from the Purbeck Beds of Swanage. It is the first evidence of a mammal from the Wealden formation, and will be exhibited and described by Mr. Smith Woodward at the next meeting of the Zoological Society, on the 17th inst.

DE CANDOLLE states, in his "Origin of Cultivated Plants," that maize is unknown in the wild condition. Some light may possibly be thrown on the origin of cultivated maize, by the discovery of a wild species, the only one of the genus, in Mexico. It is described by Prof. Sereno Watson, in the "Contributions to American Botany," under the name *Zea nana*.

WE learn from the *Journal of Botany* that the great Index of Genera and Species of Flowering Plants, on which Mr. B. Daydon Jackson has been continuously engaged for nearly ten years, is now ready for the printers' hands, and will be issued by the Clarendon Press, under the title "Index Kewensis nominum omnium plantarum phanerogamarum, 1735–1885." The work has been carefully revised by Sir Joseph Hooker, who, besides annotating the manuscript, has undertaken the care of the geographical distribution.

IF the weather during the approaching winter be as severe as that which we had last winter, many persons will be likely to take some interest in an invention which is attracting notice in America. This is an electric snow-sweeper—a snow-sweeper driven by an electric motor. The *Engineering Magazine*, of New York, says that while the machine is driven along the track of an electric railway by a motor of 30 horse-power, taking its current through the trolly wire, the two sweeping brushes are each driven by an independent motor, and all the three motors are reversible. It is stated that this plough is competent to remove from a track snow having a depth of from 3 to 12 inches, while running at a speed of from 4 to 10 miles an hour. The independent action [of the brush-motors enables them, when necessary, to be run at a high rate of speed while the plough is moved slowly along the track, and thus to cut away hard, compacted snow, or drifts. It is said that the machine was thoroughly tested last winter, and its effectiveness thereby completely demonstrated.

IT is known that ozone can be abundantly produced by the electric silent discharge, and many years ago Siemens devised an "ozone-tube" for the purpose, consisting of two thin glass tubes, one within the other; the inner lined, and the outer coated, with metal, to which alternating currents of high tension are brought, acting on the gas to be ozonized within. From recent experiments in Siemens and Halske's laboratory, it appears that a good result may be had with only one dielectric, and for this not only glass, but mica, celluloid, porcelain, or the like, may be used. Thus the ozone-tube may be arranged with a metallic tube within, and the outer tube a metal-coated dielectric; or the inner metal tube may have a dielectric coat, while a metal tube is the inclosing body. As metals that are little or not at all attacked by ozone, platinum, tin, tinned metals, and aluminium are recommended. Through the inner tube flows cold water, and through the space between the tubes air, dried and freed from carbonic acid. Several such tubes may be combined in a system, and worked with alternate currents (for single tubes the continuous current with commutator is best). An apparatus of this kind is now at work in the laboratory, yielding 2·4 mg. of ozone per second. Experiments are being made in supplying compressed ozone for technical use; and this has been accomplished with a pressure of nine atmospheres. One use of ozone, on which Herr Frölich lays special stress (in the recent lecture from which these data are taken), is the disinfection and sterilization of water. And doubtless with an abundant supply of the substance, the use of it would be greatly extended.

MR. A. CRAWFORD, the manager of the travelling dairy connected with the Department of Agriculture, Victoria, is able to give a very favourable report of the results of the operations of the dairy during the last two years. It has been the means, he says, of very largely improving the general average of both cheese and butter sent to market. A good many pupils have been taught who had never made butter or cheese before, and several of them are now managing factories. In nearly all the places he has visited Mr. Crawford has lectured on dairy farming; and in many cases he has gone to outlying districts as well as to important centres.

AT the recent general meeting of the German Anthropological Society, Prof. Montelius, of Stockholm, delivered two remarkably interesting archæological lectures. In the first he dealt with the chronology of the Neolithic Age, especially in Scandinavia. The monuments of that age, he said, belonged to three different periods. First, were free-standing dolmens without passages; next, passage-graves; finally, stone cists. The last are later in proportion to the completeness with which they are covered by the mounds heaped over them. Behind the periods represented by these classes of monuments was a Neolithic period from which no grave of any kind is known to have survived. It has left traces, however, in its implements, which are of an earlier form than the various sorts found in the different groups of monuments. The Scandinavian forms of Neolithic weapons, implements, and ornaments are closely akin to those which have come down to us in the rest of Europe, especially in North Germany, England, Italy, and even Cyprus. This may be held to prove that there was a more or less active commercial intercourse between Scandinavia and the Continent at a very remote time. To this commercial intercourse Prof. Montelius is disposed to attribute the relatively high civilization of Scandinavia in the Neolithic Age. Prof. Montelius also contends that the various periods of the Neolithic Age in Scandinavia were more nearly contemporaneous with those of other parts of Europe than has hitherto been generally supposed.

IN his second lecture Prof. Montelius treated of the Bronze Age in the East and in Southern Europe. He distinguished the following periods:—(1) A copper period without bronze, represented by the finds of Richter in Cyprus and those obtained by Schliemann from the first city at Hissarlik. (2) The bronze period in the islands of the Ægean Sea, Rhodes, Crete, &c. (3) A later bronze period—(*a*) with shaft tombs at Mycenæ, (*b*) with bee-hive tombs in the neighbourhood of Mycenæ, Orchomenos, &c. These cities had not a pure Hellenic civilization, but must be regarded rather as Oriental colonies. The knowledge of bronze certainly came to Europe from the East; not by way of Siberia and Russia, nor across the Caucasus, but probably through Asia Minor and the Mediterranean to Italy and Spain, whence it rapidly spread to Gaul, Britain, and other countries.

A PENINSULA called Keweenaw Point, jutting into Lake Superior from the southern shore towards the north-east, is famous as the centre of a vast copper-mining industry. Last year the mines produced no less than 105,586,000 pounds of refined copper, and it is estimated that during next year production will be increased by at least 20 per cent. Mr. E. B. Hinsdale, who contributes to the latest Bulletin of the American Geographical Society an article on the subject, has much that is interesting to say about the numerous prehistoric mines which have been found in this region. These ancient mines—judging

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from their extent—must have been worked for centuries. Who the workers were, no one can tell. They seem to have known nothing of the smelting of copper, for there are no traces of molten copper. What they sought were pieces that could be fashioned by cold hammering into useful articles and ornaments. They understood the use of fire in softening the rocks to enable them to break away the rock from the masses of copper. They could not drill, but used the stone hammer freely. More than ten cart-loads of stone hammers were found in the neighbourhood of the Minnesota mine. In one place the excavation was about 50 feet deep, and at the bottom were found timbers forming a scaffolding, and a large sheet of copper was discovered there. In another place, in one of the old pits, was found a mass of copper weighing 46 tons. At another point the excavation was 26 feet deep. In another opening, at the depth of 18 feet, a mass of copper weighing over 6 tons was found, raised about 5 feet from its native bed by the ancients, and secured on oaken props. Every projecting point had been taken off, so that the exposed surface was smooth. Whoever the workers may have been, many centuries must have passed since their mines were abandoned. Their trenches and openings have been filled up, or nearly so. Monstrous trees have grown over their work and fallen to decay, other generations of trees springing up. When the mines were rediscovered, decayed trunks of large trees were lying over the works, while a heavy growth of live timber stood on the ground.

THE last two parts of the *Izvestia* of the Russian Geographical Society (vol. xxvii., 3 and 4) contain M. Grum-Grzimailo's report on his journey to Central Asia, and General Tillo's calculations of the heights determined by the Russian traveller during his journey. The report, which adds little to the information given in the explorer's letters, is accompanied by a map embodying the results of the extensive surveys made by the two brothers in the Eastern Tian Shan, the Hashun Gobi, the Barkul oasis, and the region in the south-east of it, as far as the 36th degree of latitude and the 72nd degree of longitude. It was already known that during this journey the brothers Grum-Grzimailo had discovered, some fifty miles to the south-east of Turfan, a depression situated between the two chains of the Eastern Tian Shan and the Kuruk-tag Mountains, the level of which proved to be very near to the sea-level, or even below it. At the spot, Lukchin-chir, their barometer rose (on October 27) to 771·7 mm. On the same day, the pressure of the atmosphere, reduced to the sea-level, attained 784 mm. at Krasnoyarsk, 787·7 mm. at Yeniseisk, 774 mm. at Irkutsk and Tomsk, and 767 mm. at Przevalsk and Narynsk; so that there may be some doubt as to the pressure in the latitude and longitude of Lukchin-chir (at the sea-level) really being 767 mm., as adopted by General Tillo, which would give for that spot 50 metres below the level of the sea. But the possible error cannot be very great, and we thus have, between the two above-named chains of mountains, an undoubted depression, the surface of which is very near to the level of the ocean.

MR. L. STEJNEGER describes, in the Proceedings of the American National Museum, a new North American lizard of the genus Sauromalus. It is very large, the total length of four specimens averaging 540 millimetres. This enormous lizard is closely allied to the much smaller species which inhabits the arid regions of the mainland to the north of the Gulf of California, viz. *Sauromalus ater*, with which it has been confounded. It may be readily distinguished by the characters given in Mr. Stejneger's diagnosis.

SOME time ago the Educational Museum of Tokyo was abolished, and the collections were transferred to the Science College of the Imperial University. Dr. I. Ijima, Professor of Embryology and Comparative Anatomy, who volunteered to take care of the ornithological collection, offered to send it to Mr. L. Stejneger, of the U.S. National Museum, in instalments for identification and study, and the proposal was gladly accepted. Dr. Stejneger has made some progress with the work, and has just issued "Notes" in which he presents the results of his examination of the first instalment. He has had the satisfaction of finding "quite a number of interesting additions to the Japanese avifauna."

WE have received Nos. 7-9 of vol. i. of "Illustrations of the Flora of Japan, to serve as an Atlas of the Nippon-Shokubutsushi," by Tomitaro Makino, a monthly publication, brought out in Tokyo, apparently somewhat on the plan of the "Icones Plantarum." Each number contains about six plates (uncoloured), with descriptions, of new or remarkable species, natives of Japan. The drawings are exceedingly well done, and the descriptions (in English) would compare favourably, in accuracy and completeness, with those of some works published in this country. The species described appear to be taken at random, those in the same number having no affinity with one another.

MESSRS. BAILLIÈRE, TINDALL, AND COX have issued the fifth edition of the "Manual for the Physiological Laboratory," by V. D. Harris and D'Arcy Power. The work has been enlarged, the increase being due mainly to the more detailed account which has been given, for junior students, of microscopes and their properties; and to the description, for senior students, of the latest methods of histological research. The parts relating to physiological chemistry have been thoroughly revised, and many additional illustrations have been inserted.

MR. JAMES STIRLING, Assistant Government Geologist, Victoria, has published at Melbourne some valuable and interesting notes on the hydrology of the Mitta Mitta. The following are the leading conclusions to which he has been led by his observations:—(1) That there is considerable inequality in the amount of rainfall over different portions of the same watershed area in each of the various streams flowing from the Australian Alps, the Mitta Mitta being cited as an instance of this; and that as the recording stations at present established are all below the normal line of cloud flotation (under 4000 feet), where the rainfall is greater, the actual quantity which falls in the several watershed areas is really greater than that shown in the records. (2) That the low percentage of discharge to rainfall is due in all probability to a complexity of causes, among which may be cited the excessive evaporation in certain areas, largely due to the great range of temperature; the different heat-radiating powers of different rock-masses; and percolation along fault lines, contacts of the igneous and sedimentary formations, &c.; and, in some areas, the absorption by certain species of the prevailing eucalyptus vegetation. (3) To determine the actual quantity of rainfall and the causes affecting its local distribution, it has become necessary to establish meteorological stations at the higher altitudes in the Australian Alps. (4) And in order to supply further trustworthy data, it is, Mr. Stirling thinks, imperative that a system of complete topographical survey should be instituted.

THE Annual Report, for 1888-89, of the Geological and Natural History Survey of Canada has been issued. It forms the fourth volume of the new series, and includes reports and maps of various investigations and surveys. The volume opens with summary reports, by Mr. Alfred R. C. Selwyn, the Director, on the operations of the Geological Survey for the year 1889. Then come the following reports:—On a portion of the west Kootanie district, British Columbia, by G. M. Dawson; an exploration in the Yukon and Mackenzie basins, by R. G. McConnell; exploration of the glacial Lake Agassiz in Manitoba,

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by Warren Upham; the mineral resources of the province of Quebec, by R. W. Ells; the surface geology of Southern New Brunswick, by R. Chambers; chemical contributions to the geology of Canada from the laboratory of the Survey, by G. C. Hoffmann; mining and mineral statistics of Canada, by H. P. Brumell; division of mineral statistics and mines, by E. D. Ingall and H. P. Brumell; annotated list of the minerals occurring in Canada, by G. C. Hoffmann.

THE administration of forests seems to be, on the whole, one of the most satisfactory departments of public activity in India. Dr. Ribbentrop, in his report for the year 1889–90, states that over 4200 square miles were added to the area of forest estates under control, thus bringing the total area up to nearly 105,500 square miles. The gross revenue exceeded 153 lakhs of rupees, giving a surplus over expenditure of nearly 73 lakhs, or an increase in a single year of 15 lakhs. The surplus in 1885 was only 41 lakhs. It is believed that this rate of increase may be maintained, as the rich forests of Upper Burma have still to be opened out.

IN a recent communication from Alta Verapaz, a department in Guatemala (*Met. Zeit.*), Dr. Sapper describes the climate. The position is on the north slope of a hill-range stretching east and west, and the large rainfall (it has a rainy season in winter, as well as that in summer common to the whole of Central America) apparently affects the frequency of earthquakes. For the district is of limestone and dolomite, and honeycombed with caverns and subterranean watercourses, and heavy rains lead to a collapse of such cavities, so that towards the end of the summer rain season, and still more towards that of winter, the number of earthquakes and tremors is distinctly increased. The winter of 1889–90 had unusually heavy rains, and the earthquakes were also unusually numerous (seventeen in 1890 as against five the previous year).

A PAPER upon the sulphides of boron is communicated by M. Paul Sabatier to the September number of the *Bulletin de la Société Chimique*. Hitherto only one compound of boron with sulphur has been known to us, the trisulphide, B_2S_3, and concerning even that our information has been of the most incomplete description. Berzelius obtained this substance in an impure form by heating boron in sulphur vapour, but the first practical mode of its preparation in a state of tolerable purity was that employed by Wöhler and Deville. These chemists prepared it by allowing dry sulphuretted hydrogen gas to stream over amorphous boron heated to redness. Subsequently a method of obtaining boron sulphide was proposed by Fremy, according to which a mixture of boron trioxide, soot, and oil are heated in a stream of the vapour of carbon bisulphide. M. Sabatier finds that the best results are obtained by employing the method of Wöhler and Deville. The reaction between boron and sulphuretted hydrogen only commences at red heat, near the temperature of the softening of glass. When, however, the tube containing the boron becomes raised to the temperature, boron sulphide condenses in the portion of the tube adjacent to the heated portion; at first it is deposited in a state of fusion, and the globules on cooling present an opaline aspect. Further along the tube it is slowly deposited in a porcelain-like form, while further still the sublimate of sulphide takes the form of brilliant acicular crystals. The crystals consis of pure B_2S_3; the vitreous modification, however, is usually contaminated with a little free sulphur. Very fine crystals of the trisulphide may be obtained by heating a quantity of the porcelain-like form to 300° at the bottom of a closed tube whose upper portion is cooled by water. The crystals are violently decomposed by water, yielding a clear solution of boric acid, sulphuretted hydrogen being evolved. On examining the porcelain boat in which the boron had been placed, a non-volatile black substance is found, which appears to consist of a lower sulphide of the composition B_4S. The same substance is obtained when the trisulphide is heated in a current of hydrogen; a portion volatilizes, and is deposited again further along the tube, while the residue fuses, and becomes reduced to the unalterable subsulphide B_4S, sulphuretted hydrogen passing away in the stream of gas.

TWO selenides of boron, B_2Se_3 and B_4Se, corresponding to the above-described sulphides, have also been prepared by M. Sabatier, by heating amorphous boron in a stream of hydrogen selenide, H_2Se. The triselenide is less volatile than the trisulphide, and is pale green in colour. It is energetically decomposed by water, with formation of boric acid and liberation of hydrogen selenide. The liquid rapidly deposits free selenium, owing to the oxidation of the hydrogen selenide retained in solution. Light appears to decompose the triselenide into free selenium and the subselenide B_4Se.

SILICON SELENIDE, $SiSe_2$, has likewise been obtained by M. Sabatier by heating crystalline silicon to redness in a current of hydrogen selenide. It presents the appearance of a fused hard metallic mass incapable of volatilization. Water reacts most vigorously with it, producing silicic acid, and liberating hydrogen selenide. Potash decomposes it with formation of a clear solution, the silica being liberated in a form in which it is readily dissolved by alkalies. Silicon selenide emits a very irritating odour, due to the hydrogen selenide which is formed by its reaction with the moisture of the atmosphere. When heated to redness in the air it becomes converted into silicon dioxide and free selenium.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* ♂) from India, presented by Mr. G. E. Lidiard; two Senegal Touracous (*Corythaix persa*), a Madagascar Porphyrio (*Porphyrio madagascariensis*) from West Africa, presented by Mr. J. B. Elliott; a Blue-fronted Amazon (*Chrysotis æstiva*) from Brazil, presented by Mrs. H. R. Warmington; two Puff Adders (*Vipera arietans*) from South Africa, presented by Messrs. Herbert and Claude Beddington; two Tree Boas (*Corallus hortulanus*) from St. Vincent, W.I., presented by H.E. the Hon. Sir Walter F. Hely Hutchinson, K.C.M.G.; a Tree Boa (*Corallus hortulanus*) from Demerara, presented by Mr. J. J. Quelch, C.M.Z.S.; a Black-headed Lemur (*Lemur brunneus*) from Madagascar, a Brown Capuchin (*Cebus fatuellus*) from South America, a Black-headed Caique (*Caica melanocephala*) from Demerara, a Red and Blue Macaw (*Ara macao*) from Central America, deposited; a Black-headed Caique (*Caica melanocephala*) from Demerara, purchased.

OUR ASTRONOMICAL COLUMN.

OUTBURST OF DARK SPOTS ON JUPITER.—Attention has been called by several observers to a number of dark spots which have appeared lately on the first belt north of the north equatorial belt of Jupiter, in about latitude 20°. Mr. Denning derived a period of rotation of 9h. 49m. 27·2s. from his observations of one of these objects between August 21 and September 15 (*Observatory*, October 1891). A change then occurred, for this spot, and others near it, were found to have a rotation period of 9h. 49m. 44·2s. from September 15 to October 15. This sudden change of 17 seconds in the rate of motion of a region of some extent is most remarkable. A series of photographs of Jupiter were taken at Lick Observatory in August, which, according to Mr. Stanley Williams, "are of such a degree of excellence that an examination of them is almost like looking at the planet itself" (*Observatory*, November 1891). These photographs show six or seven dark spots, and a comparison of them with a sketch made about one rotation later clearly indicates a displacement of the spots with reference to the great red spot,

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owing to the more rapid movement of the belt in which they occur. Prof. E. E. Barnard observed the spots so early as May last (*Astronomische Nachrichten*, No. 3063). He found in September that they were decreasing their longitudes about 10° daily. At this rate they would describe a complete rotation round Jupiter, relative to the great red spot, in about 36 days. The daily loss derived from Mr. Denning's observations in August and September would bring the two spots in conjunction in about 39 days.

WOLF'S PERIODIC COMET.—The following ephemeris is from one given by Dr. Thraen in *Astronomische Nachrichten*, No. 3064 :—

Ephemeris for Berlin Midnight.

1891.	Right Ascension.	Declination.	Brightness.
	h. m. s.	° ′ ″	
Nov. 14 ...	4 35 16 ...	−8 29 28 ...	10·2
,, 17 ...	33 27 ...	9 36 16	
,, 20 ...	31 31 ...	10 36 44 ...	9·3
,, 23 ...	29 31 ...	11 30 38	
,, 26 ...	27 31 ...	12 17 51 ...	8·3
,, 29 ...	25 31 ...	12 58 22	
Dec. 2 ...	23 37 ...	13 32 15 ...	7·4
,, 5 ...	21 48 ...	13 59 40	
,, 8 ...	20 8 ...	14 20 51 ...	6·5
,, 11 ...	18 39 ...	14 36 10	
,, 14 ...	17 22 ...	14 45 59 ...	5·7
,, 17 ...	16 17 ...	14 50 41	
,, 20 ...	15 26 ...	14 50 38 ...	4·9
,, 23 ...	14 49 ...	14 46 14	
,, 26 ...	4 14 27 ...	14 37 52 ...	4·3

Although the comet is getting fainter and moving south, it may probably be followed to the last date in the above ephemeris with instruments of moderate aperture. The greatest southern declination of 14° 51′ 8″ is reached on November 18.

THE TOTAL LUNAR ECLIPSE OF NOVEMBER 15.—If atmospheric circumstances permit, a total eclipse of the moon may be observed over all Europe on Sunday next, November 15. The following are the times of contact with the earth's shadow given in the *Nautical Almanac* :—

	G.M.T. h.	m.
First contact with the penumbra	9	36·7
,, ,, ,, shadow	10	35·1
Beginning of total phase	11	37·4
Middle of eclipse	12	18·9
End of total phase	13	0·4
Last contact with the shadow	14	2·7
,, ,, ,, penumbra	15	1·1

The first contact with the shadow occurs at 55° from the most northern point of the moon's limb counting towards the east, the last contact at 95° from the same point counting towards the west.

THE ELEMENTS OF THE MINOR PLANETS.—The *Vierteljahrschrift der Astronomischen Gesellschaft* (first volume) contains two interesting compilations, on the planets discovered in the year 1890, and on the appearances of comets in the same year. The first paper is contributed by Dr. Paul Lehmann, and informs us that no less than fifteen new members of our minor planet system were discovered last year between February 20 and November 14. In the table that follows a summary of all the days on which each individual planet was observed is given, and this is succeeded by another which shows their chief elements. By combining the elements of some of the old planets with those of the new ones, some striking combinations are thus brought to light, of which we give the two following cases, in which the new planets are 292 and 288 :—

Planets.				
152	$\Omega = 41°3$	$i = 12°2$	$\phi = 4°4$	$a = 3·15$
13	43·2	16·5	5·0	2·58
99	42·0	13·9	13·8	2·80
155	43·1	14·1	14·8	2·91
292	43·1	14·7	2·4	2·53
268	$\Omega = 121°7$	$i = 2°0$	$\phi = 7°9$	$a = 3·09$
288	121·6	4·4	11·6	2·75
113	123·1	5·0	5·0	2·38
213	122·4	6·8	8·3	2·75

The next table shows the values that have been obtained after computing the mean brightest and darkest magnitudes that the planets can attain. In the last form the tabulation is so arranged that the following numbers can be directly seen :—(1) The number of oppositions in which, up to the present time, places have been found, with the number of appearances since observed. (2) The number of every known opposition in which the planet has been observed. (3) Every planet to which the foregoing statement refers. (4) The number of these planets.

SOME EXPERIMENTS MADE WITH THE VIEW OF ASCERTAINING THE RATE OF PROPAGATION OF INDUCED MAGNETISM IN IRON.

THE question, considered in a simple form, may be put thus: Suppose a magnet were suddenly brought up to one end of a long iron rod, what length of time intervenes between the occurrence of magnetization at the near end and at the far end?

Everyone, probably, would at first be inclined to say that the speed along the bar would undoubtedly be about the same as the velocity of light, and this supposition would naturally follow if the energy to places along the bar be supposed transmitted through the surrounding space; but, on the other hand, the speed may be much less if the energy of magnetization is transmitted from particle to particle in the iron—the orientation of the molecular magnets being, as it were, passed from each to the next along the bar. In such case we would, of course, expect the velocity of propagation to be comparable in speed with that of molecular phenomena rather than that of disturbances in the ether.

The velocity of sound, with which we may, perhaps, compare it, is in iron about 16,000 feet per second. The transmission of sound resulting from vibratory movement can be said to depend on the mass of the molecule, and on the mutual forces keeping the molecules in position; while the rate of propagation of a magnetic disturbance of the kind supposed would depend on the moment of inertia of the particles (assumed to be molecular magnets) round their axes of rotation, and on their mutual magnetic moments.

The propagation of such a disturbance can be observed in Prof. Ewing's magnetic model. The model, which consists essentially of a great number of small compass needles placed within each other's action, but not near enough to touch, can be disturbed at one place by bringing a magnet near, or otherwise. The disturbance then is seen to spread throughout the model, much in the same manner as we have suggested a magnetizing disturbance to be propagated in iron.

The method proposed to test matters depended upon the principle of the interference of waves travelling in opposite directions observed through the production of stationary waves.

Thus, if a bar of soft iron have two coils of wire placed one at each end, and if the same alternating current be passed through both coils, disturbance of opposite signs travelling in opposite directions along the bar should interfere, provided the rate of alternation and the length of the bar are chosen suitable to the rate of propagation.

It was proposed to detect the nodes or places of interference by means of a telephone in circuit with a third coil which could be slid along the bar.

Instead of employing two alternating coils, the bar can be bent round to form a ring, and one coil will be then sufficient.

Some preliminary experiments with a straight bar having given faint indications of the existence of places of minimum intensity, closed magnetic circuits or rings, formed of a great number of turns of soft iron wire,[1] were then tried with more decided results. When the alternating coil was in certain positions on the ring the telephone coil could be placed at points where no sound, or if any very slight, could be heard—the sound reaching a maximum in places somewhere between these points. These nodes and internodes occupied about half the ring—the opposite half of the ring from that in which the alternating coil lay. On approaching nearer the alternating coil, apparently the very unequal length of the paths prevented any effect being observed.

It was without difficulty ascertained that these were not the

[1] Two rings were made of No. 21 soft iron wire, one about 10 feet and the other 14·5 feet in circumference. Both had 8 pounds of wire wound on. The wire used in a third ring was No. 32. This ring was about 12 feet in circumference. There was about 4 miles of wire put on. The wire of this and the 14-feet ring was well coated with shellac before winding, so as to minimize Foucault currents.

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nodes looked for, because the distances between them remained unaffected on changing the rate of alternation. The distances from node to node also were found to measure different amounts (though on the whole there was a decided tendency towards regularity). The average distance apart of the nodes in the different rings tried lay between 10 and 18 inches.

The occurrence of the nodes might have been very well attributed to the ring being locally irregular in its susceptibility to induction, but for the irreconcilable fact that the effects on either side of a node were found to be of opposite phase, just as it would be, were the phenomenon due to stationary interference waves.

This was ascertained by means of two coils connected in the same sense in series with the telephone. When these coils were arranged at places of equal intensity, one on each side of a node, no sound was to be heard in the telephone, the effects neutralizing one another. A commutator, to throw in the coils singly or together as desired, is convenient for making this experiment.

From this, one would naturally assume that the currents induced on either side of a node must be of opposite sign, seeing that they neutralize each other in the telephone; but experiments with the galvanometer show it not to be the case. To test this, the galvanometer is connected up through a commutator arrangement fixed to the originator of the primary current in such a way as only to admit of the currents induced in one direction passing. Tried in this way, no difference in the direction of the current on either side of a telephone node was found, or, indeed, any trace of a minimum effect at these points. The thing can also be tested by means of a ballistic galvanometer, and a reversing key with battery, for, with a reversing key and telephone, the nodes, which are quite independent of the speed, are to be found, as well as the opposite phase effect. The ballistic galvanometer gives no indication of there being any difference at the nodes from elsewhere, and the deflection everywhere is in the same direction.

It was thought that perhaps the telephone effect was in some way connected with the fact that the form of the alternating current was not a simple wave or sign curve, owing to the method employed in producing it. This consisted of a rotating commutator, which threw in circuit alternately two cells connected up singly and in opposite directions. For this reason, the effect, when using a small alternating machine with about 40 alternations per second, was compared, and was found to be in no way different. Also what must have been a very regular variable current of the simple harmonic type was procured by means of a microphone and an organ-pipe. This gave like results.

One is thus left apparently to suppose the sound in the telephone to be due to a peculiarity in the character of the curve representing the rise and fall of the current, probably something of the nature of a subsidiary oscillation; this subsidiary oscillation being absent at the nodes, and of opposite sign on either side.

As mentioned before, it is necessary for the alternating coil to be placed at definite positions, in order that the system of nodes and internodes should occur. These positions of the alternating coil are at about the same average distance apart, and are of very much the same character with respect to regularity as the nodes of the telephone coil. In fact, if the alternating coil and the telephone coil change places round the ring, the best position for the alternating coil will always be between two nodes, and the nodes will be found situated between two old positions of the alternating coil. If the alternating coil be placed at a point where a node was found in some other position of the alternating coil, the system of nodes and internodes generally completely disappears, and now on moving the telephone coil round the ring the intensity uniformly diminishes until the diameter is reached, and then increases round the other half of the ring. This gives the phenomenon a distinctly resonant character. The induced current, as observed by a galvanometer, is always of the latter character—that is to say, a uniform fall, and then a rise on going round the ring.

As a rule the permanent magnetism of these large rings is irregular, and apparently apt to change frequently. A determination of the permanent magnetism was easily made by means of one of the coils connected with a ballistic galvanometer. By moving this through a given amount at a time, say an inch, and noting the throw of the needle, one was able to plot out a representation of the state of the permanent magnetism. In this way, places where no throw occurs were found, while to either side of such a point the throw changed sign. It was sometimes found that there was a decided tendency for the position of no throw to occur between two telephone nodes, the throw changing sign on either side of these points. But further experiments showed that this arrangement of the permanent magnetism was probably accidental, and due to the very currents employed in making the telephone observations. For when only very feeble currents had been used on a ring, these consequent poles were absent.

It is possible, as one would expect, to artificially make a minimum intensity position, at any point on a ring, by winding on a few turns of thick copper wire. But the fact that the phases on either side of such a point (found as before by means of two coils in circuit with a telephone) are the same, precludes the idea that the nodes can be due to Foucault currents.

Obviously, however, the phenomenon depends on some permanent peculiarity round the ring which happens to occur fairly regularly. What this peculiarity is, or how it is brought about, I have not yet been able to discover.

FRED. T. TROUTON.

OYSTERS AT THE ANTIPODES.

SO much attention has been given in England to the various questions connected with oyster-fisheries that it may be of interest to note some facts relating to the oyster-fisheries of our Australian kinsfolk. The subject was admirably dealt with in a lecture delivered by Mr. Saville-Kent before the Christchurch meeting of the Australasian Association for the Advancement of Science. This lecture is entitled "Oysters and Oyster-Culture in Australasia," and has been published separately.

Mr. Saville-Kent devotes attention chiefly to Australia and Tasmania, as, at the time when his lecture was prepared, he had not had an opportunity of personally studying the question in New Zealand. Beginning with Tasmania, where for five years he was officially connected with the oyster-fisheries, he points out that the oyster of Tasmania corresponds closely with the type *Ostrea edulis*, produced and cultivated in British waters. Formerly, this oyster was so abundant in Tasmanian waters, that, according to the report of a Royal Commission of Fisheries in 1882, about twenty years previously a quantity representing at current prices a retail value of no less than £90,000 had been exported in a single year to Victoria and New South Wales. At that time, oysters were so plentiful that it was a common practice to burn them in large quantities for the purpose of making lime. The strain was, of course, too severe, and by and by the Tasmanians found that, although there was still a demand for oysters, there was no longer a home-supply, and that it was necessary for them to go elsewhere for the commodity which they had so recklessly wasted. In 1884, when Mr. Saville-Kent reached the colony, the oyster-fisheries of Tasmania had for some years been an obsolete industry. Profiting by the information which had been made accessible through the International Fisheries Exhibition and associated Conferences in London in 1883, and by Prof. Hubrecht's testimony as to the oyster-fisheries of the Schelde, Mr. Saville-Kent recommended the establishment, in suitable localities, of efficiently-protected Government reserves, upon which breeding-stocks of oysters of the best quality should be carefully cultivated and permanently retained. These reserves were to fulfil the double purpose of breeding-centres, from whence the surrounding waters might be restocked, and also of model oyster-farms, around which private beds might be established on similar lines. The scheme recommended being approved, sites formerly associated with the most prolific oyster production were selected. The operations were necessarily conducted on a very modest scale. Oyster stock, suitable for laying on the reserves, could be accumulated only by slow and laborious processes, and some 20,000 to 50,000 oysters represented the approximate numbers that were gradually collected and placed under cultivation. In order that the largest possible amount of spat produced by the oyster stocks laid down might be caught, various methods were adopted, the principle being that which has been followed with so much success by M. Coste on the west coast of France. In addition to dead oyster-shells, or "cultch," which has, from the earliest days of oyster-culture, been recognized as representing a most natural and prolific catchment material for the adhesion of the spat, artificial collectors of various descriptions were introduced. In France, tiles cemented on their lower surfaces have been found

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to constitute the most productive and economic collectors. In Tasmania, as in all the other Australasian colonies, tiles being much too expensive for such a purpose, a cheap and efficient substitute for them was effectually improvised out of the thin roughly-cleft boards known as "split palings," which can be produced in timber-producing districts at a cost of from 8s. to 10s. per 1000. These paling collectors are coated on their under surface with cement, a brick or stone is fastened underneath at each end to give them stability, and a wire loop secured through the centre of their upper surface forms a convenient handle by which they can be manipulated on shore or raised with a boat-hook from beneath the water.

The results have been most satisfactory. Last year oysters had become so plentiful at Spring Bay that the Hobart market was glutted, and the sale price was reduced 50 per cent. Thus an important industry has been revived, and there can be little doubt that by the due maintenance of the breeding-reserves the oyster-fisheries of Tasmania will be restored to more than their former prosperity. In accordance with Mr. Saville-Kent's recommendations, all holders of private oyster-beds in Tasmania are bound by the terms of their leases to retain a certain amount of breeding-stock—not less than 10,000 mature oysters to the acre—permanently on their oyster-beds. This regulation contributes materially towards the distribution of spat throughout the surrounding water, and to the re-establishment of the oyster-fisheries upon a durable basis.

Referring next to Victoria, Mr. Saville-Kent says that the specific form of oyster indigenous to the Victorian coast-line is a so-called mud oyster, identical with that produced in Tasmanian waters, and to all outward appearance indistinguishable from the British native, *Ostrea edulis*. In former years vast quantities of this oyster were obtained from Western Port Bay, Port Albert, and Corner Inlet. Over-dredging, however, has reduced these prolific natural beds to the very verge of extinction, so that Victoria has for many years been dependent upon New South Wales, Queensland, South Australia, and New Zealand, for her oyster supplies. Some time ago Mr. Saville-Kent was invited by the Government of Victoria to make a tour of and report upon the fisheries of the colony, giving special attention to the practicability of reviving the oyster-fisheries. As a result of that tour of inspection, he strongly recommended the adoption, at Western Port and Port Albert more particularly, of the methods which had proved so effective in Tasmania. One such reserve with a very small stock of oysters was formed at Port Albert. Unfortunately, however, the Government omitted to make provision for periodical skilled supervision, and the reserve dwindled away. As Mr. Saville-Kent says, unless such reserves can be maintained in efficient working order, and the operations periodically required thereon be supervised by a practical ostreculturist, the money expended on their establishment is simply wasted.

At various parts of the Victorian coast-line, Mr. Saville-Kent observed considerable numbers of oyster-shells, evidently derived from deep water, that had recently been cast upon the shore by storms. He consequently predicted that more or less extensive beds would be found off the coast; and off-shore beds have in fact been since discovered. Mr. Saville-Kent points out that a most favourable opportunity is thus afforded for the restocking of the in-shore fisheries.

In New South Wales a separate species of oyster has to be taken into consideration. The commercial species of this colony is the rock oyster, *Ostrea glomerata*. At the same time a mud oyster, identical with or most closely allied to the Victorian and Tasmanian type, *Ostrea edulis*, occurs in some numbers upon the coast, but in consequence of the hitherto profuse abundance of the rock variety it has not been considered worthy of commercial attention. In form and general aspect the New South Wales rock oyster somewhat resembles the Portuguese oyster, *Ostrea angulata*, and also the American *Ostrea virgineana*. With these two species it further corresponds in its breeding habits, which are essentially distinct from those of the English, Victorian, and Tasmanian mud oyster, *Ostrea edulis*. In the case of the Australian rock oyster there is no nursing of the young brood, which is turned out to shift for itself, not only in a shell-less but even in an unfertilized condition. Like the spawn of many fishes, these ova are fertilized in the water. They can be readily fertilized artificially, and Mr. Saville-Kent has found that four days after fertilization the shells, which make their appearance on the second day, become so dense that the embryo oysters can no longer support themselves in the water, but sink to the bottom, where they assume their permanently fixed condition. Such is the fecundity of this oyster that the rocks and every available hiding-place in the bays, estuaries, and inlets of the districts it affects become literally plastered with the embryo brood; and until quite recently, artificial culture in the scientific sense has in New South Wales been usually regarded as an unprofitable and unnecessary superfluity. Lately, however, the oyster-fisheries of the colony have been seriously damaged by a disease which either destroys the oyster or makes it unfit for food. Mr. Saville-Kent attributes this disease to the pollution of rivers. If he is right in this view, in support of which he has much to say, the oyster-growers of New South Wales will, as he says, have to make the most of the water area left to them where the water is pure. They may also have to turn their attention to the cultivation of the New South Wales variety of the mud oyster.

In Queensland, as in New South Wales, the only oyster used in commerce is the rock variety, which may be said to attain its maximum development in both quantity and quality in the Moreton and Wide Bay districts. In these areas the species is so abundant that large consignments, above those required for home consumption, are exported to New South Wales and Victoria. The disease which has so seriously depleted the fisheries of New South Wales has not yet affected the Queensland beds. Mr. Saville-Kent thinks that this immunity is probably due to the circumstance that the Queensland oyster-fisheries are chiefly located in bays and channels in close proximity to the open sea, from whence, even after heavy floods from the tributary rivers, they are speedily revived by an inflow of sea-water. He urges the Queensland authorities to preserve these tributary streams as far as possible from pollution by chemical or other noxious works, which if allowed to increase to any considerable extent cannot fail to exert a very deleterious effect upon both the oyster and all other fisheries of the bays into which the rivers flow. Artificial oyster-culture, with the exception of one or two small experiments, has been so far carried out in Queensland waters on the simple lines only of transporting the young brood or ware, locally known as "cultivation," from one locality and laying it down on ground where it will develop more speedily to maturity. Mr. Saville-Kent believes, however, that it would be profitable to use split palings as spat collectors. One advantage possessed by this form of collector is the shelter from the sun's heat afforded to the young brood when left high and dry by the receding tide. Millions of the Australian rock oyster are destroyed annually through exposure to the overpowering heat of the sub-tropical sun in the early days of their attachment to exposed rocks near high-water mark. The overhanging ledges of larger rocks and the shady sides of stone jetties or embankments are invariably found to attract and support the greatest amount of oyster brood, and this shelter, which is naturally sought, plainly indicates the lines that may be most profitably followed in operations connected with the artificial cultivation of the species.

We may note that since Mr. Saville-Kent's lecture was published a report by him on "Oysters and Oyster-fisheries of Queensland" has been issued by the Queensland Government. In this report, which is carefully illustrated, full details are given as to the conditions which must be specially taken into account by all persons connected with Queensland oyster-fisheries. Referring to the "split paling" collectors, Mr. Saville-Kent emphatically repeats what he says in their favour in his lecture. After considerable experience he expresses his conviction that they are the most convenient and economic form for use in Australian waters, and that they may be characterized as an essentially Australian product. About half-tide mark represents the zone within which—at all times of the year, but especially in the months of February and August—they gather the most abundant harvest of spat. On their first attachment to the cemented collectors, the young oysters adhere to the cement by the entire surface of the attached shell. After attaining to about half an inch in diameter, the free edges of the shells begin to grow outwards, and this direction of their growth is continued until at an age of about six months they project an inch and a-half or two inches from the collector. At this stage the young oysters may be easily detached with or without the cement, and be laid on the banks as ordinary "cultivation." The collectors may then be re-cemented and re-laid for the catchment of a second crop.

Of the oyster-fisheries of South and West Australia Mr. Saville-Kent is not able, in his lecture, to give precise details. He says,

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however, that excellent oysters of fine quality and magnificent proportions, allied to *Ostrea edulis*, are exported from Spencer's Gulf in South Australia to the Victorian markets, and more especially to Ballarat. Some of these South Australian oysters are of such Brobdingnagian dimensions that it is customary to cut them in four pieces for sale at the oyster saloons, the quarters thus divided being severally allotted to separate shells of ordinary size and sold as single oysters.

Mr. Saville-Kent congratulates New Zealand upon her abundant stores of oysters of various kinds. The days for the systematic artificial cultivation of the oyster in that colony have not yet arrived, and if she carefully husbands her natural resources they may, he thinks, be long delayed.

THE TIBET EXPEDITION.

AT an extraordinary meeting of the Russian Geographical Society on October 14, General Pevtsoff made his report about the Tibet Expedition, of which he was the commander after the death of Przewalsky. Having crossed the main Tian-shan ridge by the Bedel Pass, the Expedition went southwards, through an extremely narrow gorge of the Kara-teke ridge. In some places the gorge has only the width of 30 to 35 feet, while its walls are 700 feet high. The first Kashgarian village reached was Kalpyn, whence the travellers went to Yarkend. From Yarkend they moved on the great Khotan high-road into the northern spurs of the Kuen-lun. There they stayed for some forty days, at a height of 10,000 feet above the sea, at Tokhta-hon, collecting many interesting plants and birds, while the geologist of the expedition, M. Bogdanovitch, made a long excursion into the region between the Yarkend-daria and the Tyznan Rivers. On September 13 they left the highlands, and after a three weeks' journey arrived at the Khotan oasis, the population of which (120,000) are skilful in the manufacture of carpets, felts, silks, and so on. From Khotan they went to Keria, and next to Niya, where they left their superfluous luggage, and whence they started to explore the Kuen-lun, in order to try to find a good pass to Tibet. The pass was found at the sources of the Tillan-hadji stream, not far from the Minjilin-khanum monastery. It proved to be quite available both for horses and camels. The winter was spent at Niya. On May 7, the work of exploration was resumed, and next week the Expedition reached the Kara-sai village. Followed by two men only, M. Roborovsky went up the Saryk-tuz Pass (discovered during the preceding autumn), and attained the sources of the Keriya-daria on the Tibet plateau. Its altitude proved to be there 16,500 feet, and its surface was an absolute desert. The want of food for the horses compelled M. Roborovsky soon to return to Kara-sai. He soon made a second attempt at further exploration, but, after having marched some 50 miles southwards on the plateau, he was again compelled to return. During the same time, M. Kozloff went across the border-ridge, following for some 100 miles the Bastan-tigrak River. He passed by Lake Dashi-kul and went up the river which flows into the lake from the east, through a wild desert, 14,000 feet above the sea. He also was soon compelled to return to Kara-sai. The next attempt was made by all three explorers together, accompanied by four Russians and a few natives. They went up the Aksu River, and soon were on a plateau, 15,000 feet high and almost quite devoid of vegetation. Terrible snow-storms were raging in the first days of July. The only mammals seen were two antelopes, and the only bird met with was a lark. Finding no food for the horses, the Expedition had nothing to do but to return to Kara-sai. Thence they started for Tchertchen, and at Atchan they were rejoined by M. Bogdanovitch, who had explored in the meantime the geological structure of the two passes of Saryk-tuz and Aksu. After a short stay at Mandalyk, where good grazing-grounds were found, and the horses recovered, the Expedition crossed again the Kuen-lun *viâ* the Muzluk Pass (15,500 feet high), and after having crossed it they divided into two parties, one of which, under M. Roborovsky, went south-east, and the other, under General Pevtsoff, moved southwards, up the little River Uluk-su, which is the source of the Tchertchen-daria. They soon came to an immense chalky mountain ridge, which rose to about 20,000 feet in the south, while a wide valley stretched south-westwards between that ridge and the Kuen-lun. The party stopped at the foot of this ridge, at a small lake, Yamil-kul. From some natives who were engaged in gold-mining in a gorge of the ridge, they learned that its name is Akka-tai, and that its summits are covered with perpetual snow. The party did not proceed further, and from Yamil-kul they returned to Mandalyk, and thence began their journey to Lob-nor, which journey took no less than one month. From Lob-nor the Expedition went up the Yarkend-daria, visiting on the way the great settlement of Kurla (4000 inhabitants), the fort of Karashar (10,800 inhabitants in the fort and the oasis), and the town Uruntchi, situated at the foot of the Tian-shan, and residence of the Governor-General of West China. On their way to the Russian frontier the travellers visited also the oasis of Sa-tsan, peopled by Chinese, and crossed the Malas River as well as the desert Khatyn-ula. On January 15, 1891, they entered the Russian post of Zaisan.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The Rev. Andrew Clark, M.A., Fellow of Lincoln College, has been elected by Congregation a Curator of the Bodleian Library, in place of Prof. Max Müller, whose term of office had expired.

The Provost of Oriel College has been re-elected a Delegate of the University Museum.

Mr. F. Liddell, B.A. Christ Church, has been elected to a Fellowship at All Souls' College. Mr. Liddell, who is a son of the Dean of Christ Church, was placed in the first class by the examiners in the Final Classical Schools. Mr. A. H. Hardinge, M.A., formerly Fellow of All Souls' College, has been elected to a Fellowship under Statute 3, Clause 10, of the College Statutes.

There was no candidate for election to the Burdett-Coutts Scholarships. The scholarships are of the annual value of about £115, tenable for two years, for the promotion of the study of geology, and of natural science bearing on geology. This is the fifth occasion since the foundation of the scholarships that there has been either no candidate or no election.

In consequence of the requirements of the Civil Service Commissioners for the limited competition for assistantships in the Royal Observatory, Greenwich, the Savilian Professor of Astronomy has offered a short course of lectures on Newton's "Principia." The study of Newton has been practically abolished from the requirements of the Oxford Mathematical Schools for some time past.

A studentship, provided out of the funds of the Newton Testimonial Fund, having been offered to the University by the Managing Committee of the British School at Athens, the Craven Committee will proceed to make the appointment in the course of the present term. The studentship is of the value of £50, and is tenable for one year. The holder will be required to reside at Athens for not less than three months during the ensuing winter and spring. Candidates should apply to the Secretary of the Board of Faculties, Clarendon Buildings.

CAMBRIDGE.—The first award of the Isaac Newton Studentship in Astronomy and Physical Optics has been made to Ralph Allen Sampson, B.A., Fellow of St. John's College, Cambridge.

Prof. Thomson, F.R.S., has been elected Chairman of Examiners for Part II. of the Mathematical Tripos.

The vacancy of the office of Superintendent of the Museum of Zoology will take place on January 1, 1892. The stipend is £200. Applications are to be sent to Prof. Newton before November 19.

The State Medicine Syndicate report that this year there were 64 candidates for the Diploma in Public Health, of whom 45 were successful. They propose, on account of the large number of candidates, to hold a second examination in the first week of April 1892. The Syndicate have resolved to transfer to the University a sum of £150 from their accumulated funds.

SCIENTIFIC SERIALS.

American Journal of Mathematics, vol. xiv. No. 1 (Baltimore, Johns Hopkins Press).—This number, which contains an excellent likeness and autograph of Prof. Klein, opens with articles by Goursat, "Sur une problème relatif à la déformation des surfaces," and by Appell, "Sur une expression nouvelle des fonctions elliptiques par le quotient de deux séries."—Major MacMahon, F.R.S., contributes a fourth memoir on a new

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theory of symmetric functions. The author has extended the subject of these memoirs in a paper with the title "Memoir on symmetric functions of the roots of systems of equations" in the Philosophical Transactions, A. (1890).—The next paper, by C. P. Steinmetz, was read before the New York Mathematical Society, and is entitled "Multivalent and univalent involutory correspondences in a plane determined by a net of curves of nth order."—The following note, also read before the same Society, is on the algebraic proof of a certain series. It supplies a "temporary lack," which was regretted by the author, E. McClintock, in a memoir which will be found in vol. ii. p. 108. The same writer furnishes another addition to the memoir just referred to (vol. ii.), on independent definitions of the functions $\log x$ and e^x.—H. B. Newson writes on a pair of curves of the fourth degree, and their application in the theory of quadrics; and H. P. Manning finishes this instalment with a note on linear transformation, which was suggested by a method employed by Prof. Cayley, F.R.S., in vol. v. of the *Amer. Journal*.

THE articles in the numbers of the *Journal of Botany* for October and November are mostly of interest to specialists in local floras. Mr. A. Fryer describes and figures a new English *Potamogeton* (or, rather, hybrid). Mr. T. H. Buffham describes and figures the hitherto unknown plurilocular sporanges in two sea-weeds, *Asperococcus bullosus* and *Myriotrichia clavæformis*. Mr. F. N. Williams contributes a synopsis of the primary characters in the species of *Rheum*.

THE last number received of the *Botanical Magazine* published at Tokyo, Japan (for June), contains an interesting article, illustrated, on a new Japanese *Prasiola*, *P. japonica*, by Dr. R. Yatabe, which, the author states, is collected in large quantities in the districts where it grows, and is sold as an article of food under different names in different localities. It is eaten either slightly broiled or with vinegar, the mode of preparation being very similar to that of the ordinary purple and green lavers. Other articles in the same number are on the reproduction of *Laminaria japonica*; on a recent problem in vegetable physiology (apparently the greatly discussed question of the direct absorption of nitrogen from the air by plants); and on the colours and scents of flowers; but as all these are unfortunately in Japanese, they are inaccessible to the English reader.

THE number of the *Nuovo Giornale Botanico Italiano* for October is chiefly occupied with the Proceedings of the Italian Botanical Society. The attention of Italian botanists is still directed to the interesting phenomena connected with the pollination of the Aroideæ: Signor Caleri has a paper on the flowering of *Arum Dioscoridis*, and Prof. Arcangeli one on the fertilizers of *Helicodiceros muscivorus*. He is unable to discover any evidence that the latter plant is really carnivorous.—Signor Martelli discusses a vine-disease which has lately appeared in the neighbourhood of Florence, and which he identifies with the "black rot" of the American grape, caused by a Pyrenomycetous fungus, *Physalospora Bidwelli*.

SOCIETIES AND ACADEMIES.

LONDON.

Entomological Society, October 7.—Dr. D. Sharp, F.R.S., Vice-President, in the chair.—The Chairman referred to the death, on September 14 last, of Mr. E. W. Janson, who had been a member of the Society since 1843, and who had formerly filled the offices of secretary and librarian respectively.—The Rev. Dr. Walker exhibited a long series of several species of *Erebia*, and of *Argynnis pales*, which he had recently captured near Roldal, in Norway.—Mr. W. L. Distant exhibited specimens of *Danais chrysippus*, with its two varietal forms, *alcippus*, Cram., and *dorippus*, Klug., all which he found together in the Pretoria district of the Transvaal. Mr. Jenner Weir and Colonel Swinhoe took part in the discussion which ensued as to these forms and their distribution.—The Rev. W. F. Johnson sent for exhibition specimens of *Velia currens* from stagnant water near Armagh; also a specimen of *Nabis limbatus*, killed whilst holding on to its prey, a very hard species of Ichneumon. Mr. Saunders thought that, from the nature of the Ichneumon, the only chance the *Nabis* had of reaching its internal juices would be through the anal opening, as recorded by Mr. E. A. Butler in a similar case, in the *Entomologist's Monthly Magazine*, October 1891.—Mr. F. P. Pascoe exhibited two British species of Diptera, unnamed. He said they had been submitted to Mr. R. H. Meade, but were unknown to him, and are probably new to the British list.—Mr. R. Adkin exhibited two specimens of a supposed new species of Tortrix (*Tortrix douglana*, Carpenter), bred from larvæ found on pine-trees at Tuam. Mr. C. G. Barrett said he examined the specimens with great care, but he did not consider that they belonged to a new species. He was unable to distinguish them from *Tortrix viburnana*.—M. A. Wailly exhibited preserved larvæ, in various stages, of *Citheronia regalis*, which he had bred from ova received from Iowa, United States. He said that the natives called this larva the Hickory Horned Devil, and that the specimens exhibited were the first that had been bred in this country. M. Wailly further exhibited three female specimens of *Antheræa yama-mai* bred from cocoons received from Japan; also a nest of cocoons of *Bombyx radama*, received from the west coast of Madagascar. Prof. J. B. Smith, of the United States, and Colonel Swinhoe took part in a discussion on the habits of the larvæ of *Citheronia regalis*, and as to the period at which they dropped their spines prior to pupating.—Dr. Sharp exhibited several specimens of a weevil, *Ectopsis ferrugalis*, the ends of the elytra of which bore a close resemblance to the section of a twig cut with a sharp knife. He said he had received the specimens from Mr. G. V. Hudson, of Wellington, New Zealand, who stated that they were found resting in large numbers on dead trunks and branches of *Panax arborea* in the forests.—Mr. G. C. Champion stated that the species of *Forficulidæ*, captured by Mr. J. J. Walker, R.N., in Tasmania, and exhibited by himself at the meeting of the Society in April last, was, he believed, referable to *Anisolabis tasmanica*, Bormans, described in the *Comptes rendus* of the Ent. Soc. Belgique, 1880.—The Rev. A. E. Eaton made some remarks on the synonymy of the *Psychodidæ*, and stated that, since August 1890, he had identified all of the British species in Mr. Verrall's list, except *Sycorax silacea*.—Mr. Gervase F. Mathew, R.N., communicated a paper entitled "The Effect of Change of Climate upon the emergence of certain species of Lepidoptera." A discussion followed, in which Mr. Stainton, F.R.S., Mr. Barrett Dr. Sharp, F.R.S., and Mr. McLachlan, F.R.S., took part.

Royal Microscopical Society, October 21.—Dr. R. Braithwaite, President, in the chair.—The President said that the pleasure with which he met the Fellows after the vacation was very sadly marred by the death of one of their Secretaries, Mr. John Mayall, Jun. The loss they had sustained was one which the Society could hardly hope to replace, because perhaps there was no living person who knew more about the microscope and its applications than their deceased friend Mr. Mayall. The difficulty in which they were placed had, however, for the present been met by the kindness of Dr. Dallinger, who had undertaken to fill up the vacant place until the end of the current session.—Mr. A. D. Michael proposed, and Mr. T. H. Powell seconded, that a special vote of thanks be given to Dr. Dallinger for his kindness in accepting the office of Secretary. The vote of thanks was carried by acclamation.—Mr. F. Chapman read his paper on the Foraminifera of the Gault.—Sir Walter J. Sendall exhibited and described a new apparatus which he had devised for making accurate measurements with the camera lucida, the inherent faults of which were explained by drawings on the blackboard. Mr. E. M. Nelson said there could be no doubt that camera lucida measurements when made in the ordinary way as described were grossly incorrect, and that the apparatus that had been devised was most ingenious and thoroughly scientific in principle. He thought, however, that there was a much simpler method of obtaining measurements by projecting the image for a distance of 5 feet; the curve would with so large a radius be practically reduced to a straight line. The camera lucida and neutral tint reflector were only rough-and-ready means, and useful only for ready reference; where correctness was of importance, the eye-piece micrometer would best meet the requirements; the ruling of eye-piece micrometers was now done so perfectly that it was possible to arrive at measurements even as small as 1/500,000 of an inch with far greater accuracy than could be attained with any machine. Dr. W. H. Dallinger thought there could be no doubt of the value of the apparatus within certain limits, but it would require great care for use with high powers, partly on account of its weight if made in brass, as the specimen before them; perhaps it might be made in aluminium or some other light material. The discussion was continued by Messrs. A. D. Michael, C. Beck, and Sir Walter J. Sendall.—Mr. W. I.

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Chadwick described the Leach lantern microscope as follows. The microscope can be applied to any oxy-hydrogen lantern. It is screwed on the front in place of the ordinary lantern objective, the size of flange required being 2½ inches; when the lantern objective flange is larger than this, an adaptor must be provided; and when the draw-tube of the lantern is "ricketty," a rigid lengthening tube may be adapted. The lantern condenser should be about 4 inches or 4½ inches in diameter, and of the triple form. The stage of the microscope is open at both sides, and at the top also, and serves for all classes of objects, whether ordinary microscopic slides or polariscope crystals, shown with either narrow angle rays or by the convergent system of lenses. The stage being so constructed, it is extremely accessible for the introduction of sub-condensers, with which the instrument is provided. The object-holder is quite a novel idea, the principal mechanism of it is placed under the stage (to be out of the way); two arms passing through slots in the bottom of the stage, actuated by a spring and manipulated by a milled head, serve to hold the objects flat against the inside surface of the front of the stage. The diaphragm, or compound wheel of diaphragms, is rotative on a pivot attached to the plate arm in such a manner that the whole may be raised out of the field altogether, and dropped into it again, in an instant; when the wheel is raised, a spring catch holds it in position. When in this position the whole field of the microscope can be utilized for showing objects up to 1½ inches in diameter. When, as in using polarized light, it is desired not to be incommoded with the diaphragms, the detachable plate carrying the compound wheel can be instantly removed from the stage, and when again required it can be as quickly restored. The arms of the object-holder projecting through the bottom of the stage have sufficient lateral movement to admit any zoophyte trough or wooden frame or combination of wooden frames up to 1 inch in thickness. Thus the advantages of this arrangement are clearly manifest. The two sub-condensers with which the instrument is provided are found to give satisfaction with all objectives of from 2½ to $\frac{1}{16}$ inch focus. When the light has been properly concentrated, high powers can be used. It should also be observed that when high powers are used the front lens of the objective is open to the view of the manipulator, a great convenience when inserting the object, by enabling it to be immediately adjusted within the area of the lens. When polarized light is to be used, the polarizing prism must be pushed into the rotating tube of the instrument by removing the concave lens at the back, and after inserting the prism this concave lens may be replaced in an instant. The rotating tube is an advantage over fixed tubes, as the polarizing prism can by this arrangement be placed in any desired azimuth which best suits the object. The convergent system of lenses for use with polarized light in transmitting rays through biaxial crystals was worked out by Mr. Leach. It gives powerful illuminations, and includes an angle of 170°. The front focussing arrangement was introduced by Mr. Leach in 1883. Before that time several supplementary lenses had to be kept in readiness for use, as different classes of crystals were placed in the polariscope. Mr. Leach discovered how these supplementary lenses might be dispensed with, and fitted up his system accordingly; and now all makers of first-class polariscopes attach to their instruments this great improvement. The concave field lens, with which the instrument is provided, is absolutely necessary when the polarizing prism is in use. With all powers it enlarges the field, and equalizes the distribution of illumination. The three objective adaptors with which the instrument is provided admit of any microscope power with the standard screw; they are made to slide in the front tube of the microscope, which is provided with a rack and pinion, and also with a fine screw movement. Thus, by having the various powers already screwed into the adaptors, one may be changed for another almost instantaneously. And into the front or tube portion of these adaptors the tube of the amplifier is made to slide. The amplifier which is provided is a Barlow lens, and being achromatic, it enhances the aplanatic qualities of the objective. It has been asserted "that high power cannot be used in the lantern microscope; that it is unable to exhibit fine detail upon the screen, and that no alum trough is required." No doubt this is all true so far as applied to inefficient instruments. But the Leach microscope does require an alum trough, because where great light is concentrated from the oxy-hydrogen luminant, great heat must, from the very nature of the means employed, be concentrated with it, and the alum trough is the only practical thing which can be used to absorb the heat rays. The alum trough is of large size, and is used in the ordinary slide stage of the lantern. At the conclusion of the paper, Messrs. Chadwick and Leach gave a demonstration with common and polarized light.

CAMBRIDGE.

Philosophical Society, October 26.—Prof. G. H. Darwin, President, in the chair.—The officers for the ensuing session were elected as follows:—President: Prof. G. H. Darwin. Vice-Presidents: Prof. Hughes, Prof. Thomson, Mr. J. W. Clark. Treasurer: Mr. R. T. Glazebrook. Secretaries: Mr. J. Larmor, Mr. S. F. Harmer, Mr. E. W. Hobson. New Members of Council: Mr. H. F. Newall, Mr. C. T. Heycock, Mr. A. E. H. Love.—The following communications were made to the Society:—On the absorption of energy by the secondary of a transformer, by Prof. Thomson.—On an experiment of Sir Humphry Davy's, by Mr. G. F. C. Searle. Two copper wires are passed up through holes about 5 centimetres apart in the bottom of a flat trough, their ends being level with the surface of the trough. Mercury is then poured into the trough to a depth of about 4 millimetres. On sending a powerful current through the mercury by means of the two wires the mercury in the immediate neighbourhood of the electrodes was elevated into a small cone 2 or 3 millimetres in height.—Some notes on Clark's cells, by Mr. R. T. Glazebrook and Mr. S. Skinner. In addition to the causes of variation indicated by Lord Rayleigh, the authors find that the state of amalgamation of the zinc pole may cause a fall in force if the zinc does not show a bright surface. This is worked out by means of a testing cell into which the faulty zincs are transplanted. The result is confirmed by Swinburne's experiments on zinc rods in zinc sulphate solution. To correct this fault previous amalgamation in the presence of dilute sulphuric acid is recommended, or immersion of the zinc in the paste. Dr. Hopkinson's method of testing cells by tapping was shown.—Illustrations of a method of measuring ionic velocities, by Mr. W. C. D. Whetham.—On gold-tin alloys, by Mr. A. P. Laurie.

PARIS.

Academy of Sciences, November 2.—M. Duchartre in the chair.—On aberration, by M. Mascart.—Note on Mont Blanc Observatory, by M. J. Janssen. This is a brief report of the attempt to reach the rock through the snow on the summit of Mont Blanc, in order to obtain a foundation for a proposed Observatory. In spite of circumstances which rendered the proposed building impossible, M. Janssen believed that an edifice of some kind resting on the snow would permit the necessary observations to be carried on, and had one constructed according to his ideas. No displacement of the erection occurred during the twenty days previous to M. Janssen's departure from the summit of the mountain. The construction of a similar but more important building is therefore contemplated for next year.—Note by M. Armand Gautier, accompanying the presentation of his work on "Biological Chemistry."—On the Arago Laboratory, by M. de Lacaze-Duthiers.—Contribution to the natural history of the truffle: parallelism between the Terfaz or Kama (*Terfezia*, *Tirmania*) of Africa and Asia and the truffles of Europe, by M. A. Chatin. In the comparison the points considered are geographical distribution, climate, soil, nutritious plants, periods of ripening, depth in soil, and numerous other characteristics.—An excursion in the Rocky Mountains, by M. Albert Gaudry. At the end of the recent Geological Congress at Washington a party was organized to visit the Rocky Mountains. An account is given of some of the objects of geological interest observed by the excursionists.—Note on the storm that visited Martinique on August 18, 1891 (an extract from the *American Journal of Meteorology*), by M. Faye.—Researches on butylene monobromides, by M. E. Reboul. There are three butylene monobromides known besides the isobutylene of Boutlerow. The author describes the preparation and properties of one of these, to which he assigns the constitution $CH_3—CH_2—CBr{=}CH_2$. He proposes to term it ethyl-acetylene α-hydrobromide.—Observations of two new asteroids discovered at Nice Observatory on September 24 and October 8, 1891, by M. Charlois. Observations of position are given.—On the dimensions and form of the section of a stream (*veine*) of gas under limited counter-pressure during a limited delivery, by M. Parenty.—On a model of a luminous fountain, by M. G. Trouvé.—On the direct combinations of metals with chlorine and bromine, by MM. Henri Gautier and Georges Charpy.

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With the exception of aluminium, most of the metals are hardly attacked by dry chlorine and bromine at the ordinary temperature. Aluminium, however, is acted on very energetically by liquid chlorine and bromine, whilst magnesium particularly resists the action. The reactions are very slow when the dry halogens are used. When water is present the action becomes more rapid, hydrogen being generally liberated owing to its decomposition, but in some cases the water remains unaltered. —Contribution to the chemico-physical study of the function of the kidney, by M. C. Chabrié.—On the chronology of the eruptive rocks of Jersey, by M. A. de Lapparent.—New geological observations of the Island of Sardinia, by M. Charles de Stefani.—New considerations on the Vertebrate fauna of the Upper Miocene in the Isle of Samos, by M. Forsyth Major.—The gravels of Montfort, by M. Ed. Piette.

BERLIN.

Physiological Society, October 16.—Prof. du Bois-Reymond, President, in the chair.—Dr. Lüderitz gave an account of an investigation of the changes of blood-pressure in the left ventricle and right carotid which result from gradual compression of the aorta.—The President exhibited three very successful photographs of the posterior (retinal) surface of the eye.—Doctor Wertheim recorded the disappearance of the indirect image of an illuminated disk when the object itself, as seen directly, is suddenly darkened.—Dr. Lilienfeld gave an account of a chemical examination of blood-platelets, which showed that they consist of a compound of albumin and nuclein, whose behaviour speaks against their being preformed structures.

Physical Society, October 23.—Prof. du Bois-Reymond, President, in the chair.—The Society resolved to present to Prof. von Helmholtz, on November 2, in celebration of his seventieth birthday, an address prepared by Prof. von Bezold. —Messrs. Haensch described a modification which they had made in a spectrophotometer.—Dr. Rubens gave an account of a new method of determining dispersion and refraction in the ultra-violet rays, a method which, unlike that employed by Langley, yields more accurate results by very simple means. He has already made determinations with a series of glasses, with water and with carbon bisulphide. The curve of dispersion he finds to be, on the whole, the same as that obtained by Langley for rock-salt.—Prof. Preyer enunciated his hypothesis as to the genealogy of the chemical elements.

AMSTERDAM.

Royal Academy of Sciences, September 26.—Prof. van de Sande Bakhuysen in the chair.—Prof. Franchimont showed a little bottle filled with a new chemical compound, obtained and examined by Dr. C. A. Lobry de Bruyn. This hydroxylamine is a crystalline matter without colour and smell. It is prepared by the action of natrium-methylate on the methyl-alcoholic solution of the compound of hydroxylamine and HCl, and by distillating and fractionating the result *in vacuo*. The specimen is pure to 99·6 per cent; it melts at 31°·5, and distillates under a pressure of 35 mm. between 63°·5 and 65°·5. —Mr. Behrens spoke of the microscopic structure of hard steel. If high microscopic powers are used, the network in hardened steel may be made visible on polished slices without etching or annealing. The dark sinuous lines answering to the bright ones shown by Sorby and Wedding on etched slices, it is proved that hardened steel contains hard granules bound up in a matrix of soft iron. Some varieties of grey iron from small castings may be hardened like steel, most of the graphite disappearing. After annealing the hardened metal at a red heat, the slices were dotted with blackish dust, which formed circles round the globules of the crystallites and little heaps in the midst of them. It is to be presumed that graphite has reappeared in the course of the annealing. Full details will speedily be given in the *Recueil des travaux chimiques des Pays-Bas*.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Pflanzenleben, Zweiter Band: A. K. von Marilaun (Leipzig, Bibliographisches Institut).—British Fungi: G. Massee (L. Reeve).—Studies in American History: M. S. Barnes and E. Barnes (Boston, Heath).—A Text-book of the Science of Brewing: E. R. Moritz and G. H. Morris (Spon).—The Universal Atlas, Part 8 (Cassell).—Geological and Natural History Survey of Canada, Annual Report, vol. iv., 1888-89 (Montreal, Foster, Brown).—Daily Weather Charts to illustrate the Tracks of Two Cyclones in the Arabian Sea (Eyre and Spottiswoode).—Meteorological Charts of the portion of the Indian Ocean adjacent to Cape Guardafui and Ras Hafún (Eyre and Spottiswoode).—Arithmetical Physics; Part 2*a*, Magnetism and Electricity; 3rd edition: C. J. Woodward (Simpkin).—A Graduated Course of Natural Science, Part 2: B. Loewy (Macmillan).—Delagoa Bay, its Natives and Natural History: R. Monteiro (Philip).—Observations made at the Hong Kong Observatory in the Year 1890: W. Doberck (Hong Kong).—Light, an Elementary Treatise: Sir H. T. Wood (Whittaker).—A First Book of Electricity and Magnetism: W. P. Maycock (Whittaker).—The Alkali-Maker's Hand-book; 2nd edition: G. Lunge and F. Hurter (Whittaker).—The Practical Telephone Hand-book: J. Poole (Whittaker).—The Plant World: G. Massee (Whittaker).—T. Cooke and Sons' Catalogue of Astronomical and Scientific Instruments, 1891 (York).—A Text-book of Physiology, Part 4, 5th edition, revised: Prof. M. Foster (Macmillan).—Iconographia Floræ Japonicæ, vol. i. Part 1: Dr. R. Yatabe (Tokyo, Maruya).—Catalogue of the Michigan Mining School, Houghton, Michigan, 1890-91 (Houghton).—Manual for the Physiological Laboratory; 5th edition: Drs. Harris and Power (Baillière).—Die Entstehung der Landtiere, ein Biologischer Versuch: Dr. H. Simroth (Leipzig, Engelmann).—Revisio Generum Plantarum, Pars 1: Dr. O. Kuntze (Dulau).

PAMPHLETS.—Proposed Railway through Siberia: W. M. Cunningham (London).—Molecular Motion in the Radiometer, in Crookes' Tubes and in some other Phenomena: D. S. Troy (New York, Hodges).—Oysters and Oyster-Fisheries of Queensland (Brisbane).

SERIALS.—Quarterly Journal of the Geological Society, No. 188, vol. xlvii., Part 4 (Longmans).—Morphologisches Jahrbuch, 17 Band, 4 Heft (Leipzig: Engelmann).—Quarterly Journal of Microscopical Science, No. 128 (Churchill).—Boletim da Commissão Geographia e Geologica do Estado de S. Paulo, Nos. 4-7 (S. Paulo).—Mittn. des Vereins für Erdkunde zu Halle a/S (Halle a/S).—Geological Magazine, November (K. Paul).—Jahrbuch der Meteorologischen Beobachtungen der Wetterwarte der Magdeburgischen Zeitung, Band ix. Jahrg. x., 1890 (Magdeburg).—Records of the Australian Museum, vol. i. No. 8 (Sydney).—Journal of the Chemical Society, November (Gurney and Jackson).—Sitzungsbericht der k. Akademie der Wissenschaften Math. Naturw. Classe, xcix. Band, Abthg. 1, Heft 4-10; Abthg. 2*a*, Heft 4-10; Abthg. 2*b*, Heft 4-10; Abthg. 3, Heft 4-10 (Wien).

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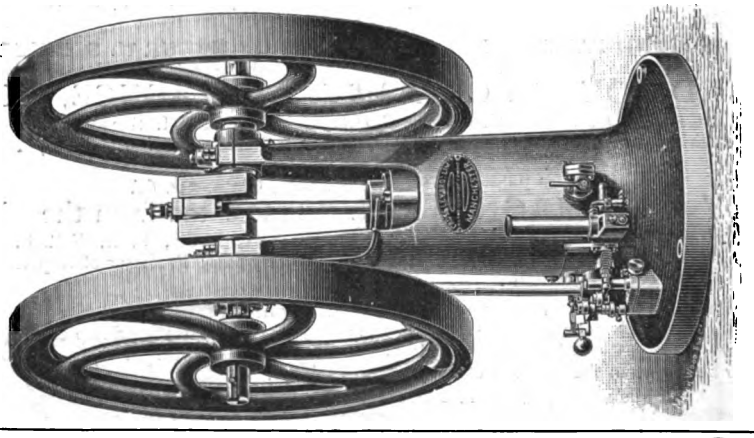

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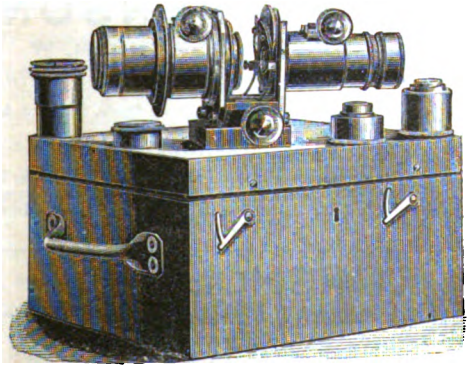

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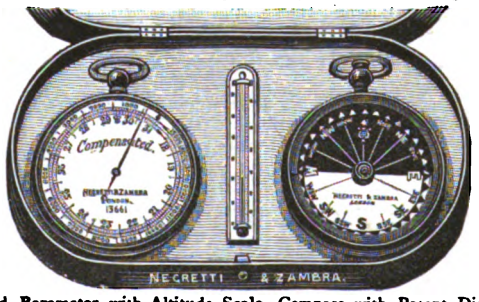

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THURSDAY, NOVEMBER 19, 1891.

SCOTCH FISHERIES.

The Ninth Annual Report of the Fishery Board for Scotland: being for the Year 1890. (Edinburgh: Printed for Her Majesty's Stationery Office by Neil and Co., 1891.)

THIS, like the three preceding Reports, is published in three parts: I. General Report; II. Report on Salmon Fisheries; III. Scientific Investigations.

The first part deals with such matters as the statistics relating to fish landed and cured, Crown brands, number of boats and men fishing, an account of the services rendered by the various vessels employed in marine police and fishery superintendence; and generally, reports on all business matters connected with this section of the Board's work. The Shetland herring fishing seems to have been particularly successful, and to have attracted the usual number of English and Irish boats, but the reports as to the number of Scotch boats employed in this and long-line fishing tend to show that there is a gradual decrease; that the fishing does not now seem to hold out such attractions to the rising generation as it once did; that probably over-competition is telling upon this as well as other industries.

The chief article in the second part is the annual report of Mr. Archibald Young, Inspector of Salmon Fisheries for Scotland. It contains an account of the fishings in the various rivers and lochs, with answers to queries from district boards and local fishing authorities. The question of the advisability of having a close-time for trout is only casually mentioned; and we cannot but think that the Board would do well if, with all the facilities at its command, it paid more careful attention to such matters as the life-history and habits of the salmon. Many points of great interest and usefulness have still to be settled: for instance, we see no attempt to distinguish between the different "runs" of fish in the various rivers, their spawning periods, and subsequent movements. We are not told whether, in the case of the salmon hatcheries, any attention has been paid to the spawning of spring run fish—probably a point of great importance in early rivers. Might the Board not collect and publish much valuable information as to the best means of keeping up a steady supply of Salmonidæ, by looking at the question a little more with the interest of the naturalist and sportsman? A great deal of information is yet required as to late and early spawning, migratory movements of the young, rate of growth in the sea, food, &c. To carry out some of these inquiries might possibly require more legal power than the Board possesses, but that the Fishery Board is in a more favourable position than other fishery authorities will be generally admitted. These excellent reports as to the state of the salmon fishings should be used more as a means for improving that state than as the finished results of a year's inspection.

The third or scientific section of the Report is a volume of nearly 430 pages. The word *scientific* has, with the Fishery Board, a significance of its own: it covers a multitude of subjects, but is on the whole a convenient title. Part III. is subdivided into four sections. In the first (A.), after detailed tabulated results of the work done by means of the cruiser *Garland*, &c., there are one or two papers deserving of special mention.

A paper by Dr. Fullarton, "On the Suitability of Scottish Waters for Oyster Culture," is an exhaustive account of an expedition made by him to a great number of lochs on the west coast, and shows clearly that some of these lochs formerly produced an abundance of oysters, and have had, within recent years, an abundant fall of spat on their spawning beds. A table of temperatures and densities is also given, which is valuable, but would certainly have been much more so if the temperature readings had been given in the common Fahrenheit instead of the Centigrade scale. The next paper is by the Secretary for Scientific Investigations, Dr. T. Wemyss Fulton, on "The Capture and Destruction of Immature Sea Fish." The two most important parts of this paper deal with the vitality of trawled fish, and the numbers of immature fish taken by shrimpers. It is the first attempt ever made to collect accurate data as to the proportion of living and dead fish brought up in a trawl net worked upon a certain kind of bottom for a certain length of time. The information regarding the number of immature fish taken by shrimpers has been collected in the Solway Firth and on the Lancashire coast. The results cannot but be surprising to very many. Taking the Solway alone, Dr. Fulton's totals show that, in one year, a single boat captures over 110,000 immature plaice. It is gratifying to learn that in this district the fishermen are, for once, provident, and return to the sea all these little fishes, for the sum-total of all immature fish landed in the year, comes to about 3,653,000. Dr. Fulton finds that, owing to the short time the shrimp trawl remains at the bottom—a sandy bottom—none of the young fish die. This contradicts a statement very generally made by those who denounce the system of fishing with beam trawl, and also is exactly the reverse of the finding of MM. Giard and Roussin in their report on this subject to the French Minister of Marine—a report founded, however, not on experimental observation, as Dr. Fulton does not fail to point out. These observations further seem to show that very young fish caught in a shrimp trawl are much more tenacious of life than older fish taken in a large net of similar construction, and under similar conditions. This point, we think, is of some importance, and has not been sufficiently taken notice of. It bears on the question of size of mesh and destruction of small fish taken.

The biological section opens with a long paper, "On the Food of Fishes," by W. Ramsay Smith, B.Sc., and, following this, another paper by Dr. Wemyss Fulton, on "The Comparative Fecundity of Sea Fishes." In this paper the author first deals with the proportional weight of the ova compared to the rest of the body. This naturally leads him into the somewhat complicated subject of the proportion of eggs ripe at the spawning-time of certain fishes which do not shed their spawn *en masse*. He finds that in such cases it would often be an impossibility for the body of the fish to contain all the eggs it naturally produces in an enlarged and ripe condition. Taking

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the instance of a plaice, it is shown that, if all its ova were ripe at once, the mass of eggs would themselves weigh a pound and a half heavier than the body of the fish without the ovaries.

Fish with demersal ova are distinguished from fish with pelagic ova, and tables are constructed which show the ratio of the weight of the ova present at one time, to the weight of the rest of the fish taken at 1000. The final results are given in the form of mean ratios. Dr. Fulton then goes on to draw three conclusions from his data. First, he says, it appears "to explain the majority of cases in which the females of a species are in excess of the males." What precisely explains this we are quite at a loss to see, and we cannot imagine an explanation which has not in it either a statement regarding the preponderance of the female element in the early nuclear plasma of the eggs, or an account of a wholesale destruction of males. The second point is the generally greater size of the female, and the third, "not merely the gradual growth of ova to replace mature ova shed during a prolonged spawning period, but the more or less sudden increase of bulk, which occurs in the ovum shortly prior to its extrusion." The second point seems to us to be like the first in requiring a more complete explanation; the third to be the clearest point advanced. The remainder of the paper is taken up with detailed statements, treating the fish according to their classification.

Among the papers following we have examples of Mr. Scott's conscientious systematic work in his second paper on "The Invertebrate Fauna of the Inland Waters of Scotland," and "Additions to the Fauna of the Firth of Forth."

Dr. Fullarton contributes a paper on "The Development of the Plaice—Preliminary Report." There are a number of excellent figures, but as there seems to be little that is new in the text, we do not give it further mention.

Prof. McIntosh adds to his already long list of observations "On the Life-Histories and Development of the Food and other Fishes." Interesting forms, such as a hybrid brill, lesser weaver, and sand-eel, are dealt with, as well as several unknown eggs and a curious unknown post-larval form.

Prof. Prince, so often associated with Prof. McIntosh, follows with "Notes on the Development of the Angler Fish (Lophius)."

In his statement that "hitherto no British observer has secured the ova," he has overlooked the fact that, in some previous notes by one of the naturalists to the Fishery Board, the procuring of a mass of ova was recorded. This, of course, in no way detracts from the interest of Prof. Prince's valuable paper.

The biological section closes with a note on "A Case of Hermaphroditism in a Haddock," by W. Ramsay Smith. Both ovary and testis appeared perfectly normal, and were removed from a fish 18 inches long, and 3 pounds in weight.

The physical investigations of the Board are dealt with by Dr. Mill in Section C.; and a review of the contemporary scientific fishery investigations, by Dr. Wemyss Fulton, forming a fourth section, brings the Report to a close.

THE MAMMALS OF INDIA.

Catalogue of Mammalia in the Indian Museum, Calcutta. By W. L. Sclater, M.A., F.Z.S., Deputy Superintendent of the Indian Museum. Part II. (Calcutta: Printed by order of the Trustees of the Indian Museum, 1891.)

THE Indian Museum at Calcutta is rich in Mammals. Not only are those of our Eastern possessions well illustrated, but it possesses also a good general series from other parts of the world. The collection has, moreover, the advantage of being well catalogued. In 1863, the late very zealous and acute zoologist, Edward Blyth, published a catalogue of the specimens contained in the Museum of the Asiatic Society of Bengal. This Museum, when transferred to the Government of India, formed the nucleus of the present Indian Museum. The 1330 specimens mentioned in that catalogue have now increased to 4872, representing 590 species, of which 276 are found within our Indian Empire, and 314 are exotic.

Of this greatly augmented collection Dr. John Anderson commenced a catalogue, and the first part, containing the orders Primates, Prosimiæ, Chiroptera, and Insectivora, was published in 1881. In consequence of Dr. Anderson's relinquishing his appointment as Superintendent of the Museum, the work has remained in abeyance for some years; but it has now been taken up and completed by Mr. W. L. Sclater, the present Deputy Superintendent, and eldest son of the distinguished Secretary of the Zoological Society of London. This volume contains the orders Rodentia, Ungulata, Proboscidea, Hyracoidea, Carnivora, Cetacea, Sirenia, Marsupialia, and Monotremata.

The Mammals of our Indian Empire have attracted the attention of many well-qualified zoologists. Hodgson, Blyth, Jerdon, Tickell, Horsfall, Elliot, Dobson, Anderson, and others, have contributed much to elucidate their history, habits, and distribution. The work now being published under the auspices of the Indian Government by Mr. W. T. Blanford, the first part of which appeared in 1888 (see NATURE, vol. xxxviii. p. 513), contains a valuable summary of all that is known upon the subject up to the present time. Mr. Sclater's work is of a less ambitious kind, professing to be only a catalogue of the Mammalia contained in the Museum, not mentioning any other species. Such catalogues are not only invaluable for working purposes in the institution itself, but they have also a more extended area of usefulness, being often works of reference which no zoologist investigating the group they treat of can dispense with. In the present case there will be found under the heading of every species much information as to its literature, synonymy, and geographical distribution. As catalogues naturally deal largely with names, the selection of those which accord best with a common-sense interpretation of the rules of zoological nomenclature is a matter of primary importance, and in this respect Mr. Sclater appears to have shown upon the whole great judgment, having been careful to avoid unnecessary alterations in generally accepted names, either such as are caused by splitting genera, or by reviving obsolete, long-forgotten, or never received specific appellations. Although no de-

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tailed specific descriptions are attempted, the work is rendered more useful than a mere list would be, by the introduction of keys, by means of which all the Indian species can be discriminated. There are also some critical remarks upon disputed questions of specific distinction, which the large series of specimens at the author's disposal has enabled him to throw light upon, such as the identity of *Ovis poli* of the Pamir and the so-called *Ovis karelini* of the Thian Shan. Under the heading *Elephas indicus*, we note that Mr. Sclater refers to Schlegel's having pointed out in a well-known memoir (of which a translation appeared in the *Natural History Review*, vol. ii., 1862) certain distinctions between the true Indian elephant and that inhabiting the islands of Ceylon and Sumatra (*Elephas sumatranus*, Schlegel), and he repeats the characters assigned to the two supposed species or varieties. Although no fresh evidence is brought forward in favour of Schlegel's views, it is not likely that Mr. Sclater would, without good reasons, reject Dr. Falconer's elaborate refutation of them, published in the succeeding volume of the same Review. Dr. Falconer was such a great authority on elephants, and his arguments for the specific unity of the Asiatic forms have been so generally held to be sound, that Schlegel's two species can only be rehabilitated by a careful comparison of a considerable series of specimens undoubtedly natives of both localities. Perhaps Mr. Sclater may have an opportunity of doing this while in the East, and thus definitely settle a question of considerable zoological interest.

W. H. F.

A TEXT-BOOK OF CHEMICAL PHYSIOLOGY AND PATHOLOGY.

A Text-book of Chemical Physiology and Pathology. By W. D. Halliburton, M.D., B.Sc., M.R.C.P. (London: Longmans, Green, and Co., 1891.)

IN spite of the fact that several standard works on the subject of physiological chemistry exist, both in German and English, the need has nevertheless been universally felt of one that should at the same time present a review of the present condition of the subject from an impartial standpoint, and give some account of the methods of research employed.

Hoppe Seyle's works have been of immense service, but suffer from being onesided, and representing only the views and methods of the Strassburg school. The only work in English which promised to be universal in its scope—namely, that by Gamgee—is unfortunately still unfinished.

Prof. Halliburton, who is justly celebrated for his work in all departments of physiological chemistry, has attempted to fill this gap in our literature, and with a large measure of success.

The first fifty pages of the book are taken up with an account of the apparatus and analytical methods chiefly employed in physiological chemical research. The only fault we have to find with this part of the book is that there is not enough of it. In a book intended as a guide to those who would work practically at the subject one hundred and fifty pages might well be devoted to these subjects, seeing that so many workers boldly attack the chemical problems of physiology with scarcely any practical knowledge of chemical analytical methods.

The second part treats of the chemical constituents of the organism, concluding with two chapters on fermentation and ptomaines, the chapters on the latter and on proteids being especially good, and presenting an excellent *résumé* of our present knowledge of these subjects.

The next section is taken up with an account of the tissues and organs of the body. Here the author is thoroughly at home, and can speak with the authority of many years' practical work at the subject. It is rather difficult, however, to see on what principle he includes respiration in this part, especially as the subjects of alimentation, excretion, and general metabolism have each a part to themselves; unless it be, that it is so intimately connected with the physiology of the blood. In this chapter a student might be led astray by seeing the table of relations between the tension of the gases in venous blood and of those in the alveolar air. The important thing to know is the tension of gases in arterial blood; and by giving those in venous blood in juxtaposition to those in the alveolar air, the author glosses over the difficulties presented by the question of gas interchange in the lungs. In this connection, too, he does not notice Bohr's important work on the subject (interchange of gases in the lungs), although he gives a full account of the Danish physiologist's researches on the combination of hæmoglobin with CO_2.

In the latter part of the book no reference is made to Altmann's views on fat absorption, or to Ehrlich's suggestive work on the oxidative processes taking place in living tissues.

But a few errors of omission are inevitable in a work of this size and scope, and Dr. Halliburton wins our admiration for the completeness and correctness of his book, which everywhere shows signs of the care with which the proof-sheets have been revised and brought up to date. The accounts of recent analytical methods and work render it invaluable in a physiological laboratory, and it will be repeatedly referred to by students who desire more than a superficial knowledge of the subject. In Germany it has already found favour with physiologists, and is considered the best work on the subject. The fact that it is being translated into German, under the auspices of Prof. Kühne, is of itself sufficient recommendation for any work; and there is no doubt that in its new dress it will command as much success in Germany as it has already commanded in England.

E. H. STARLING.

OUR BOOK SHELF.

Praktisches Taschenbuch der Photographie. By Dr. E. Vogel, Assistant in the Photochemical Laboratory of the Technical High School of Berlin. (Berlin: Robert Oppenheim, 1891.)

THIS is a small volume, of some 200 pages, but it is full of useful information for working photographers, whether amateurs or professionals. Under nine sections the author treats of all the subjects likely to be required by the manipulator of the camera, from the purchase of his apparatus onward through every detail essential for successful work. The value of the book is greatly enhanced by numerous illustrations, which are

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executed with that clearness and finish for which so many Continental scientific works are justly to be commended. To give an idea of its contents it will be sufficient to mention the headings of the sections, viz. apparatus for the negative process, photographic objectives, instantaneous shutters, portable cameras, equipment of the dark room, general remarks on exposure, negative processes, positive processes, cyanotype and similar processes. The work, as its title implies, is purely technical, and, as such, does not call for lengthened notice in these columns, but for the particular object with which it has been written it is admirably adapted, and should find many readers in this country. We have nothing which can be compared with it for conciseness and completeness.

An Introduction to the Differential and Integral Calculus. By T. Hugh Miller. (London: Percival and Co., 1891.)

THIS small book contains a fair amount of the calculus put together in a clear and readable form. It merely touches the subject, but appears to contain enough to meet the wants of a South Kensington examinee. "It assumes a knowledge of elementary algebra and trigonometry as far as the properties of plane triangles." The student is supposed to be unacquainted with analytical geometry, but as he is credited with a knowledge of the exponential and binomial theorems, with "indeterminate coefficients" and a few other matters, it will be seen that *elementary* includes a fair grasp of the two subjects named. Six chapters are devoted to the elements, successive differentiation, the theorems of Leibnitz, Taylor, and Maclaurin, maxima and minima values of a function of one variable, and the evaluation of indeterminate expressions; the remaining four chapters are devoted to elementary integration, formulæ of reduction, rational fractions, and a few applications of the integral calculus. We presume that the miscellaneous examples are taken from South Kensington papers; those in the text are old friends which figure in Todhunter's works. In the text the following slips occur: p. 4, l. 15, for $f(x)$ read $f'(x)$; p. 18 (6), read e^{x^2}; p. 37 (3), for $\frac{x^6}{720}$ we get $\frac{x^6}{5}$; p. 40 (3), ? $(a-b)^2/a$ for the maximum; p. 41, l. 4 up, for $2a+3b$, read $3a+2b$; p. 42 (1), read $\cos^3\theta$ and $3\sqrt{3}d^2/16$; p. 62 (4), ? last connecting sign (read $-$); p. 71 (4), for π read π^2; p. 80 (24), in first place read $(1+x^2)^2$. In the answers, we differ from the author in (1), (20), (74), and (88). We prefer to work (84) from $\int (1+t^2)^{\frac{3}{2}}dt$, where t stands for tan x.

Star Groups. By J. Ellard Gore. (London: Crosby Lockwood and Son, 1891.)

A KNOWLEDGE of the principal constellations visible in our latitudes may be easily acquired from the thirty maps and accompanying text contained in this work. All stars down to the sixth magnitude are shown, and brief descriptions given of the objects of interest in each constellation. The maps are intended to be useful as an introduction to larger atlases, and will doubtless serve this purpose well; but a beginner unacquainted with the motions of the heavenly bodies will hardly find in them what he requires. G.

The Universal Atlas. (London: Cassell and Company, 1891.)

THIS atlas is being issued in twenty-eight parts, including the index, eight of which have already appeared. It contains fifty-eight single page maps and thirty-two double page, several illustrating physical geography. The maps are well drawn and reproduced, and full of detail, whilst their large scale has enabled the names of all places of any importance to be printed with perfect legibility. In fact, all who require a good atlas, for reference or otherwise, would do well to obtain this one. G.

La Transcaucasie et la Péninsule d'Apchéron. Calouste S. Gulbenkian. (Paris: Hachette et Cie., 1891.)

THIS is a very pleasant book of travels, well worthy of the attention of all who for any reason take interest in the Caucasus. The author has no very stirring adventures to tell us of, but he presents lucid and attractive descriptions of the towns and districts through which he passed, and of the manners and customs of the inhabitants. Especially good are the chapters he has devoted to the petroleum industry—chapters which have already appeared in the *Revue des Deux Mondes*. He gives also a very interesting account of Oriental carpets, the manufacture of which plays so great a part in the Caucasus.

How to Organise a Cruise on the Broads. By E. R. Suffling. (London: Jarrold and Sons, 1891.)

IN preparing this little book, the author did not attempt to provide a guide to the Broads. He intended the volume to serve merely as a supplement or appendix to the various guides already accessible. A cruise on the Broads is heartily enjoyed by everyone who tries it under tolerably favourable conditions, and certainly not least by students of natural history. Anyone who may think of making the experiment will find in Mr. Suffling's pages all the information that is really necessary for the formation of suitable plans. In one chapter he presents a brief and interesting diary of what may be looked for at the Broads during the various months of the year.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

A Difficulty in Weismannism.

I HAD intended to accept Prof. Hartog's challenge, and say a few words on this subject at an earlier date, but absence from home and many engagements have interfered until now.

In some respects it would have been more convenient to defer such a discussion until Weismann's last essay, "Amphimixis," has become more widely known, or even until the appearance of his complete and detailed work, which is expected some time next year. Prof. Weismann tells me that the points raised by Prof. Hartog are considered in this treatise, and, such being the case, he is unwilling to tax his already over-strained eyesight with any earlier reply.

As the question has been raised, I will briefly speak of the manner in which I have tried to see my way through such difficulties. I do not, however, wish to involve anyone else in the responsibility for the attempt, which is no doubt crude and insufficiently thought out.

Accepting Prof. Hartog's five theses as fair statements, I have always proceeded to make his hypothesis B, and in this I believe I am following Prof. Weismann. Hypothesis A had never occurred to me, and I agree with Prof. Hartog in considering it as valueless But I believe a way through the difficulties raised against hypothesis B may be found in the assumption of a relationship between the Ahnenplasmas in the germ-cell. Such a relationship is perhaps hinted at by Prof. Hartog in Thesis III., where he speaks of these units as lying "associated together," and in this respect the metaphor of two packs of cards in Thesis IV. is, I believe, inadequate. I have always been accustomed to regard the relationship between the ancestral units, the "pattern" or figure which they form, as an essential part of the process. I have regarded the units as the necessary material, like the pigments in a colour-box, while their arrange-

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ment would correspond to a finished picture. With such a conception I should prefer in Thesis IV. the metaphor of a kaleidoscope. The pattern at any one point will determine the pattern that succeeds, although, with an infinite number of pieces, the latter must always be different. But though differing, the successive patterns will resemble each other far more closely than those which are separated by wide intervals. Similarly, I do not think it is inconceivable that the arrangement of the ancestral units may have a determining effect on the arrangements which will succeed, in spite of the loss and restoration of half the units in each generation. Such a conception has the further advantage that it renders intelligible the action of external conditions on the germ-cells, either directly or through the medium of the body-cells. The ancestral units may be excessively stable, but the arrangements may be modified by a shock, just as the pattern in a kaleidoscope may be changed by a blow instead of the "normal" process of rotation (corresponding, of course, to the loss and restoration of half the units).

Oxford, November 16. EDWARD B. POULTON.

Town Fogs and their Effects.

THE influence of fogs on health, referred to in the very interesting paper by Dr. Russell (NATURE, November 5, p. 10), seems to call for further investigation. On the face of it, and judging by the composition of fogs, the discomfort they bring, their hurtfulness to plants, &c., fogs must surely damage health. And the injurious effect, I would point out, might not be *at once* apparent in the death-rate. What, on the other hand, is the precise nature of the beneficial effect of fogs (for such there seems to be)? If they plague mankind, they probably also plague those enemies of mankind, the minute organisms on which disease depends. And if so, we might even suppose some lives to be saved when fog comes on. It would be interesting to hear from hospitals for special diseases, how the patients are affected by fog. I understand that people suffering from asthma often rather enjoy a fog, or the sulphureous atmosphere of the Underground Railway. Has this ever been explained? M.

The Eclipse of the Moon.

I VENTURE to send some notes upon last night's eclipse of the moon, taken by me here up to 11.35 p.m., when the sky became rather suddenly and entirely overcast.

The first indication of the penumbra of the earth's shadow was distinctly visible upon the north-east limb of the moon a little before 10.25; and at 10.35 (time given by the almanac) her north-east limb was well in shadow, and hidden by a remarkably dense or black shadow. At this time the sky here was quite clear, and promised to keep so for some time. At 10.45 the shaded part of the moon was so dark as to be invisible upon the sky even through glasses. At 10.50 a very beautifully coloured prismatic "cock's eye" formed in the sky exactly opposite the shaded limb, taking a fan-like shape radiating from that side of the moon; the prismatic colours being repeated twice, as in a double quadrant of a rainbow; while the sky round the bright part of the moon was clear and uncoloured. At 10.55 a thin white cloud, with ring of prismatic colours, formed round the moon; the earth's shadow still remaining very dark, with well-defined edge, and little or no penumbra beyond it. At 11.5 the thin cloud entirely cleared, the shadow still very dark, the upper and lower edges of the moon's limb just visible as threads of light upon the sky; and at 11.10 a very slight warmish tint appeared about the north-east part of shadow. At 11.15 the sky very clear and dark about the moon, stars before invisible coming out brightly. The earth's shadow was now well advanced over the moon, strongly defined, and as dark as the sky beyond it. At 11.22 light thin clouds again gathered round the moon, a narrow crescent of her only remaining. At 11.25 the moon became wholly hidden by a dense cloud. At about 11.35 I caught a momentary glimpse of the moon through the cloud, a very small part of her south-west limb just showing. At 11.40 sky entirely overcast; a faint aurora or red colour spreading upward, apparently below this cloud or mist, from the north.

I have only to add that the *darkness* and *absence of colour* of the shaded part of the moon was even more marked in this eclipse—so long as I was able to observe it—than in that of October 4, 1884, which was then set down to an abnormal density of the earth's atmosphere, and was supposed to have some connection with the strange sunsets and other phenomena of that period. I was not altogether unprepared for the same general character in last night's eclipse, as, so far as my own observations go, I am led by them to believe that the conditions then present in our atmosphere are still with us.

Southampton, November 16. ROBERT C. LESLIE.

Comparative Palatability.

THE following observations were made during the last week of September and first of October:—

Two tadpoles of the small newt were taken by a "silver-fish." Three others, placed next day in the globe containing this fish and two goldfish, were not swallowed, though attempted from time to time. A brandling (*Allolobophora fœtida*) was once taken by the same silver-fish; but refused the next day and afterwards.

A large frog (♀) ate brandlings readily. Two slugs were taken by frogs. Tadpoles of the small newt were disregarded.

A very interesting experiment was made with a brimstone butterfly (*Rhodocera rhamni* ♂). It was offered to a frog which had just taken a *V. urticæ*. Though fairly seized several times, the brimstone was always rejected. After one rejection, a second *V. urticæ* was swallowed; after another, a *Spilosoma* larva. The butterfly was then given to a spider, which attacked it, but left it unwound. A *V. urticæ* placed in the web was at once seized, partially wound, and sucked. Then the spider returned to the brimstone; but immediately left it again for the *Vanessa*, which was thoroughly wound, sucked, and moved higher up into the web. At dusk, the brimstone had been very imperfectly fastened. Next morning, however, it had been taken up by the spider.

That a frog is not much hurt by the nippers of *Ocypus* is shown by the following experiment. A specimen which had been taken from the side seized the frog's tongue, was rejected after a few minutes, and removed by the forceps. The frog immediately after took a large earthworm.

Small frogs are exceedingly bold and voracious; often attacking prey which is as large as themselves, and which they could not possibly swallow. House- and harvest-spiders, hairy and smooth larvæ (among them those of *Spilosoma* sp. and *Mamestra persicariæ*), ladybirds, earthworms, brandlings, and silver-Y moths, were all swallowed somehow; while large "devil's coach-horses" were invariably attacked. Tadpoles of the small newt were disregarded. E. B. TITCHENER.

Inselstrasse 13, Leipzig.

The Inheritance of Acquired Characters.

WILL you allow me to call the attention of your readers to a sentence in Mr. Hemsley's review of Schimper's and Karsten's works on the mangrove vegetation? "Mangroves grown in soil free, or practically free, from chloride of sodium, develop foliage of less substance, furnished with a larger number of stomata." If this means, as I understand it, that the change takes place immediately with the change in the conditions of growth, it would be very interesting to have further details; as the fact would furnish a very strong argument that the peculiarities in the mangrove vegetation are the result of the inheritance of acquired characters. ALFRED W. BENNETT.

St. Thomas's Hospital, November 7.

"The Darwinian Society."

IN your issue of November 5 (p. 19) information is given that a local Society is about to be inaugurated in Edinburgh, under the title "The Darwinian Society."

As the Society is apparently to be merely for the encouragement of the study of natural science in the University of Edinburgh, the name is surely too pretentious to be suitable; and it is one that might well, I think, be kept in reserve for bestowal in later years upon a chartered Society of similar magnitude and as far-reaching extent as that founded in honour of Linnæus. It is therefore to be hoped that a more applicable name than the one proposed may be found for the new Edinburgh University Society. WILLIAM WHITE.

Sheffield, November 10.

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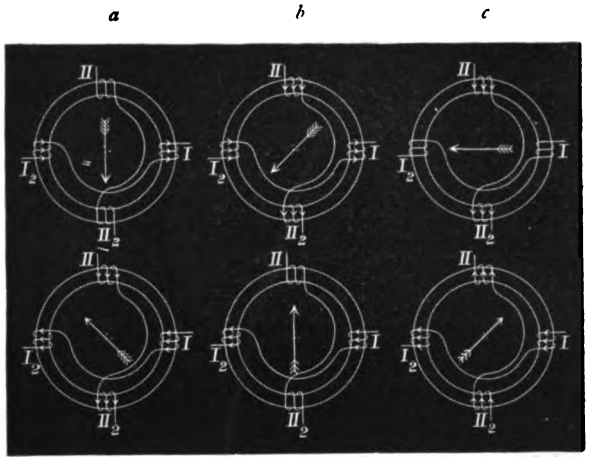
SOME NOTES ON THE FRANKFORT INTERNATIONAL ELECTRICAL EXHIBITION.[1]

V.

The Evolution of the Multiphase Alternate Current Motor.

THE two-phase alternate current motor described in Part IV. has the disadvantage that, not merely does the magnetic field rotate, but it also varies in strength: this causes the driving force to fluctuate, and diminishes the power that the motor would otherwise give out. That there is this variation in strength of the magnetic field



[FIG. 12 (repeated).—Rotating magnetic field produced by two alternating currents, differing by 90° in phase.

may be seen from Fig. 17, where the continuous curves I and II show at any moment the strengths of the currents in the coils I and II, Fig. 12 (repeated for reference from the last article); while the dotted curves I_2, II_2, in Fig. 17, give the values at any moment of the currents in the coils I_2, II_2, in Fig. 12. For example, when the time equals *a* (Fig. 17), the currents flowing in the coils I and II have respectively their maximum value and nought; whereas when the time equals *b*, the currents flowing in each of these coils is the same, being equal to $2 \times \sin 45^\circ$ into the maximum value.

Now if each of the four coils occupies only a small portion of the ring, as shown in Fig. 12, the currents in all the four coils will always help one another to magnetize the iron ring, hence the magnetizing force at any moment will be approximately proportional to the number of convolutions in one of the coils multiplied by the *arithmetical* sum of the ordinates of all the four coils I, II, I_2, and II_2, that is, multiplied by twice the ordinate of the upper or summation curve.

If the maximum ordinate of either of the curves I or I_2 be called H, the ordinate of the upper or summation curve is equal to H when the time is *a*, *c*, or *e*, corresponding with the illustrations marked *a*, *c*, and *e* in Fig. 12; whereas the ordinate of this summation curve is $\frac{2}{\sqrt{2}}$ H, or 1·414H, when the time is *b*, *d*, or *f*, corresponding with the illustrations marked *b*, *d*, and *f* in Fig. 12.

Hence, if K be the number of convolutions in one of the coils, the sum of the products of the current into the number of convolutions, or the *number of ampere-turns*, as it is called, will vary between two values proportional to HK and 1·414 HK—that is, will vary by 41·4 per cent. The variation in the magnetism produced by such a change in the number of ampere-turns will be less than 41·4 per cent., and much less if the magnetic induction be considerable; still, the fluctuation in the strength of the rotating field may be greater than is desirable.

If in place of the two pairs of coils (Fig. 12) there be three, I, I_2, II, II_2, III, III_2, as in Fig. 18, and if the alternating currents passing through these three circuits be of the same maximum altitude and periodic time, but differ by 60° in phase, the variation in the number of ampere-turns will be much less than if only two alternating currents be employed. For on examining the sum of the ordinates of the three continuous curves, I, II, III (Fig. 19), which are the curves of three such currents, we see that the sum, or the ordinate or the top curve, varies between 2H sin 60°, when the time equals *t*, and H + 2H sin 30°, when the time equals *t*. Hence this sum varies between $\sqrt{3}$H and 2H, corresponding with a change of only 14 per cent. in the magnetizing force, and with less than 14 per cent. in the magnetism produced.

Such a system, however, would require six wires, whereas the same reduced maximum variation of the number of ampere-turns can be practically attained by employing only three wires conveying three alternate

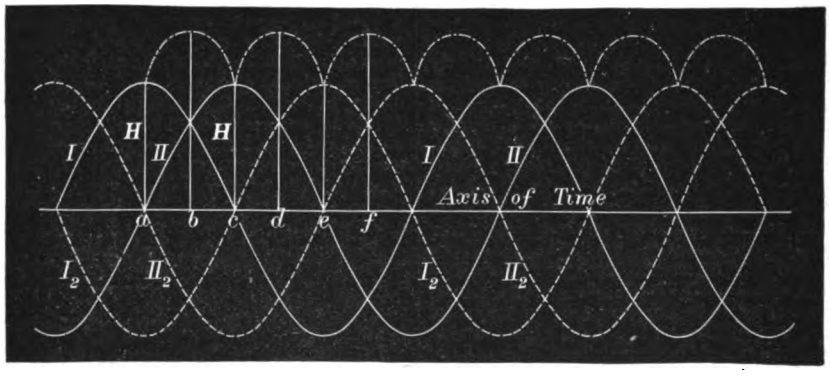


FIG. 17.—Two harmonic alternating currents of the same period and maximum amplitude, but differing by 90° in phase.

currents differing by 120° in phase, and by joining up the motor as shown in Fig. 20.

That it is possible to use only three wires, so that either wire always acts as the return wire for the currents in the other two, arises from the fact that the algebraical sum of three harmonic alternate currents of the same period and maximum amplitude but differing by 120° in

[1] Continued from vol. xliv. p. 619.

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phase is always nought. This may be easily proved thus: the values of three such currents are given at any moment by projecting on a stationary line, POQ (Fig. 21), the three equal limbs of the three-legged figure $O\alpha$, $O\beta$, $O\gamma$, as it rotates with uniform velocity round the point O. $O\alpha$, $O\beta$, $O\gamma$, are, therefore, the maximum values of the three currents, and the actual values of the currents for the position of the figure shown are OA, OB, OC, respectively, corresponding in relative magnitude with the lengths of the arrows which are attached to A, B, C, in Figs. 18 and 20, and in direction with the arrows attached to the latter figure, on the assumption that a current is regarded as positive when it circulates round the iron ring in such a direction as to tend to send a north pole counter-clockwise round the iron ring. Now the sum of the projections on POQ of any three lines $O\alpha$, $O\beta$, $O\gamma$ (Fig. 22) is simply the projection of $O\zeta$ found by drawing $\alpha\epsilon$ and $\epsilon\zeta$ parallel and equal to $O\beta$ and $C\gamma$ respectively. But if such a construction be made for Fig. 21, ζ and O will always coincide, therefore the sum of the projections of $O\alpha$, $O\beta$, and $O\gamma$, Fig. 21, must be always nought.

Fig. 23 shows three curves, I, II, III, drawn so as to give the values at any moment of three harmonic alternating currents each of the same altitude, H, and periodic time, but differing by 120° in phase, and it is seen that the sum of the three ordinates—that is, the ordinate of the top curve—varies from H + 2H sin 30°, when the time equals t, to 2H sin 60°, when the time equals t', so that the ordinate of the summation curve varies from 2H to $\sqrt{3}$H, corresponding with a variation of 14 per cent. But this is exactly the variation that we obtained in Fig. 19, hence if there be twice as many convolutions in each of the three coils of Fig. 20, as in each of the six coils of Fig. 18—that is, the same total number of coils in the whole ring—and if the three equal harmonic alternating currents differing by 120° in phase have each the same

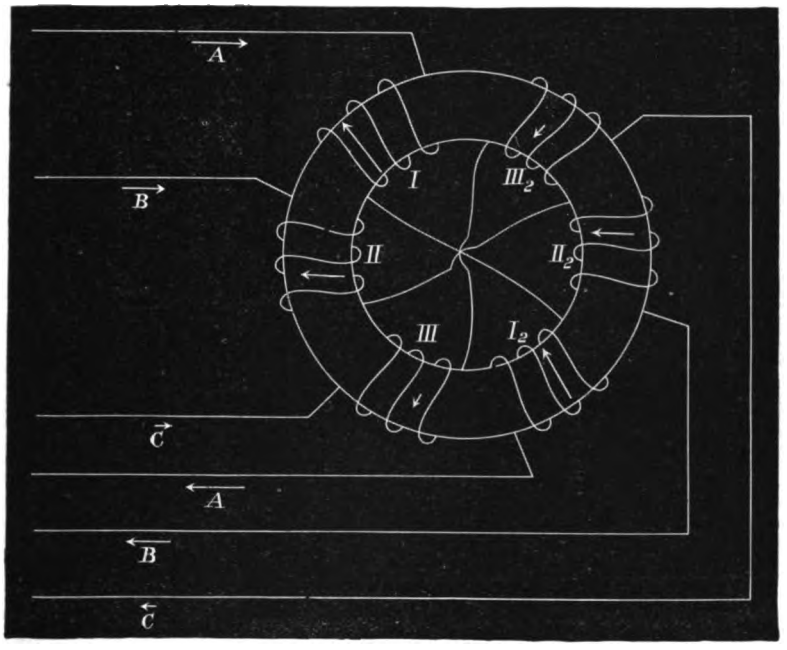

FIG. 18.—Currents differing by 60° in phase, and represented in direction and magnitude by the direction and length of the arrows.

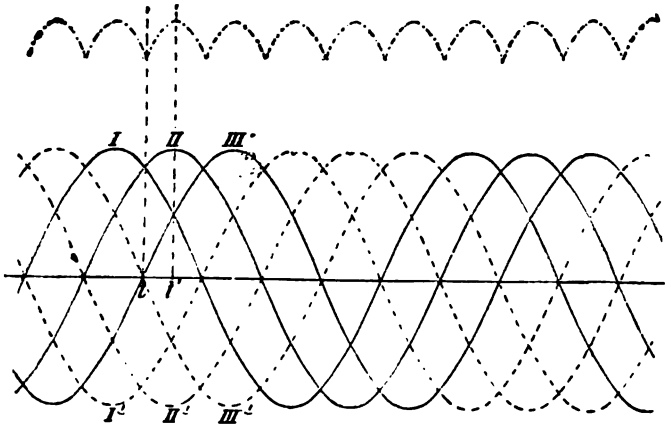

FIG. 19.—Three harmonic alternating currents of the same period and maximum altitude, but differing by 60° in phase.

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maximum amplitude as the three equal harmonic alternating currents differing by 60° in phase, there will be the same maximum variation in the number of ampere-turns in the two cases.

Mr. Dolivo Dobrowolski, the designer of the three-phase motor employed in the Lauffen-Frankfort transmission, was the first to draw attention to this variation in the number of ampere-turns with a rotatory magnetic field motor, and the pulsation of the field thus produced. But we venture to think that in his deductions he lays too much stress on the mere variation in the number of ampere-turns, and too little on the fact that the variation in the number of ampere-turns only imperfectly indicates the variation in the strength of the rotating field. That the one variation does not alone measure the other arises first from the induction in iron not being, as is well known, proportional to the magnetizing force; secondly, from the magnetic leakage in the motor being different when the axis of the rotating field cuts the ring between two of the coils, and when it passes through a coil conveying a current, a case which cannot, of course, be neglected when each coil occupies a considerable portion of the ring, as in an actual three-coil motor.

Hence we doubt whether it could be decided theoretically without experiment with which of the windings indicated in Figs. 12 or 20 the rotating magnetic field would undergo the greater variation in strength, if in both cases the windings occupied the whole of the iron ring.

Since, as we have proved, the algebraical sum of the three alternate currents in the coils I, II, III, Fig. 20, is always nought, Kirchhoff's second law suggests that these

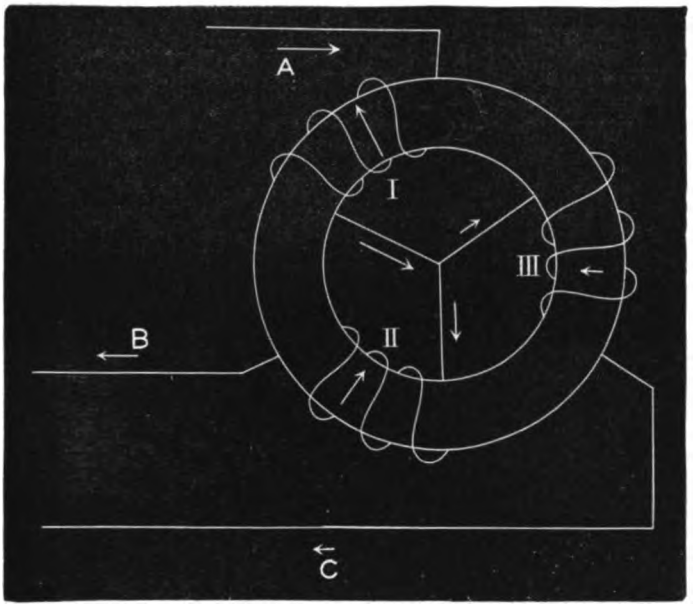

FIG. 20.—Three-phase alternate current motor (open winding); currents differing by 120° in phase, and represented in direction and magnitude by the direction and length of the arrows.

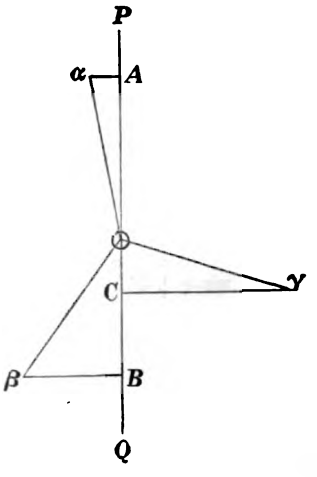

FIG. 21.

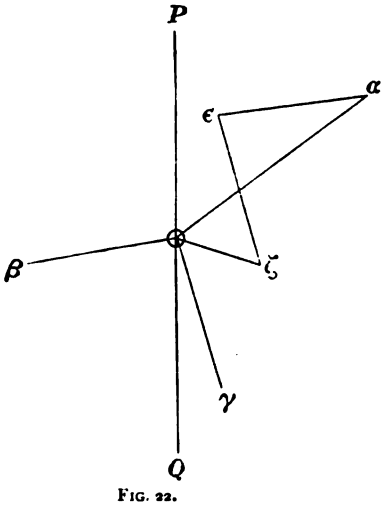

FIG. 22.

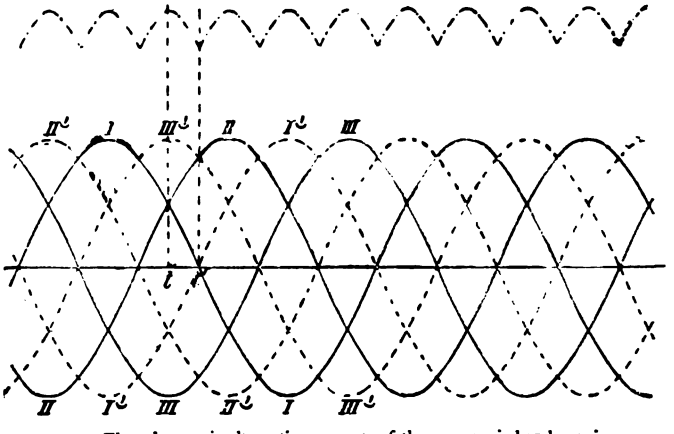

FIG. 23.—Three harmonic alternating currents of the same period and maximum altitude, but differing by 120° in phase.

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three coils, instead of being wound on the ring as indicated in Fig. 20, may be wound so as to form a closed circuit, as shown in Fig. 24. With this arrangement of coils it is easy to show that the current

$$\text{in I} = \frac{A+B}{3},$$

$$\text{„ II} = \frac{B-C}{3},$$

$$\text{„ III} = \frac{C+A}{3},$$

where A, B, and C represent simply the arithmetical values of the currents in the three main leads. And in Fig. 24 the arrows attached to the wires A, B, and C, and to the coils I, II, III, are drawn of such proportional lengths that the above connection between the currents

Since the three coils both for the open and the closed methods of winding (Figs. 20 and 24) are connected together, and since the current in any one coil varies like the current in the preceding coil, with a lag of 120°, each value of the current may be regarded as travelling round the ring from each coil to the next. This idea has led Mr. Dobrowolski to call such a motor a rotatory current or "*drehstrom*" motor.

In joining up a three-phase *drehstrom* motor, we have to decide whether we shall adopt the arrangement shown in Fig. 20 or that illustrated in Fig. 24. The latter, or closed winding, would be employed when we desired that the maximum potential difference between the terminals of any one of the three coils should be equal to the maximum potential difference between any two of the mains; while with the open method of winding (Fig. 20) the maximum difference of potential between the terminals of any one of the three coils would be only $\frac{1}{\sqrt{3}}$, or 0·5744

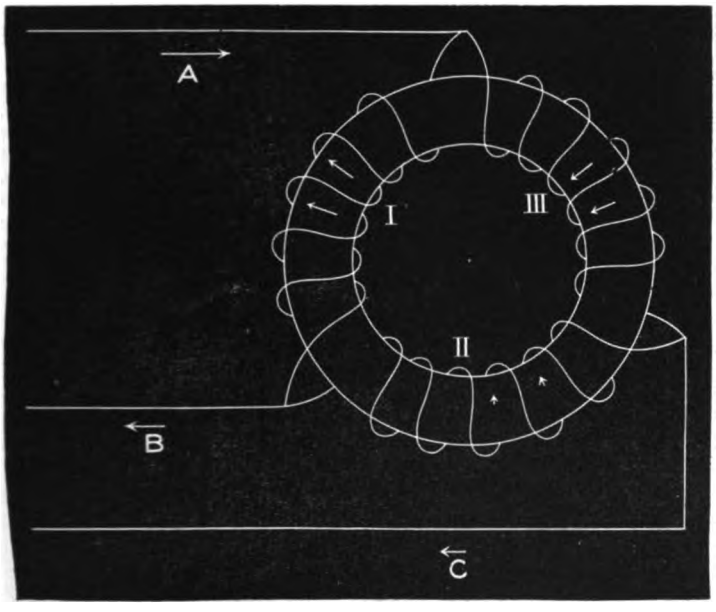

FIG. 24.—Three-phase alternate current motor (closed winding); currents differing by 120° in phase, and represented in direction and magnitude by the direction and length of the arrows.

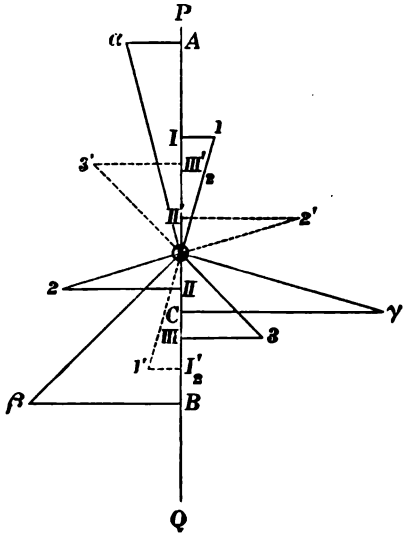

FIG. 25.—Projections of Oα, Oβ, Oγ give direction and relative magnitude of currents in the mains A, B, C of Figs. 20, 24, 27, 29, and 31, and of currents in coils I, II, III of Fig. 20. Projections of O1, O2, O3 completely represent currents in coils I, II, III of Fig. 24; and projections of O1, O2', O3 completely represent currents in coils I, II, III of Fig. 27. ½

in the three coils and the currents in the three mains is satisfied.

Next, to find the difference in phase, we draw three lines, so that their projections on POQ (Fig. 25) represent in direction and magnitude $\frac{OA+OB}{3}$, $\frac{OB-OC}{3}$, $\frac{OC+OA}{3}$, which is done by taking one-third of the diagonals of each of three parallelograms constructed respectively on Oα and Oβ *produced backwards*, on Oβ and Oγ *produced backwards*, and on Oγ and Oα *produced backwards*. In this way is obtained the three-legged figure, O1, O2, O3, with three equal sides making angles of 30° with Oα, Oβ, Oγ, respectively; then O I, O II, and O III, the projections of O1, O2, and O3 on POQ, give us the direction and magnitude of the three currents in the coils I, II, III (Fig. 24). Hence we see that the current in coil I lags 30° behind the current in A, the current in coil II 30° behind the current in B, and similarly the current in coil III 30° behind the current in C.

times the maximum potential difference between any two of the mains. The open method has the further advantage that the middle point where the single current branches into two (Fig. 20) can be permanently connected with the earth; so that, while the maximum potential difference between each pair of mains may be, say, 20,000 volts, the potential of no point of the whole system can ever differ from that of the earth by more than 10,000 volts, a result which of course enables the insulation of separate aërial conductors to be more easily carried out.

The open method of winding has therefore been adopted for the transformers at Lauffen and at Frankfort, as well as for the motor at Frankfort; but, for the reasons which follow, the actual winding employed is more complex than that indicated in Fig. 20.

In addition to the defect possessed by the two-phase alternate current motor arising from the variation in the strength of the rotating magnetic field, there is another defect caused by the rotation of the field not proceeding

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at a uniform rate, so that the driving force is intermittent. Both these defects, however, can be much lessened by subdividing up the coils wound on the iron ring of the motor, a result that can be attained without increasing the number of main wires beyond three by employing the following device. Imagine one half of each of the three coils of

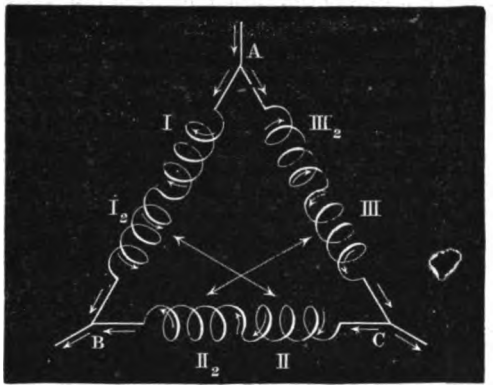

FIG. 26.—Transformation of a three-phase alternate current motor (closed winding), with currents differing by 120° in phase, into a six-phase motor, with currents differing by 60° in phase.

the motor in Fig. 24 to be wound in the opposite direction; then an arrangement, symbolically indicated in Fig. 26, would be obtained, where the six halves of the former three coils, I, II, III, are now called I, I_2, II_2, II, III, and III_2, as we go round the triangle, Fig. 26. If now, without separating any of the connections, the coils I_2 and II be made to change places, as well as II_2 and III, we obtain the arrangement of winding shown on the motor in Fig. 27.

Now it is to be observed that since the coils I and I_2 (Fig. 27) are in series, being in fact simply parts of the same coil I of Fig. 26, the current in the one must be, of course, exactly the same as the current in the other; but as one-half of the coil is wrapped one way round the iron ring, and the other half the other way, the currents, as far as sending a north pole round the ring is concerned, will have diametrically opposite effects—that is, will differ by 180° in phase. Hence, while the currents in the three coils I, II, III, in Fig. 24, differed by 120° in phase, the currents in the six coils I, II, III, I_2, II_2, III_2 (Fig. 27) will differ by 60° in phase, so that, as far as the magnetization of the iron ring is concerned, we have arrived at exactly the arrangement of currents shown in Fig. 18. There is, however, this important difference—that, whereas in Fig. 18 six main wires were required, in Fig. 27 only three are needed.

The difference in phase between the currents in the six coils (Fig. 27) and the currents in the mains can be at once obtained from Fig. 25. For it is easy to show that the current

$$\text{in} \quad \text{I} = \frac{\text{A} + \text{B}}{3},$$

$$\text{,,} \quad \text{II} = \frac{\text{B} - \text{C}}{3},$$

$$\text{,,} \quad \text{III} = \frac{\text{C} + \text{A}}{3},$$

where A, B, and C represent simply the arithmetical values of the currents in the three main leads. Arithmetically, then, for the same currents in the mains A, B, C, the currents in the three coils I, II, III of Fig. 27 are the same as the currents in the three coils I, II, III of Fig. 24. But while, as far as sending north polarity counter-clockwise round the iron ring is concerned, the current in coil II of Fig. 24 was negative, that in coil II of Fig. 27 is positive. Hence, while it was the projection of O2 (Fig. 25) that gave the current in coil II of Fig. 24, it will be the projection of O2′ that will completely represent the current in coil II of Fig. 27, &c.

Hence, in Fig. 27 the current in the coil I will be O I, the projection of O1; the current in the coil II will be O II′, the projection of O2′; that in coil III will be O III, the projection of O3; that in coil I_2 will be O I_2′, the projection of

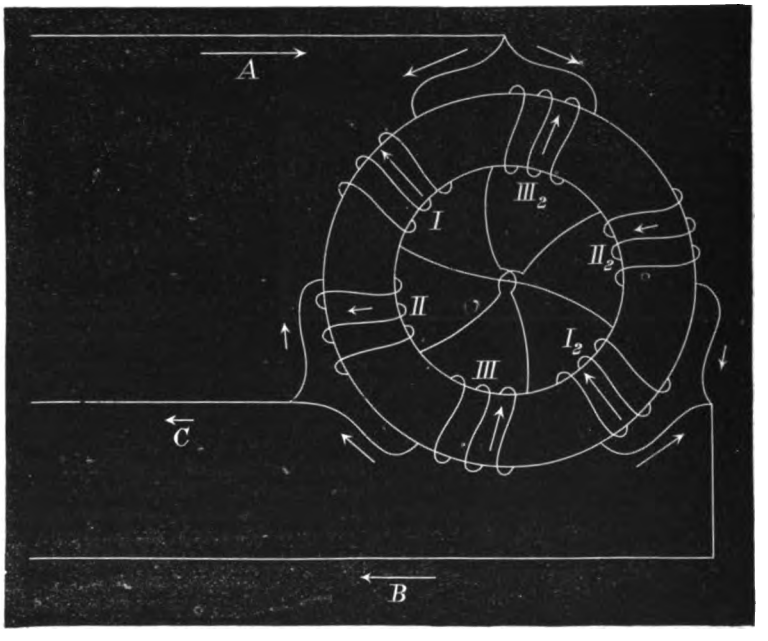

FIG. 27.—Six-phase alternate current motor (closed winding); currents differing by 60° in phase, and represented in direction and magnitude by the direction and length of the arrows.

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O1', &c. And the various arrows attached to the various parts of *Fig.* 27 are all drawn so as to represent, in direction and by their proportional lengths, the currents as determined from Fig. 25.

If, instead of starting with the closed circuit three-*phase motor* (*Fig.* 24), we deal in the same sort of way

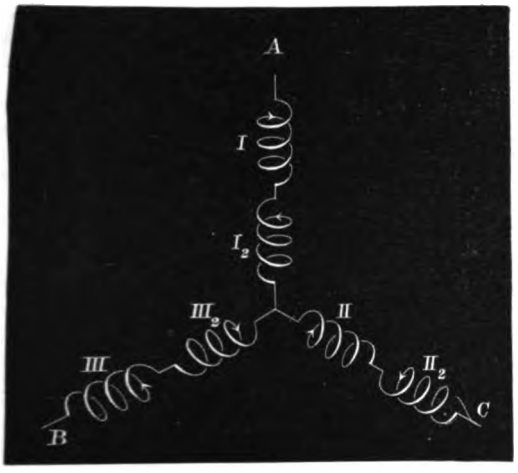

FIG. 28.—Transformation of a three-phase alternate current motor (open winding), with currents differing by 120° in phase, into a six-phase motor, with currents differing by 60° in phase.

with the open circuit three-phase motor (Fig. 20), we obtain Fig. 28, by supposing each coil to consist of two coils in series oppositely wound; and then, by interchanging the positions of the coils without breaking any of the connections, we arrive at the six-phase motor seen

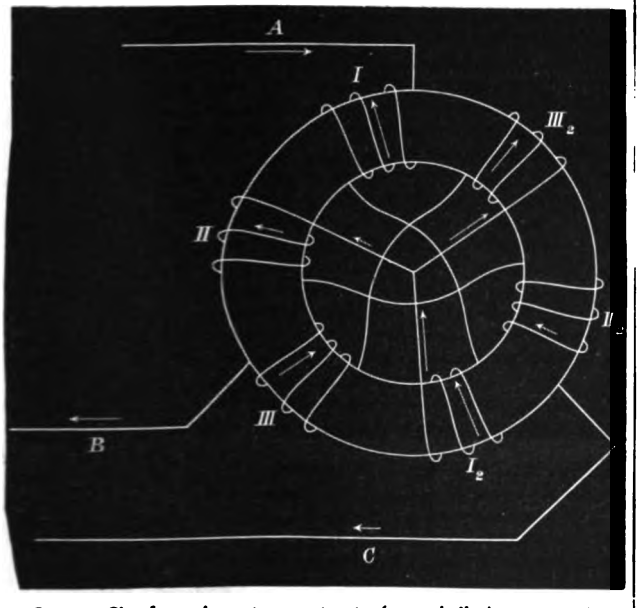

FIG. 29.—Six-phase alternate current motor (open winding); currents differing by 60° in phase, and represented in direction and magnitude by the direction and length of the arrows.

in Fig. 29, where the current in coil I is completely represented by Oα (Fig. 25), that in coil II by the projection of Oγ *produced backwards*, that in coil III by the projection of Oβ, &c.

Lastly, if we combine the closed and open methods of winding, and consider each coil as consisting of two in series but wound in opposite directions, we arrive at the symbolical Fig. 30; then, by interchanging the positions of the coils without separating any of the connections, the twelve-phase motor shown in Fig. 31 is produced,

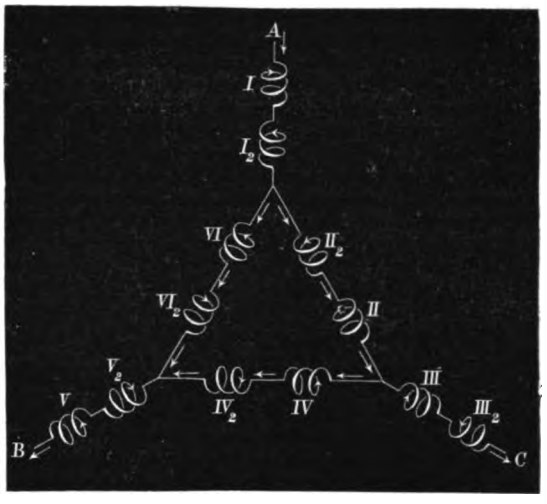

FIG. 30.—Transformation of an open wound combined with a closed wound six-phase alternate current motor into a twelve-phase motor with current differing by 30° in phase.

where the current in each coil differs from that in the preceding by 30° in phase.

In the twelve-phase motor (Fig. 31) the current in coil I is completely represented by the projection of Oα (Fig.

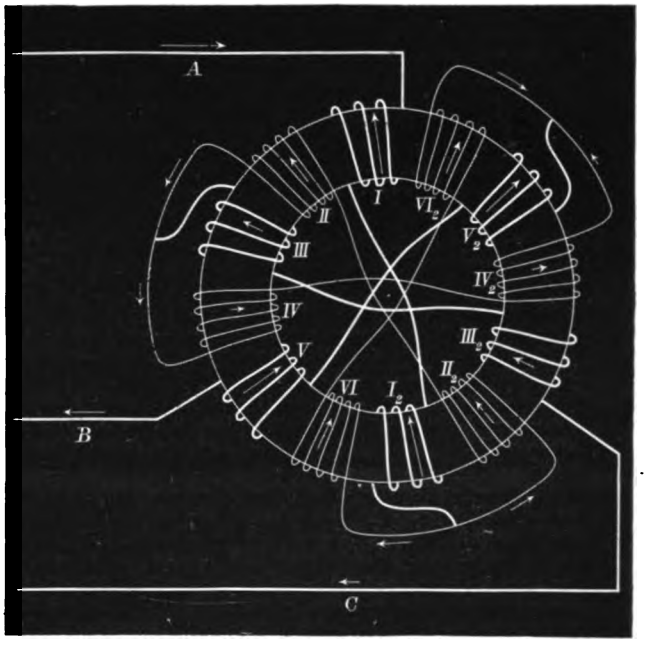

FIG. 31.—Twelve-phase alternate current motor; currents differing by 30° in phase and represented in direction and magnitude by the direction and length of the arrows.

25), that in coil II by the projection of O3', that in coil III by the projection of Oγ *produced backwards*, that in coil IV by the projection of O2, &c. The maximum currents then in the coils II, II_2, IV, IV_2, VI, VI_2, will

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be only $\frac{1}{\sqrt{3}}$, or 0·5744 times the maximum value of the currents in I, I_2, III, III_2, V, V_2. Hence the number of convolutions in each of the coils II, II_2, IV, IV_2, VI, VI_2 should be $\frac{2}{3}$rds of the number in each of the coils I, I_2, III, III_2, V, V_2, and the cross-section of the wire in each of the first six coils only $\frac{2}{3}$ths of that in the last six, both of which ratios are symbolically indicated in Fig. 31.

By still further following out the same general idea, an alternate current motor with twenty-four, or more, coils on it can be developed, requiring only three main wires to supply the current. And thus, thanks to the labours of Tesla, Bradley, Haselwander, Wenström, and last, but by no means least, to the striking ingenuity of Dolivo Doblowolsky, a practical alternate current motor can now be constructed, which will produce as steady a driving force as the best modern direct current motor.

W. E. A.

(*To be continued.*)

THE IMPLICATIONS OF SCIENCE.[1]

I.

WHEN I was honoured by an invitation to lecture here this evening, I felt much troubled as to the subject which I might most fitly select as my theme. During the forty years I have been a member of the Royal Institution, I have had the privilege of listening to lectures on many very different branches of science, and I know that all branches of science have few or many followers amongst the audience I am now addressing.

It has struck me, however, that for this single lecture it might be well not to confine myself to any subordinate department of scientific inquiry, but rather to invite your attention to certain questions which deeply concern them all. Thus, it has seemed to me, I might hope to interest a greater number of hearers than it would be possible for me otherwise to do.

I felt the more encouraged to take this course when I recalled to mind on how many previous occasions I had myself listened to discourses of a similar breadth of scope, given in this theatre by very distinguished men of science.

Foremost among them I may mention Prof. Huxley, who has here, as elsewhere, called attention to questions which underlie all physical science. I may also refer to that brilliant mathematician, Prof. Clifford, the sad and sudden ending of whose brief career we have good reason to deplore.

It would be easy to mention the names of other scientific celebrities who have here discoursed on matters beyond the scope of any one branch of science. These two, however, will, I think, suffice.

But before proceeding further I would feign say a few words as to the title of my lecture, so as at once to prevent any misunderstanding as to the object I have in view.

By "*the implications of science*," I mean nothing to which any section of my hearers can object, whatever their notions about "creed" or "conduct" may be. I desire carefully to eliminate all questions of either religion or morals, and I shall confine myself purely and simply to the consideration of certain propositions which appear to me to be latent within, and give force to, what we regard as well-ascertained scientific truths. They are propositions which must, I believe, be assented to by every consistent follower of science, who is convinced that science has brought to our knowledge *some* truths on which we can, with entire confidence, rely.

My appeal, then, is to the pure intellect of my hearers, and to nothing else. And indeed I desire to take this opportunity plainly to declare, before this distinguished audience, that not only here and now, but everywhere and always, I unhesitatingly affirm that no system can, or should, stand, which is unable to justify itself to reason. I possess no faculty myself, nor do I believe that any human faculty exists, superior to the intellect, or which has any claim to limit or dominate the intellect's activity. Feelings and sentiments have their undoubted charm and due place in human life, but that place is a subordinate one, and should be under the control of right reason.

But it is by no means only or mainly against those who would undervalue *reason* in the interest of *sentiment*, that I have this evening to protest. My object is to uphold what I believe to be the just claims of our rational nature against all who, from whatever side, or in the name of whatsoever authority, would impugn its sovereign claims upon our reverence, or unduly restrict the area of its sway.

As I have already intimated, I propose to fulfil this task by calling attention to some half-dozen far-reaching truths implicitly contained in scientific doctrines universally admitted, so that those doctrines cannot logically be maintained, if such implied truths are *really* and *seriously* doubted, and still less if they are *really* disbelieved and denied. These truths, then, are what I mean by "*the implications of science*." But what *is* science?

The word "science" is now very commonly taken as being synonymous with "*physical science*." There is much to be said against giving the word so narrow a meaning; nevertheless that meaning will sufficiently serve my purpose this evening. "Science," then, thus understood, is merely ordinary knowledge pursued with extreme care—most careful observation, measuring, weighing, &c.—together with most careful reasoning as to the results of observations and experiments, and also painstaking verification of any anticipations which may have been hazarded. In this way our thoughts are made to conform as accurately as may be with what we regard as the realities they represent.

The value and the progress of science are unquestioned. Many foolish discussions are carried on in the world about us; but certainly no one disputes or doubts the value of science or the fact of its progress. The value of carefully ascertained scientific truths will not at any rate be disputed in *this* theatre, which has witnessed the triumphs of the immortal Faraday, and which may justly claim to be a very temple of science. And certainly *I* have no disposition to undervalue it, who have loved it from my earliest years, and devoted such small powers as I possess to its service. I am profoundly convinced that, since I can recollect, biological science has made great progress, and I see grounds for absolute certainty now about many propositions in zoology which were doubtful or undreamed of when I was a lad.

We all, then, agree that science does advance. Nevertheless, it is obvious that such advance would be impossible if we could not, by observations, experiments, and inferences, become so certain with respect to *some* facts as to be able to make them the starting-points for fresh observations and inferences as to other facts. Thus, with respect to the world we live in, most educated men are now certain as to its daily and annual revolutions, as also that its crust is largely composed of sedimentary rocks, containing remains or indications of animals and plants more or less different from those which now live. No one can reasonably deny that we may rely with absolute confidence and entire certainty upon a variety of such assertions.

But our scientific certainties have been acquired more or less laboriously, and a questioning attitude of mind is emphatically the scientific attitude. We ought never to

[1] Friday Evening Discourse delivered at the Royal Institution by Dr. St. George Mivart, on June 5, 1891.

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rest satisfied about any scientific inquiry the truth of which has not been demonstrated, unless we find that it is one which we have no possible power to answer. It would obviously be idle to occupy ourselves about the shape or number of the mountains on that side of the moon which is constantly turned away from us.

Yet, although doubt and inquiry are necessary in science, nevertheless doubt has its legitimate limits. Blind disbelief is scientifically fatal, as well as blind belief. We all know how apt men are, when seeking to avoid one extreme, to fall into the opposite one, and it is possible to get into an unhealthy condition of mind so as to be unable to give a vigorous assent to anything. It is necessary distinctly to recognize there is such a thing as *legitimate* certainty, not to perceive the force of which is *illegitimate* doubt. Such doubt would necessarily discredit all physical science.

Universal doubt, for example, is an absurdity. It is scepticism run mad.

If anyone affirms that "*nothing is certain*," he obviously contradicts himself, since he thereby affirms the certainty of uncertainty. He says *that*, which, if true, absolutely contradicts what he has declared to be true.

But a man who affirms what the system he professes to adopt forbids him to affirm, and who declares that he believes what he also declares to be unbelievable, should hardly complain if he is called "foolish." No system can be true, and no reasoning can be valid, which inevitably ends in absurdity. *Such* scepticism, then, cannot be the mark of an exceptionally intellectual mind, but of an exceptionally foolish one, and every position which *necessarily* leads to scepticism of this sort must be an untenable position.

A very little reflection suffices to show how self-refuting such modes of thought are.

Thus, if a man were to say, "*I cannot know anything because I cannot be sure that my faculties are not always fallacious*," or "*I cannot be sure of anything because, for all I know, I may be the plaything of a demon who amuses himself by constantly deceiving me*"—in both these cases he contradicts himself, because he obviously grounds his assertion upon his perception of the truth that "*we cannot arrive at conclusions which are certain by means of premisses which are uncertain or false.*"

But if he knows *that* truth, he must know that his faculties are not always fallacious, and that his demon cannot deceive him in everything.

My object in making these remarks is to enable us to get clear of mere idle, irrational doubts which have no place in science and can have none, so that we may recognize the fact that we all of us have certainty as to *some* facts according to our degrees of knowledge. Obviously we can only judge of truth by our mental faculties, and if a man denies their validity we must pass him by, contenting ourselves with calling his attention to the fact that he refutes himself. If a man professes to doubt his faculties, or to doubt whether language can be trusted to convey thought, then plainly we cannot profitably argue with him. But if, on account of his absurdity, we cannot refute him, it is no less plain that he cannot defend his scepticism. Were he to attempt to do so, then he would show, by that very attempt, that he really had confidence in reason and in language, however he might verbally deny it.

Confident, then, that there are some scientific statements on which we may rely with *certainty*, let us consider a few truths implicitly contained in them.

In the first place, science makes use not only of observations and experiments, but also of reasoning as to the results of such experiments. It needs that we should draw valid inferences; but this implies that we may, and *must*, place confidence in the principle of deduction—in that perception of the mind which we express by the word "*therefore*." When we use that word, we mean to express by it that there is a truth, the certainty of which is shown through the help of different facts or principles which themselves are known to be true.

It is sometimes objected to deductive reasoning—to the syllogism—that it really teaches us nothing new, all that is contained in the conclusion being contained already in the premisses. But this objection is due to a want of perception of the great difference which exists between *implicit* and *explicit* knowledge. Let us suppose a person to be looking at some very flexible and soft kind of fish. He may, perhaps, say to himself, "This creature can have no spinal column!" Then it may strike him that naturalists have classed fishes, together with other animals, in a great group, one character of which is the possession of a spinal column, and so he may *explicitly* recognize a truth implied in what he knew before. So great, indeed, is the difference between explicit and implicit knowledge, that the latter may not really deserve to be called "*real knowledge*" at all. No one will affirm that a student who has merely learned the axioms and definitions of Euclid has attained such a *real knowledge* of all the geometrical truths the work contains that he will fully understand all its propositions and theorems without having to study them. Yet all the propositions, &c., of Euclid are *implicitly* contained in the definitions and axioms. Nevertheless, the student will have to go through many processes of inference by which these implicit truths may be explicitly recognized by him, before he can be said to have any real *knowledge* of them.

The VALIDITY OF INFERENCE is, then, one of the truths implied by physical science, and we shall presently see the intellectual penalty which must be paid for any *real* doubt about it.

In the second place, physical science is emphatically experimental science. But every experiment, carefully performed, implies a most important latent truth. For when an experiment has shown us that anything is certain—as, for example, that a newt's leg may grow again, after amputation, because one actually *has* grown again; we shall find that such certainty implies a prior truth. It implies the truth that if the newt has come to have four legs once more, it cannot at the very same time have only three legs. This may seem too trivial a remark to some of my hearers, but there is nothing like a *concrete* example for making an *abstract* truth plain. Anything we are certain about, because it has been proved to us by experiment, is certain only if we know, and because we know, that a thing which has been actually proved cannot at the same time remain unproven. If we reflect again on this proposition, we shall see that it depends on a still more fundamental truth which our reason recognizes—the truth, namely, that "nothing can at the same time both be and not be"—the truth known as "THE LAW OF CONTRADICTION"; and this I bring forward as a second truth implied by physical science.

If we reflect upon this law, we shall see that our intellect recognizes it as an absolute and necessary truth which carries with it its own evidence. It is but the summing up in one general expression, of all the concrete separate cases—such as that of the newt's legs, of the fact that if a man possesses two eyes he cannot at the same time have only one, and so on.

But an objection has been made as follows: "It is very true that I cannot imagine having 'two eyes' and only 'one eye' at the same time, and so I must practically acquiesce in the statement, but I am only compelled to do so by the impotence of my imagination." Thus, instead of the "law of contradiction," Mr. Herbert Spencer has put forward as an ultimate truth—"his universal postulate"—the assertion that "*we must accept as true propositions we cannot help thinking, because we cannot imagine the contrary.*" But if any of my hearers will reflect over what his mind tells him when it pronounces

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that he cannot at the same time have both two eyes and only one eye, he will, I think, see that his perception is (as *mine* is) a perception of real incompatibility, and consequent positive impossibility. He will not find his mind a mere blank, passively unable to imagine something. He will find that his mind actively asserts its power to judge of the matter as well as what its judgment is, and that the truth is one which positively applies to *things*, and not merely to his own imaginings.

Moreover, this objection ignores the difference between intellect and imagination. Yet there are very many things we can conceive of but cannot imagine, as, for example, our "act of sight" or "our own annihilation."

But it appears to me evident that Mr. Herbert Spencer's "universal postulate" can never be itself an ultimate truth, but must depend upon the law of contradiction. For, supposing we had tried to imagine a thing and *failed*, how could we from that ever be sure we might not at the same time have actually tried and *succeeded*, if we could not rely upon the law of contradiction?

The consequences resulting from any real doubt as to this law we will see later on.

In the pursuit of science, observation is anterior to experiment; but in every observation in which we place confidence, and still more in every experiment, a third fundamental truth is necessarily implied: this implied truth is THE VALIDITY OF OUR FACULTY OF MEMORY.

It is plain that it would be impossible for us to be certain about any careful observation or any experiment, if we could not feel confidence in our memory being able to vouch for the fact that we had observed certain phenomena and what they were. But what is memory?

Evidently we cannot be said to remember anything unless we are conscious that the thing we so remember has been present to our mind on some previous occasion. A mental image might present itself to our imagination a hundred times; but if at each recurrence it seemed to us something altogether new, and unconnected with the past, we could not be said to *remember* it. It would rather be an example of extreme forgetfulness than of memory.

By asserting the trustworthiness of our faculty of memory, I do not, of course, mean that we may not occasionally make mistakes about the past. It is quite certain we may, and do, make such mistakes. But, nevertheless, we are all of us certain as to *some* past events. Probably there is no single person now in this room who is not certain that he was somewhere else before he entered it. Memory informs us—certainly it informs *me*—as surely concerning *some* portions of the past, as consciousness does concerning some portions of the present.

If we could not trust our faculty of memory, the whole of physical science would be, for us, a mere present dream. But there can be no such thing as *proof* of the trustworthiness of memory, since no argument is possible without trusting to the veracity of memory. It is therefore a fundamental fact which must be taken on its own evidence, and from a consideration of the results of any real doubt about it—results I will refer to presently.

Yet it has been strangely declared, by a leading agnostic, that we may trust our memory because we learn its trustworthiness by experience. Surely never was fallacy more obvious! How could we ever gain experience if we did not trust memory in gaining it? Particular acts of memory may, of course, be confirmed by experience if the *faculty* of memory be already trusted, but in every such instance it must be confided in. The agnostic referred to has told us in effect that we may place confidence in our present memory because in past instances its truth has been experimentally confirmed, while we can only know it *has* been so confirmed by trusting our present memory!

But if we admit the trustworthiness of memory at all, a most important consequence follows—one relating to the distinction between what is *subjective* and what is *objective*.

Every feeling or state of consciousness present to the mind of the subject who possesses it is "subjective," and the whole of such experiences taken together constitute the sphere of *subjectivity*. Whatever is external to our present consciousness or feelings is for us "objective," and all that is thus external is the region of OBJECTIVITY. Now memory, inasmuch as it reveals to us part of our own past, reveals to us what is "objective," and so introduces us into the realm of objectivity, shows us more or less of objective truth, and carries us into a real world which is beyond the range of our own present feelings. This progress, then, this knowledge of *objectivity*, is, through memory, IMPLIED in every scientific experiment the facts of which we regard as certain.

But our scientific observations and experiments carry with them yet another implication more important still: this is the certainty of our KNOWLEDGE OF OUR OWN CONTINUOUS EXISTENCE. Unless we can be sure that we actually made the observations and experiments, on our having made which we rely for our conclusions, how can those conclusions be confidently relied on by us?

This implication is so important—in my opinion so *fundamentally* important—that I must crave your permission to notice it, later on, at some length. But before considering it, I desire to call your attention to the fact that the propositions thus implied by physical science, run directly counter to a system of thought which is widely current to-day, and which has now and again found expression in this theatre. The popular views I refer to may be conveniently summed up as follows:—

(1) All our knowledge is merely relative.

(2) We can know nothing but phenomena.

(3) We have no supremely certain knowledge but that of our own feelings, and therefore we have none such of our continuous existence.

(4) We cannot emerge from subjectivity, or attain to real knowledge of anything objective.

Therefore, either I am very much mistaken, or those who uphold the views I have just summed up are much mistaken.

It may seem presumptuous on my part to come forward here to-night to controvert a system upheld by men of such undoubted ability and so unquestionably competent in science, as are men who uphold the system I oppose. I feel therefore that a few words of personal apology and explanation are due from me.

For full five-and-thirty years I have been greatly interested in such questions. But when my intellectual life began, it was as a student and disciple of that school with which the names of John Stuart Mill, Alexander Bain, G. H. Lewes, Herbert Spencer, and Prof. Huxley have been successively associated—more or less closely. The works of writers of that school I studied to the best of my ability, and I had the advantage of personal acquaintance with some of the more distinguished of them. Thus, by conversation, I was much better enabled to learn what their system was than I could have learned it by reading only.

However, by degrees, I became sceptical about the validity of the system I had at first ingenuously adopted, but it took me not a few years to clearly see my way through all the philosophical fallacies—as I now regard them—in which I found myself entangled. I say "see my way *through*," for I did not free myself from them by *drawing back* but by *pushing forwards*—slowly working my way through them and out on the other side. These circumstances constitute my apology for appearing before you as I do. I have been a dweller in the country which I am willing to aid anyone to explore who may wish to explore it.

(*To be continued.*)

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ELECTRICITY IN RELATION TO SCIENCE.

THE third annual dinner of the Institution of Electrical Engineers was held at the Criterion on Friday, November 13. Prof. William Crookes, the President, was in the chair. In proposing the toast of the evening, "Electricity in relation to Science," Prof. Crookes delivered the following speech:—

We have happily outgrown the preposterous notion that research in any department of science is mere waste of time. It is now generally admitted that pure science, irrespective of practical applications, benefits both the investigator himself and greatly enriches the community. "It blesseth him that gives, and him that takes." Between the frog's leg quivering on Galvani's work-table and the successful telegraph or telephone there exists a direct filiation. Without the one we could not have the other.

We know little as yet concerning the mighty agency of electricity. "Substantialists" tell us it is a kind of matter. Others view it, not as matter, but as a form of energy. Others, again, reject both these views. Prof. Lodge considers it "a form, or rather a mode of manifestation, of the ether." Prof. Nikola Tesla demurs to the view of Prof. Lodge, but thinks that "nothing stands in the way of our calling electricity ether associated with matter, or bound ether." High authorities cannot even yet agree whether we have one electricity or two opposite electricities. The only way to tackle the difficulty is to persevere in experiment and observation. If we never learn what electricity is, if, like life or like matter, it should remain an unknown quantity, we shall assuredly discover more about its attributes and its functions.

The light which the study of electricity throws upon a variety of chemical phenomena—witnessed alike in our little laboratories and in the vast laboratories of the earth and the sun—cannot be overlooked. The old electro-chemical theory of Berzelius is superseded, and a new and wider theory is opening out. The facts of electrolysis are by no means either completely detected or co-ordinated. They point to the great probability that electricity is atomic, that an electrical atom is as definite a quantity as a chemical atom. The electrical attraction between two chemical atoms being a trillion times greater than gravitational attraction is probably the force with which chemistry is most deeply concerned.

It has been computed that, in a single cubic foot of the ether which fills all space, there are locked up 10,000 foot-tons of energy which have hitherto escaped notice. To unlock this boundless store and subdue it to the service of man is a task which awaits the electrician of the future. The latest researches give well-founded hopes that this vast storehouse of power is not hopelessly inaccessible. Up to the present time we have been acquainted with only a very narrow range of ethereal vibrations, from extreme red on the one side to ultra-violet on the other—say from 3 ten-millionths of a millimetre to 8 ten-millionths of a millimetre. Within this comparatively limited range of ethereal vibrations, and the equally narrow range of sound vibrations, we have been hitherto limited to receive and communicate all the knowledge which we share with other rational beings. Whether vibrations of the ether, slower than those which affect us as light, may not be constantly at work around us, we have until lately never seriously inquired. But the researches of Lodge in England, and Hertz in Germany, give us an almost infinite range of ethereal vibrations or electrical rays, from wave-lengths of thousands of miles down to a few feet. Here is unfolded to us a new and astonishing universe—one which it is hard to conceive should be powerless to transmit and impart intelligence.

Experimentalists are reducing the wave-lengths of the electrical rays. With every diminution in size of the apparatus the wave-lengths get shorter, and could we construct Leyden jars of molecular dimensions the rays might fall within the narrow limits of visibility. We do not yet know how the molecule could be got to act as a Leyden jar; yet it is not improbable that the discontinuous phosphorescent light emitted from certain of the rare earths, when excited by a high-tension current in a high vacuum, is really an artificial production of these electrical rays, sufficiently short to affect our organs of sight. If such a light could be produced more easily and more regularly, it would be far more economical than light from a flame or from the arc, as very little of the energy in play is expended in the form of heat rays. Of such production of light, Nature supplies us with examples in the glow-worm and the fire-flies. Their light, though sufficiently energetic to be seen at a considerable distance, is accompanied by no liberation of heat capable of detection by our most delicate instruments.

By means of currents alternating with very high frequency, Prof. Nikola Tesla has succeeded in passing by induction through the glass of a lamp energy sufficient to keep a filament in a state of incandescence without the use of connecting wires. He has even lighted a room by producing in it such a condition that an illuminating appliance may be placed anywhere and lighted without being electrically connected with anything. He has produced the required condition by creating in the room a powerful electrostatic field alternating very rapidly. He suspends two sheets of metal, each connected with one of the terminals of the coil. If an exhausted tube is carried anywhere between these sheets, or placed anywhere, it remains always luminous.

The extent to which this method of illumination may be practically available experiments alone can decide. In any case, our insight into the possibilities of static electricity has been extended, and the ordinary electric machine will cease to be regarded as a mere toy.

Alternating currents have at the best a rather doubtful reputation. But it follows from Tesla's researches that as the rapidity of the alternation increases they become not more dangerous but less so. It further appears that a true flame can now be produced without chemical aid—a flame which yields light and heat without the consumption of material and without any chemical process. To this end we require improved methods for producing excessively frequent alternations and enormous potentials. Shall we be able to obtain these by tapping the ether? If so, we may view the prospective exhaustion of our coal-fields with indifference; we shall at once solve the smoke question, and thus dissolve all possible coal-rings.

Electricity seems destined to annex the whole field not merely of optics, but probably also of thermotics.

Rays of light will not pass through a wall, nor, as we know only too well, through a dense fog. But electrical rays of a foot or two wave-length of which we have spoken will easily pierce such mediums, which for them will be transparent.

Another tempting field for research, scarcely yet attacked by pioneers, awaits exploration I allude to the mutual action of electricity and life. No sound man of science endorses the assertion that "electricity is life"; nor can we even venture to speak of life as one of the varieties or manifestations of energy. Nevertheless electricity has an important influence upon vital phenomena, and is in turn set in action by the living being—animal or vegetable. We have electric fishes—one of them the prototype of the torpedo of modern warfare. There is the electric slug which used to be met with in gardens and roads about Hornsey Rise; there is also an electric centipede. In the study of such facts and such relations the scientific electrician has before him an almost infinite field of inquiry.

The slower vibrations to which I have referred reveal

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the bewildering possibility of telegraphy without wires, posts, cables, or any of our present costly appliances. It is vain to attempt to picture the marvels of the future. Progress, as Dean Swift observed, may be too fast for endurance. Sufficient for this generation are the wonders thereof.

GEOLOGICAL PHOTOGRAPHS.

AT the meeting of the British Association in 1889, a Committee was appointed for the purpose of arranging for the collection, preservation, and systematic registration of photographs of geological interest in the United Kingdom. Since its formation, the Committee has succeeded in obtaining a number of photographs, 588 of which were received and registered up to August last; and in its second report, presented at the Cardiff meeting of the British Association, the Committee was able to state that, in the choice of subjects, greater care had been taken during the year to include the most typical views. As yet, only about half of the British counties are represented in the collection, while some are still represented inadequately. The work is one of great interest and importance, both from a scientific and an educational point of view, and it may be hoped that local Societies and Field Clubs, whose co-operation the Committee is particularly anxious to secure, will everywhere associate themselves with the scheme, and do what they can to bring it to completion. The Committee, in a circular just issued, suggests that these Societies and Clubs might materially aid the scheme by mapping out their districts under the direction of a local geologist, and drawing up a list of sections and localities of which photographs would be desirable. New sections and exposures of strata should be noted. This preliminary work could be done during the winter session, and arrangements made for the use of the camera in the ensuing spring, or when opportunity offered.

In its report, the Committee refers especially to the work accomplished by the Geological Photographic Committee of the Yorkshire Naturalists' Union. Among the photographs of this Society are many relating to sections which cannot be reproduced—as, for instance, fossil trees laid bare in quarrying and in excavations for the foundations of buildings now covered over. The Hertfordshire Natural History Society and the East Kent Natural History Society have also organized schemes for the photography of local geological features; and the Committee has already received from them views which, it is hoped, will be supplemented by a further series next year.

Of course it is not always easy to obtain the services of a professional photographer, but few Societies should have much difficulty in securing the help of amateur photographers, so many of whom are now to be found in all parts of the country. In order that there may be unity of action, the Committee has drawn up a set of instructions, copies of which may be obtained on application to the secretary, Mr. Osmund W. Jeffs, 12 Queen's Road, Rock Ferry, Cheshire. It is pointed out that the photographs should illustrate characteristic rock exposures, especially those of a typical character or temporary nature; important boulders; localities affected by denudation, or where marked physiographical changes are in operation; raised beaches; old sea-cliffs and other conspicuous instances of marine erosion; characteristic river-valleys or escarpments, and the like; glacial phenomena, such as *roches moutonnées*, moraines, drums, and kames; or any natural views of geological interest. Photographs of microscopical sections and typical hand-specimens of rocks are also admissible.

Detailed lists of photographs officially received are published in the report of the Committee, which also states where the photographs may be obtained. Lists for insertion in the third report will be received up to June 15, 1892.

It is satisfactory to find that geological photographic schemes similar to that of the British Association are being adopted in other countries. The Committee, in its second report, alludes to the action taken in the matter by the Société Géologique de Belgique, and to the Committee of Photographs appointed by the Geographical Society of America. The American Committee proposes to prepare lists for international exchange.

NOTES.

AFTER a rather prolonged delay, the Commission for the delimitation of the Anglo-French frontier in the neighbourhood of Sierra Leone, in accordance with the West African agreement between Great Britain and France of August 10, 1889, has been appointed. Captain Kenney, R.E., the British Commissioner, with his party, proceeded to Sierra Leone by the steamer of November 14 last. The Secretary of State for the Colonies permitted the Director of Kew to nominate a botanist to accompany the expedition, and the Government Grant Committee of the Royal Society made a grant to meet his expenses, part of which will also be borne by the Government of Sierra Leone. The mission has been undertaken by Mr. G. F. Scott-Elliot, M.A. Camb., B.Sc. Edinb., F.L.S., who has recently published in the Journal of the Linnean Society an account of the new species of plants found by him in a journey through a little known part of Southern Madagascar. The botany of the interior of Sierra Leone is very little known, but is believed to be of great interest. The Commission will be absent about six months. It will proceed in the first instance to Falaba, and then proceed to the point of intersection of the 10th parallel of North latitude and the 13th meridian (French) of West longitude.

WE understand that Prof. Hennessy, F.R.S., will shortly resign the Chair of Applied Mathematics and Mechanism in the Royal College of Science, Dublin. The salary of the post is £400, rising to £500, a year, with a share of the fees. The appointment rests with the Lord President of the Council, and applicants should address themselves to the Secretary, Science and Art Department.

THE arrangements to be made for the Crystal Palace Electrical Exhibition are to be discussed at a meeting of the honorary council of advice and of the special committee appointed by the Electrical Section of the London Chamber of Commerce. The meeting will be held at the Mansion House on Wednesday, November 25, at 3 o'clock. The Lord Mayor will preside. The whole of the space is now practically allotted, extra buildings having been erected for certain large installations which could not otherwise have been accommodated.

A COMMITTEE has been appointed by the American Institute of Electrical Engineers to suggest plans for the International Electrical Congress to be held in Chicago in 1893, in connection with the World's Fair. A local Committee, with Prof. Gray as President, is being formed at Chicago for the purpose of making preparations for the same Congress. The Chicago journal *Electricity* sees no reason "why a perfectly harmonious arrangement should not be made between the Institute and the local Committee, whereby both will work together to promote the success of the Congress."

THE Spanish Government intend to open two Exhibitions in September 1892 in celebration of the fourth centenary of the discovery of America. One of these will be at Madrid, and will be called the Exposition Historique Américaine de Madrid. The

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other, at Huelva, will be called the Exposition Historique Européenne de Madrid. The former should have considerable interest for anthropologists, as it has for its object, according to the official programme, "de présenter de la manière la plus complète l'état où se trouvaient les différentes contrées du Nouveau-Continent avant l'arrivée des Européens et au moment de la conquête, jusqu'à la première moitié du XVII^e siècle." It will comprise objects, models, pictures, &c., illustrating the customs and civilization of the peoples at that time inhabiting America.

WE greatly regret to have to record the death of Prof. Henry N. Moseley, F.R.S. He died on November 10, at the age of forty-six. We hope next week to give some account of his services to science.

DR. OSCAR BAUMANN is about to undertake a series of explorations in the interest of the German East Africa Company. In the German Masai territory there are, he says, in a letter to *Globus*, many regions about which little is known; and about these he hopes to bring back much fresh light. He proposes to study the conditions which must be taken into account by projectors of railways, and, if possible, to open a direct caravan route to Lake Victoria.

IN the course of his interesting presidential address at the meeting of the Institution of Civil Engineers last week, Mr. Berkley referred to the production of iron in the United States. The most conspicuous difference between American and English practice, he said, was the output from one blast furnace. The largest production in Great Britain did not seem to exceed 750 tons in the week, while in America it had reached 2000 tons. It might be questioned whether this large output from a single furnace was not obtained at some sacrifice of economy of fuel used and of wear and tear of furnace. The production of pig-iron in the United States now amounted to 10,000,000 tons, or 2,000,000 tons more than that of the United Kingdom. This amount was wholly used within the country, showing a larger quantity of iron used per head of the population (300 pounds) than in any other part of the world. In Great Britain, after deducting from its production of iron the quantity exported, the consumption only equalled 250 pounds per head of the population.

THE lecture season at the London Institution, Finsbury Circus, was opened on Monday evening with a lecture by Sir M. E. Grant Duff on "Some of our Debts to the East."

THE barometric depression on November 11, according to Mr. G. J. Symons, has been exceeded only five times in the 34 years during which he has been making meteorological observations. In a letter to the *Times* Mr. Symons says that as he anticipated some such depression, he started the Richard brontometer at 6.30 a.m., and kept it running for 10 hours—*i.e.* through the chief part of the depression. This, at the cost of a little trouble and a roll of paper, gave him a record of the motion of the barometer such as had never before been obtained—somewhere about 60 feet long—and with every little pulsation shown in detail, even if it lasted only two seconds.

WE have received the meteorological year-book published by the Madgeburg *Zeitung* for 1890, being the tenth year for which the observations have been made on a uniform plan. In addition to observations taken three times daily, the volume contains hourly values of pressure and wind, and continuous records of sunshine; also, curves of pressure and temperature for those periods of exceptional weather during which the ordinary hourly values would fail to represent the details of the oscillations—a plan which seems highly commendable. The whole work is very complete and compact.

THE Deutsche Seewarte has published a catalogue of its valuable library, containing entries of 10,660 works and excerpt papers in various subjects, about a quarter of which refer to meteorology. The library has been enriched by the acquisition of the books which formerly belonged to Prof. Dove and other eminent men. This laborious undertaking has been carried out with great care, and is classified under subjects and authors. Some difficulty has been experienced in dealing with a few English authors; *e.g.*, on p. 67, Balfour Stewart is entered under the Christian name, and on p. 160, Powell is entered as the Christian name of Baden-Powell. And the works issued by institutions are not always sufficiently distinguished from those which are due to individual writers; *e.g.*, on p. 5, Sir Thomas Farrer is credited with a work on "Telegraphic Weather Information"—of which, probably, he would have no recollection, it being merely a circular signed in his capacity as Secretary of the Board of Trade. But these are mere trifles, and in no way detract from the value of the work as a whole, for which the scientific world will be grateful.

MR. ALBERT KOEBELE, the American entomologist, is travelling in the Australasian colonies for the purpose of studying the enemies of insect pests. In introducing him to the Wellington Philosophical Society at a meeting on September 23, Sir James Hector recalled the circumstances connected with a memorable service which Mr. Koebele lately rendered to California. In 1888, when on a visit to South Australia in search of a small fly (Testophonus), a parasite on that dreadful pest *Icerya purchasi*, Mr. Koebele discovered a single ladybird (Vedalia) preying on the pest. He found a second specimen in New South Wales, and then on his arrival in New Zealand he found that the Icerya about Auckland was also being destroyed by something, and this, too, turned out to be Vedalia. He at once saw that here was the thing he sought, and he was fortunate enough to be able to collect several thousands of Vedalias, which he afterwards liberated in California. Up to that time California had been so eaten up by Icerya that the damage was estimated at twenty millions of dollars annually. Yet, in twelve or fifteen months after the liberation of Vedalia in April 1889, the State was practically free from the dreaded pest. Sir James Hector rightly characterized this work of Mr. Koebele as one of the grandest things in the interest of fruit and tree-growers that have been effected in modern times.

PROF. G. L. GOODALE, of Harvard University, has recently paid a visit to the Museums and Botanical Gardens in the tropics and in the southern hemisphere, and has contributed an interesting description of them to the *American Journal of Science*. In the number for October we find an account of the Technological Museum at Sydney, which contains a very complete collection of the economic vegetable products of Australia, and which is largely visited by the working classes; of the two Botanic Gardens at Brisbane, one of them under the management of the Society of Acclimatization; the Botanic Gardens at Geelong, Dunedin, Christchurch, and Wellington; the Museums at Dunedin, Christchurch, Wellington, and Auckland; and the small but excellent local Museum and Garden at Hobart. Prof. Goodale notices, with commendation, the tenacity with which all the Australian Museums cling to rare specimens of archæological and ethnographical interest, instead of utilizing them for exchange.

MESSRS. MACMILLAN AND CO. have published the first number of the *Record of Technical and Secondary Education*, a bi-monthly journal of the progress made by County Councils and other local authorities in the administration of the Technical Instruction Acts. The periodical is issued on behalf of the National Association for the Promotion of Technical and Secondary Education. Lord Hartington contributes an intro-

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ductory statement, in which he sets forth briefly the objects of the *Record*. It will, he says, be of a strictly practical character, and will not interfere with any educational journal now in circulation. It will "give the latest information, not only with respect to what is being done in this Kingdom, but also in regard to such educational work on the Continent and in America as may be of service to those who are engaged in carrying out schemes of technical instruction." The *Record* may become a journal of great value, and we trust there will be a cordial response to Lord Hartington's appeal to the members of the County Councils, their organizing secretaries, and others interested in the work, to supply the Committee with early and regular information as to what is being done in their several centres. The present number contains, besides Lord Hartington's statement, County Council schemes and reports relating to Oxfordshire, Surrey, Bedfordshire, Lancashire, Birmingham, and Aberdeen; details regarding Scholarship schemes in the West Riding and Somerset; notes on the work of the counties and county boroughs; miscellanea; and reviews.

THERE are not many remains of the ancient Mexican feather-work which excited the surprise of the Spanish conquerors of the New World. The most famous surviving specimen is the standard, described by Hochstetter, which is now in the Vienna Ethnographical Museum. Another specimen has lately been discovered by Mrs. Zelia Nuttall in the Schloss Ambras, near Innsbruck. It is mentioned in an inventory, drawn up in 1596, of the treasures of the castle. This very valuable relic is the decorative part of a round shield made of interlaced reeds, and consists of feather-mosaics representing a monster, the contours of which are fastened by strips of gold. Formerly the shield was adorned with costly quetzal feathers, only small fragments of which survive. *Globus*, which has an interesting note on the subject, speaks of similar old Mexican shields in the Stuttgar Museum, and refers to a statement of Stoll to the effect that beautiful feather-ornaments are still made by the Indians of Guatemala.

IN the Report of the U.S. National Museum for 1884, Prof. O. T. Mason published a short paper on the throwing-sticks of the Eskimo. The use of a like device for the throwing of spears and harpoons was formerly well known in Mexico; and Prof. Mason has written to *Science* to say that he lately received from Lake Patzcuaro, in Mexico, "a modern altatl, well worn and old-looking, accompanied with a gig for killing ducks." The apparatus, which was bought from the hunter by Captain J. G Bourke, U.S.A., has been placed in the National Museum. The thrower is 2 feet 3 inches long, and has two finger-holes, projecting, one from the right, one from the left side. The gig consists of three iron barbs, exactly like the Eskimo trident for water-fowl. "The problem now is," says Prof. Mason, "to connect Alaska with Patzcuaro."

A PAMPHLET on "The Dwarfs of Mount Atlas," by Mr. R. G. Haliburton, has been published by Mr. David Nutt. Along with it are printed portions of the paper on the subject read by Mr. Haliburton before the recent Oriental Congress. His views are accepted by Sir J. Drummond-Hay, who represented Great Britain in Morocco for over forty years, and by Mr. Hunnot, our Consul at Saffi. There is, of course, no inherent improbability in the statement that there are tribes of dwarfs to the south of Mount Atlas. Such tribes are known to exist elsewhere in Africa, and they may exist in the regions where Mr. Haliburton thinks he has discovered them. The question is one of evidence. Even if dwarfs have many settlements there, it does not follow that there is any solid foundation for Mr. Haliburton's theories as to the part their race has played in the evolution of mythology. Still the suggestion is an interesting one.

IN the report on "Oysters and Oyster-Fisheries of Queensland," to which we referred last week, Mr. Saville-Kent presents quite an idyllic picture of the circumstances of those who devote themselves in Queensland to the culture of "bank oysters"—that is, oysters which spread over extensive level banks that are more or less uncovered at low water. He says that "probably in no other country in the world is so healthy, congenial, and unlaborious a means of earning a substantial competency open to, and turned to practical account by, all classes as that of bank oyster culture in the Queensland oyster-producing districts of Moreton or Wide Bays. With a nominal rental payable for the ground cultivated and occupied for a homestead, a climate that permits of dispensing with all but the most necessary form of raiment, and fish procurable in such abundance as to substantially minimize the butcher's bill, no more perfect terrestrial Elysium is probably at the disposal of small capitalists having sufficient means for the supply of their most immediate necessities during that first year or two that must elapse before their oyster-crops have increased to a remunerative extent."

MR. CHARLES CHILTON contributes to the new number of the *Records of the Australian Museum* (vol. i. No. 8) an excellent paper on a new and peculiar fresh-water "Isopod" from Mount Kosciusko. Towards the end of 1889, Mr. Chilton received from the trustees of the Australian Museum, Sydney, a small collection of Australian Crustacea, containing, among others, some terrestrial and fresh-water species collected by Mr. R. Helms while on an expedition to Mount Kosciusko on behalf of the Museum. Among these Mr. Chilton at once saw that one was quite different from any of the terrestrial and fresh-water Crustacea previously described from Australia, and that it belonged to a genus *Phreatoicus* established by himself in 1882, for a peculiar blind subterranean Isopod found in wells in Canterbury, New Zealand. This genus was of special interest, both because of the situation in which the original species was found, and because it combined characters belonging to several different families, and was also, to some extent, intermediate between the Isopoda and the Amphipoda. The discovery of a species belonging to the same genus in such a widely remote situation as Mount Kosciusko, and living under such different conditions, was therefore of peculiar interest; and Mr. Chilton thinks that it will probably have an important bearing on the difficult question of the origin of the blind subterranean forms. In the paper in the *Records* he does not enter upon this question, but he hopes to do so on a future occasion, when describing more fully the subterranean forms from New Zealand.

MESSRS. MACMILLAN AND CO. have issued the fifth edition, revised, of Part IV. of Prof. M. Foster's "Text-book of Physiology." This completes the work, with the exception of the appendix, which differs so widely in character from the rest of the book that it seemed desirable to issue it separately. It will be published very shortly.

SOME time ago the Department of Agriculture in New South Wales included in its list of economic plants suitable for cultivation in the north-eastern portion of the colony the "Avocado" or "Alligator pear" (*Persea gratissima*, Gærtn.). Several inquiries about it having since been made, Mr. F. Turner provides an account of the plant, with an illustration, in the August number of the *Agricultural Gazette* of New South Wales. Unless it is grown in very sheltered situations, the climate of Sydney is too cold for its successful cultivation as a commercial crop; but Mr. Turner thinks that on the northern rivers of New South Wales it should bear fruit as prolifically as it does in Southern Queensland. Some years ago, in the Brisbane Botanic Gardens, a fine Alligator pear-tree bore annual crops of very fine fruit, and it may do so still. When Sir W. W. Cairns was Governor of Queensland, he often asked Mr. Turner for some of the fruit when it was in

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season, and Mr. Turner kept him well supplied, for at that time no one seemed to care much for it. His Excellency told Mr. Turner he was very fond of the fruit for breakfast, and he used to eat it spread on bread and butter, with pepper and salt added to give it zest, and in various other ways. Mr. Turner did not care for the fruit at first, but afterwards became as fond of it as his tutor. So we are not unlikely to hear of the Alligator pear by-and-by as a popular Australian product.

MESSRS. J. B. BAILLIÈRE ET FILS, Paris, have lately added some good volumes to their well-known Bibliothèque des Connaissances Utiles. In one of them—"La Pêche et les Poissons des Eaux Douces"—M. Arnould Locard presents a clear and interesting summary of the various classes of facts which must be understood by all who desire to become expert in the art of fishing in fresh waters. M. Lacroix-Danliard contributes a volume on "La Plume des Oiseaux," dealing with the birds whose feathers are utilized by man, and with the industrial processes to which the demand for feathers has given rise. A useful volume on "Les Plantes d'Appartement et les Plantes de Fenêtres" is contributed by M. D. Bois.

UNDER the title "Bibliotheca Accipitraria," Mr. J. E. Harting has on the eve of publication a bibliography of falconry, with critical notes. It deals with 378 works in various languages, ancient and modern, and will be illustrated with portraits of famous falconers by Holbein, Titian, Vandyck, Frans Floris, Gerhardt, and other masters. The volume concludes with an English glossary, and a vocabulary, in seven languages, of the technical terms used by falconers.

AN important treatise on Salt-Range fossils has been issued in the series entitled "Palæontologia Indica," which contains figures and descriptions of the organic remains procured during the progress of the Geological Survey of India. Prof. Waagen, the author of the treatise, in concluding it says he has tried to make it as useful as possible both to Indian geology and to geological science in general.

MR. STUART A. RUSSELL'S new work on electric light cables and the distribution of electricity will be issued shortly in Messrs. Whittaker's "Specialists' Series."

MESSRS. DULAU AND CO. have issued a catalogue of zoological and palæontological books which they offer for sale. It includes the following "parts"—Natural history publications of the British Museum; Protozoa, Bacteria; Cœlenterata.

THE last volume of the Memoirs of the Statistical Section of the Russian Geographical Society contains an interesting work by M. Borkovsky, who has devoted more than twenty-five years of his life to the study of the grain-production of Russia, and the directions in which cereals are transported within Russia both for export and for home consumption. The results totally upset the current theory as to Russia being a granary of Europe, and are grimly confirmed by the famine which now prevails in several provinces of the empire. It appears from M. Borkovsky's figures and maps that Russia may be divided into two parts, strictly dependent on her orographical structure: one of them, which corresponds to the south-eastern slope of the broad swelling which stretches across the country from south-west to north-east, has an excess of grain during the years of good crops, which excess sometimes exceeds twice or thrice the wants for local use. But there is also another part—the north-western one—which always has less corn than is wanted for its population. Taking the years 1882–85, which were years of average crops, a line traced from Kieff to Nijni-Novgorod and further north-east divides Russia into two almost equal parts, of which the south-eastern exports wheat and rye into the north-western part to the amount of no less than 710,000 tons of wheat and 508,000 tons of rye, the exports to foreign countries attaining at the same time the respective figures of 1,780,000 and 1,029,600 tons. Taking into account the respective populations of the two regions, and the amount of corn consumed by the distilleries (which does not exceed 14 English pounds per inhabitant), M. Borkovsky shows that the total consumption of wheat and rye attains only the figure of 437 pounds per inhabitant (109 pounds of wheat) in the exporting region, and the still lower figure of 382 pounds (46 pounds of wheat) in the region which imports corn. The average consumption throughout Russia thus attains only 430 pounds per inhabitant, out of which 14 pounds must be deducted for the use of the distilleries. These figures will certainly seem very low if it is remembered that the great mass of the Russian peasants consume extremely small quantities of meat—bread being their chief and almost exclusive food. It appears, moreover, that if Russia exported no grain at all, and the whole of the crop of cereals were consumed within the country, the average consumption would nearly approach the average consumption in France—that is, 505 English pounds on an average year; while the surplus obtained during years of exceptionally good crops would only cover the deficit during the bad years, which recur in the steppes of South-East Russia with almost the same regularity as in India, *i.e.* every ten to twelve years.

AN important paper is contributed by M. Moissan to the current number of the *Comptes rendus* describing two interesting new compounds containing boron, phosphorus, and iodine. A few months ago M. Moissan succeeded in preparing the iodide of boron (comp. NATURE, vol. xliii. p. 565), a beautiful substance of the composition BI_3, crystallizing from solution in carbon bisulphide in pearly tables, which melt at 43° to a liquid which boils undecomposed at 210°. When this substance is brought in contact with fused phosphorus an intense action occurs, the whole mass inflames with evolution of violet vapour of iodine. Red phosphorus also reacts with incandescence when heated in the vapour of boron iodide. The reaction may, however, be moderated by employing solutions of phosphorus and boron iodide in dry carbon bisulphide. The two solutions are mixed in a tube closed at one end, a little phosphorus being in excess, and the tube is then sealed. No external application of heat is necessary. At first the liquid is quite clear, but in a few minutes a brown solid substance commences to separate, and in three hours the reaction is complete. The substance is freed from carbon bisulphide in a current of carbon dioxide, the last traces being removed by means of the Sprengel pump. The compound thus obtained is a deep-red amorphous powder, readily capable of volatilization. It melts between 190° and 200°. When heated *in vacuo* it commences to volatilize about 170°, and the vapour condenses in the cooler portion of the tube in beautiful red crystals. Analyses of these crystals agree perfectly with the formula BPI_2. Boron phospho-di-iodide is a very hygroscopic substance, moisture rapidly decomposing it. In contact with a large excess of water, yellow phosphorus is deposited, and hydriodic, boric, and phosphorous acids formed in the solution. A small quantity of phosphoretted hydrogen also escapes. If a small quantity of water is used a larger deposit of yellow phosphorus is formed, together with a considerable quantity of phosphonium iodide. Strong nitric acid oxidizes boron phospho-di-iodide with incandescence. Dilute nitric acid oxidizes it to phosphoric and boric acids. It burns spontaneously in chlorine, forming boron chloride, chloride of iodine, and pentachloride of phosphorus. When slightly warmed in oxygen it inflames, the combustion being rendered very beautiful by the fumes of boric and phosphoric anhydrides and the violet vapours of iodine. Heated in contact with sulphuretted hydrogen, it forms sulphides of boron and phosphorus and hydriodic acid, without liberation

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of iodine. Metallic magnesium when slightly warmed reacts with it with incandescence. When thrown into vapour of mercury, boron phospho-di-iodide instantly takes fire.

THE second phospho-iodide of boron obtained by M. Moissan is represented by the formula BPI. It is formed when sodium or magnesium in a fine state of division is allowed to act upon a solution of the di-iodide just described in carbon bisulphide; or when boron phospho-di-iodide is heated to 160° in a current of hydrogen. It is obtained in the form of a bright-red powder, somewhat hygroscopic. It volatilizes *in vacuo* without fusion at a temperature about 210°, and the vapour condenses in the cooler portion of the tube in beautiful orange-coloured crystals. When heated to low redness it decomposes into free iodine and phosphide of boron, BP. Nitric acid reacts energetically with it, but without incandescence, and a certain amount of iodine is liberated. Sulphuric acid decomposes it upon warming, with formation of sulphurous and boric acids and free iodine. By the continued action of dry hydrogen upon the heated compound the iodine and a portion of the phosphorus are removed, and a new phosphide of boron, of the composition B_6P_3, is obtained.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus* δ) from India, presented by Mr. James Hammond; two Pink-footed Geese (*Anser brachyrhynchus*), British, presented by Mr. Cecil Smith, F.Z.S.; two Tuberculated Tortoises (*Homopus femoralis*) from South Africa, presented by the Rev. G. H. R. Fisk, C.M.Z.S.; two White-tailed Sea-Eagles (*Haliaëtus albicilla*), European, purchased.

OUR ASTRONOMICAL COLUMN.

THEORY OF ASTRONOMICAL ABERRATION.—An interesting point connected with astronomical aberration was raised by M. Mascart in a paper presented to the Paris Academy of Sciences on November 2. It would at first appear that if observations demonstrated that the constant of aberration had precisely the same value for all stars, the velocity of light in space must be uniform. This interpretation, however, seems open to objections. Eclipses of Jupiter's satellites furnish a method for determining the velocity of light in the space contained within the earth's orbit, and, as is well known, the results obtained in this manner agree very well with those deduced from experiments made on the surface of the earth. But astronomical aberration depends only upon the relation of the velocity of the observer to that of the light in the region occupied by the instrument, and is unaffected by any variations in the velocity of propagation of the light-waves between the object observed and the earth. A real difference in the constant of aberration given by different stars would therefore indicate that the velocity of light was not uniform in the parts of space traversed by the earth. From this reasoning, M. Mascart is led to conclude that the values derived from the experiments, direct and astronomical, made to determine the velocity of light, should be limited to the space contained within the terrestrial orbit. The induction is certainly a legitimate one, and it must be admitted that to consider the velocity of light in interstellar space as uniform is to rely entirely on hypothesis.

SUGGESTIONS FOR SECURING GREATER UNIFORMITY OF NOMENCLATURE IN BIOLOGY.[1]

COMPLAINTS are constantly being made, not only by laymen but by actual workers in science, of the increasing complexity of modern terminology. The fact is indisputable, but is it altogether to be regretted? Is it not rather the outward expression of fuller knowledge and clearer conceptions? If so, the complaints of those indolent persons who wish to gain a "general knowledge" of the subject with the least possible trouble to themselves are worthy of no more consideration than those of the landsman out yachting, of whom Mr. Hamerton writes:—

"You cannot speak of anything on board without employing technical terms which, however necessary, however unavoidable, will seem to him a foolish and useless affectation by which an amateur tries to give himself nautical airs. If you say 'the main-sheet,' he thinks you might have said more rationally and concisely, 'the cord by which you pull towards you that long pole which is under the biggest of the sails,' and if you say 'the starboard quarter,' he thinks you ought to have said, in simple English, 'that part of the vessel's side that is towards the back end of it and to your right hand when you are standing with your face looking forwards.'"

As a modern yacht or ironclad requires a more elaborate terminology than a fishing-boat or a trireme, so it is necessary that the exact morphology of to-day—to speak only of one branch of biology—should be weighted with a more extensive nomenclature than was required for the simpler comparative anatomy of former days. That many are repelled by the bristling outwork of more or less barbarous Greek and Latin compounds is undoubted, and is much to be regretted; but I quite fail to see that it can be avoided as long as we have to deal with a comparatively inflexible language like English. I would recommend anyone who is deeply impressed with the evils of the present system to try and translate a technical description of one of the ordinary students' types—say an earthworm or a crayfish—into "plain English" without loss of conciseness or lucidity.

I think it may be taken as axiomatic that whenever the bounds of knowledge are extended, either by the investigation of new problems or by the re-examination of old ones with the aid of improved methods and extended views, an elaboration of nomenclature is inevitable. Indeed, the introduction of an extended terminology, either because of the discovery of new facts or of the more accurate grouping of old ones, is a distinct gain; it emphasizes an actual advance in knowledge.

There are, however, certain undoubted evils connected with the introduction of new terms which must have troubled all of us at some time or other.

Two workers at a given subject living in different parts of the world invent each a terminology of his own. Each system is adopted by the inventor's own friends or countrymen, and no attempt is made by the general body of biologists to give either scheme official sanction—on grounds of priority or otherwise. In this respect systematists have a great advantage; if a given specific or generic name can be shown to have priority, it takes precedence of every other, however much more suitable the latter may be. Morphological names, on the other hand, always run the risk of being either ignored altogether, or ousted by others which, although no more appropriate, and perhaps considerably later in date, happen to be invented or adopted by some widely-read author.

New terms are sometimes proposed without a due sense of responsibility—on inadequate grounds or even from mere love of novelty; and, on the other hand, the conservative tendency leads to the continued employment of unsuitable terms when appropriate ones have been proposed in their place. New names are often casually introduced in the body of a large and highly technical paper, where they are certain to be seen by few; and, lastly, it frequently happens that such terms are inadequately defined.

Unfortunately this state of things can hardly be remedied by anything corresponding to the British Association's Rules, which have proved so useful in systematic zoology and botany. In these departments the appropriateness of a name is a matter of little importance, but in morphological nomenclature suitability is of far more importance than priority, and the most respectable and time-honoured terminology should never be allowed to stand in the way of one by which homologies, mutual relations, &c., are adequately expressed.

As morphology is essentially a progressive science, any attempt to draw up hard and fast rules on nomenclature is for the most part to be deprecated; the fittest must be allowed to survive. I think, however, that a few rules and definitions might be framed and afforded the official sanction, say, of the British, American, and Australasian Associations.

For instance, it is about time that we made up our minds as to what exactly we mean by *biology*, whether the whole science

[1] A Paper read before Section D (Biology) of the Australasian Association for the Advancement of Science, Christchurch, N.Z., by T. Jeffery Parker, B.Sc., F.R.S., Professor of Biology in the University of Otago, January 1891.

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of living things, or that department which deals with the mode of life of organisms—habits, relation to the environment, &c.; the former use of the term is almost universal in English-speaking countries, but many of the leading German writers give it the other signification. Again, *morphology* and *anatomy* are terms of fundamental importance, and zoologists and botanists might surely agree upon a common definition for each. The same applies to other terms common to the two sub-sciences, *ovary* being the most flagrant example of divergence. To take one more example, the word *biogenesis* was introduced by Prof. Huxley to signify the origin of organisms from pre-existing organisms. Eimer, in his recent work on organic evolution, uses the term *biogenetic law* for the law that individual recapitulates ancestral development.

Another matter, which might certainly be settled once for all, is the meaning to be attached to adjectives and prefixes denoting position, such as dorsal and ventral, anterior and posterior, proximal and distal, mesial and lateral, epi-, hypo-, pre-, post-, &c. Such terms of position, although easy enough to apply in most cases, are constantly being misused; *epipubis* (for *pre-pubis*) is a modern and widely-used term; the dorsal and ventral roots of the spinal nerves are still frequently called *anterior* and *posterior*, and the great body-veins the *superior* and *inferior* venæ cavæ; and the botanical use of many terms of position (*e.g.* the *dorsal* and *ventral* sutures of a carpel) is absolutely meaningless.

Another step in the right direction would be the publication, under the auspices of the British, American, and Australasian Associations, the Anatomische Gesellschaft, and similar bodies, of a glossary of biological terms, in which the history of the word, its inventor, the precise sense in which he used it, and any subsequent changes of meaning it may have undergone, would be set forth. Such a glossary might, I think, be usefully arranged under somewhat similar headings to those employed in the *Zoologischer Jahresbericht*, the whole work being of course supplied with an alphabetical index. With a single responsible editor, and a sub-editor for each department, the work would not be one of insuperable difficulty.

An even more practicable suggestion than the last, and one which, although supplementary to, is not dependent upon it, is that in such publications as the *Zoological Record*, the Journal of the Royal Microscopical Society, and the *Zoologischer Jahresbericht*, there should be a record of new terms as well as of new species. The recorders who do the work of these publications with such fidelity and success, would hardly find their labours increased by noting down all the new terms used by the authors in their various departments, and placing them in a special list, each being accompanied by name of author, date, and definition. If this were done, we should have fewer instances both of useless synonyms and of identical words being employed for totally distinct things. I do not think, for instance, that the body-cavity of Peripatus would have been called a *pseudocœle* by Mr. Sedgwick, or a *metacœle* by Mr. Hatchett Jackson, if these writers had had the means of knowing that the former term had been previously applied by Dr. Burt Wilder to the so-called fifth ventricle of the mammalian brain, and the latter to the fourth ventricle.

Finally, matters would be very much improved if every author who finds himself obliged to coin a word would notify the fact in a conspicuous part of his paper, accompanying the term with an adequate definition. One has only to point to Allman's monograph on the Gymnoblastic Hydroids, or to Haeckel's Report on the Deep-Sea Medusæ, to give a practical instance of the advantage of such a practice.

My proposals for promoting greater uniformity of nomenclature in biology may therefore be summed up under three heads, as follows:—

1. The appointment of a strong international committee to define terms of general and fundamental importance, such as the subdivisions of biological science, terms common to zoology and botany, terms denoting position, &c.
2. The issue of an authoritative historical glossary.
3. The systematic record of new terms.

METEORIC IRON.

THE *Annalen des k.k. naturh. Hofmuseums*, No. 2 of vol. vi., contains a further contribution by E. Cohen and E. Weinschenk to their interesting studies on meteoric irons.

By treating comparatively large masses in the cold with very dilute hydrochloric acid (1 in 20) so that the process of solution was very slow, in some cases extending to several months, a residue is left from which it is found possible to isolate several more or less definite compounds, distinct from the freely soluble main mass of the meteorite.

It is in the portion insoluble in the highly dilute acid, which in some cases amounts to no more than 5 per cent. of the whole, that the main interest in analytical work on meteoric irons centres. The patience and care involved in the separation of its various constituents often find their reward in some interesting discovery. As a typical example of the constituents into which a meteorite may be separated by this treatment with dilute acid, it will suffice to quote the percentage numbers obtained in the case of a slice of the Magura iron. They are as follows:—

Nickel-iron which passed into solution				92·67 per cent.
Cohenite	...	...	...	4·00 ,,
Tænite + jagged fragments	...	...		0·13 ,,
Schreibersite	...	...	...	0·09 ,,
Tænite + cohenite	...	...	...	2·93 ,,
Non-magnetic residue	...	...	...	0·18 ,,

In most meteoric irons the soluble portion consists to a large extent of a nickel-iron kamacite, which mainly constitutes the broad layers of the Widmanstätten figures seen on an etched polished surface. The authors are of opinion, from a comparison of various analyses, that this alloy has a constant composition represented by the formula $Fe_{14}Ni$.

Cohenite, which occurs in very brittle tin-white crystals, has at present been only found in the Magura iron. It was analyzed and described in a previous paper by Dr. Weinschenk, who found it to consist of a definite carbide of iron, nickel, and cobalt, having the composition represented by the formula $(FeNiCo)_3C$. Very similar crystals in the Wichita iron were found to have the composition represented by the formula $(FeNiCo)_4C$, analogous to the well-known spiegeleisen, Fe_4C. Cohenite corresponds to the carbide Fe_8C, which separates out in crystals when cast-iron is slowly cooled between 600° and 700°. Many points of resemblance such as this between meteoric and ordinary cast-iron appear to show that the conditions as regards temperature, &c., during their production must have been very similar in the two cases.

Tænite, occurring usually in thin silver-white lamellæ of great toughness between the broader layers of kamacite, is a nickel-iron, of which there appear to be two varieties, containing respectively about 65 and 73 per cent. of iron. Further analyses, however, are necessary in order to determine its true composition. The jagged and angular fragments of iron-black colour were analyzed, and found to consist of a nickel-iron containing about 7 per cent. of nickel, and were thus in all probability identical with kamacite.

The phosphor-nickel-iron schreibersite is generally found in large tabular crystals of tin-white colour. The new analyses show that its composition may be represented by the formula $(FeNiCo)_3P$. In some meteorites a phosphor-nickel-iron occurring in needles is found. This is the so-called rhabdite of Rose. Whether it is identical or not with schreibersite has not yet been decided, owing to the difficulty of obtaining pure material. A non-magnetic residue, consisting chiefly of transparent grains, the authors find is common in greater or less amount to most meteoric irons. In such residues a great variety of minerals have been identified with more or less certainty, such as diamond, cliftonite (a graphitic pseudomorph after diamond), quartz, tridymite, chromite, cordierite, garnet, corundum, pyroxenes both rhombic and monoclinic, &c.

The aim of the authors in the present investigation was to answer the following questions:—How widely distributed is cohenite? Are schreibersite and rhabdite definite compounds? Has kamacite a constant composition? Has tænite always the same physical and chemical properties? What is the composition of the jagged fragments so generally left undissolved after treatment of meteoric irons with dilute hydrochloric acid? How widely distributed are the transparent grains, as well as the diamond, cliftonite, &c.?

Unfortunately, owing to the fact that the present joint investigation had to be brought to a somewhat premature conclusion, a definite answer to all of these questions could not be given. We may, however, expect soon to hear more on the points still left undecided, as the promise is made that the gaps in the present investigation shall be filled up as soon as possible.

G. T. P.

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UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The first election to a Geographical Studentship will be held at the end of Hilary Term 1892. The student at the time of his election must have passed all the examinations for his B.A. degree in the University of Oxford, but must not be of more than eight years' standing from matriculation. Previous to his election he must have attended the lectures of the Reader in Geography in at least two terms. Information as to the conditions of tenure may be obtained from the Reader in Geography.

The Report of the Delegacy of Non-Collegiate Students was presented to Convocation on Wednesday. It shows that the list of matriculations is rather larger than in the preceding year, but the total number of undergraduates has somewhat decreased. The total number on the books (440) is the largest which has yet been reached. Thirty-six took the B.A. degree, and nineteen the M.A. during the year. The Delegates notify that they admit, without examination, students in any special branch of study who do not desire to pass through the Arts course, and can show evidence of fitness for their special subject. Sixteen students have availed themselves of this privilege during the year. The balance-sheet appended shows that the financial condition is satisfactory. The total receipts exceeded the expenditure by £550, and the accumulated balance in the hands of the Delegacy at the close of the year was £2284.

CAMBRIDGE.—Mr. J. Macalister Dodds, of Peterhouse, has been elected Chairman of the Examiners for the Mathematical Tripos, Part I.

A petition from 2689 persons residing in New Zealand has been received by the Vice-Chancellor, praying that the Senate will grant degrees to properly qualified women. The signatories include Sir George Grey, K.C.B., formerly Governor of New Zealand, most of the Ministers of the Colonial Government, and many professors and graduates of the University of New Zealand.

Lord Walsingham, F.R.S., High Steward of the University, offers annually a gold medal to B.A.'s of not more than two years' standing for the best monograph or essay giving evidence of original research on any botanical, geological, or zoological subject; zoology being understood to include animal morphology and physiology, and an essay on any subject of chemical physiology being valued according to its physiological rather than its chemical importance.

MR. R. W. STEWART, B.Sc. (Lond.), has been appointed Assistant Lecturer and Demonstrator in Physics at the University College of North Wales, Bangor.

SCIENTIFIC SERIALS.

THE *American Meteorological Journal* for October contains:—A short memoir of the late Prof. W. Ferrel, by Prof. A McAdie, with a complete list of his scientific contributions, from 1853 to 1891; his last paper, which appeared in our columns in April 1891, was entitled "The High-pressure Area of November 1889 in Central Europe."—The mineral waters of Ypsilanti and other places in Michigan, by Dr. E. N. Brainerd.—Cloud observations at sea, by Prof. C. Abbe. This is a preliminary report relative to the principal features of the work done by him during the recent cruise of the *Pensacola* to the West Coast of Africa. A number of experiments were made to determine the relative speed and direction of movement of the various strata of air, by means of clouds and small balloons. The experiments showed that the use of balloons is practicable both on sea and land, and gives accurate results. The following are some of the results of the author's observations: the vertical circulation increases and the horizontal circulation diminishes in the doldrums; the horizontal movement is a maximum at high latitudes; the bases of the cumuli are lower and their tops higher in the low latitudes; if there be any general east wind in the upper regions at the doldrum it is above the clouds, and therefore not observable.—The last article is by Dr. Leudet, on the action of climates at elevated stations on diseases of the chest.

Bulletin de l'Académie des Sciences de St. Pétersbourg, new series, vol. ii., No. 1.—On the scales of *Holoptychius* found in Russia, by Dr. Rohon (French). The histology of the same is described, as also two new species: *Hol. virium* and *Hol. superbus*.—Ichthyological notes from the Museum of the Academy, by S. Herzenstein, being a description of the following new species: *Cottus nivosus, Centridermichthys alcicornis, Hypsagonus gradiens, Stichæus grigoriewi, St. dictyogrammus, Chirolophus japonicus, Pleuronectes obscurus, Pl. japonicus, Pl. bicoloratus* (incompletely described by Basilewsky), *Hippoglossus grigoriewi, Alburnus charusini*, and *Nemachilus kuschakewitschi*.—On the extraordinary phenomena presented by the great comet of 1882, by Th. Bredichin. After having given in a preceding paper his reasons for considering the interior tube of that comet as an anomalous tail, the author applies the same explanation to the exterior tube.—On two new laws of celestial mechanics, by H. Struve. In addition to the previously communicated results of observations made on the satellites of Saturn with the 30-inch refractor, Prof. Struve points out the remarkable relations which exist between the satellites Mimas and Tethys on the one side, and Enceladus and Dione on the other side. The observations of Mimas have shown that its orbit has an inclination of 1° 26′ on the equator of Saturn, and that its nodes have a motion of 1° every day, so that by the end of the year the orbit returns to its previous position; moreover, a considerable acceleration has been noticed in the rotation of Mimas during the last few years. From these facts M. Struve deduces the following law:—"Four times the average movement of Tethys, minus twice the average movement of Mimas, is always equal to the sums of the average movements of the nodes of the orbits of Mimas and Tethys on the equator of the planet." The same law may also be expressed in this way:—"(1) The conjunctions of Mimas and Tethys always take place about a point which is situated halfway between the ascending nodes of their orbits on the equator of Saturn. They may move off this point for about 48°, and this libration is performed in sixty-eight years. (2) The conjunctions of Enceladus and Dione always coincide with the perisaturn of Enceladus, or, at least, they must oscillate around this point." Several important conclusions relative to the mass of Rhea and that of the rings may be deduced from these laws.—On the genus *Obolus* (Eichwald), by A. Mickwitz.—On a personal equation in photometric observations of stars, by E. Lindemann.—The mammals of the Gan-su expedition of 1884–87, by Eug. Büchner (German). The few species of this very interesting fauna which have been brought to St. Petersburg, are described, the remainder being kept in the Museum of Irkutsk.—On the rotation of Jupiter, by A. Belopolsky (in German). From a perusal of all available data, the author finds the rotation-period to be equal to 9h. 55m. in the latitudes from 10° to 45°, while in the zone 0° to 5°, it is only 9h. 50m.—On the Ammonites of the Artinsk strata, by A. Karpinsky (German). The collection is derived from North-East Russia; the new species are: *Pronorites postcarbonicus, Pr. præpermicus, Paraprononites tenuis, Gastrioceras suessi, Agathiceras uralicum, Popanoceras krasnopolskyi*, and *Thalassoceras gemmellaroi*.—On a new process for separating iron-oxide from aluminium, by F. Beilstein and R. Lüther.—Chemical notes, by N. Beketoff.—On the use of incandescent light for self-registering instruments, by H. Wild.—On artificial amphibolite, by K. Khrustschoff.

IN the *Botanical Gazette* for September and October, Mr. T. Holm continues his series of articles on the minute comparative anatomy of American grasses. Brief abstracts are given of the botanical papers read at the Washington meeting of the American Association for the Advancement of Science, and at that of the Botanical Club of the same Association. Other papers are chiefly of interest to American botanists.

SOCIETIES AND ACADEMIES.

LONDON.

Zoological Society, November 3.—Prof. W. H. Flower, F.R.S., President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the months of June, July, August, and September, 1891, and called attention to certain interesting accessions which had been received during that period.—The following objects were exhibited:—(1) On behalf of Mr. F. E. Blaauw, a stuffed specimen of a young Wondrous Grass-Finch (*Poephila mirabilis*), bred in captivity at his house in Holland; (2) on behalf of Prof. E. C. Stirling, a water-colour drawing of the new

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Australian Mammal *Notoryctes typhlops;* (3) by Mr. G. A. Boulenger, an Iguana with the tail reproduced; (4) by Mr. R. Gordon Wickham, a very fine pair of horns of the Gemsbok (*Oryx gazella*) from Port Elizabeth, South Africa; and (5) by Dr. Edward Hamilton, a photograph of an example of the Siberian Crane (*Grus leucogeranus*), shot on the island of Barra, Outer Hebrides, in August last.—Mr. R. Lydekker gave a description of some Pleistocene Bird-remains from the Sardinian and Corsican Islands. These belonged mostly to recent forms, but to genera and species which in several instances had not been found fossil. They showed rather more of an African character than the present avifauna of these islands.—Mr. R. Lydekker also read some notes on the remains of a large Stork from the Allier Miocene. These remains were referred to the genus, closely allied to *Ciconia*, lately named *Pelargopsis*, but which (that term being preoccupied) it was now proposed to rename *Pelargoides*.—Mr. R. Lydekker also exhibited and made remarks on the leg-bones of an extinct Dinornithine Bird from New Zealand, upon which he proposed to base a new species allied to *Pachyornis elephantopus* (Owen), and to call it, after the owner of the specimens, *Pachyornis rothschildi*.—Dr. A. Günther, F.R.S., read a description of a remarkable new Fish from Mauritius belonging to the genus *Scorpæna*, which he proposed to call *Scorpæna frondosa*.—A communication was read from Mr. Roland Trimen, containing an account of the occurrence of a specimen of the scarce Fish *Lophotes cepedianus*, Giorna, at the Cape of Good Hope.—A communication was read from the Hon. L. W. Rothschild, giving a description of a little-known species of *Papilio* from the Island of Lifu, Loyalty Group.—Mr. R. J. Lechmere Guppy read some remarks on a fine specimen of *Pleurotomaria* from the island of Tobago.—A communication was read from Mr. L. Péringuey, giving an account of a series of Beetles collected in Tropical South-western Africa by Mr. A. W. Eriksson.

Entomological Society, November 4.—Dr. D. Sharp, F.R.S., Vice-President, in the chair.—Mr. W. F. Kirby exhibited a series of a very dark-coloured form of *Apis* reared by Mr. John Hewitt, of Sheffield, from bees imported from Tunis, and which he proposed to call "Punic bees."—Mr. C. G. Barrett exhibited five melanic specimens of *Aplecta nebulosa*, reared from larvæ collected in Delamere Forest, Cheshire, and described in the Proceedings of the Lancashire and Cheshire Natural History Society as *A. nebulosa*, var. *Robsoni*. Mr. Barrett also exhibited a beautiful variety of *Argynnis aglaia*, taken in Norfolk by Dr. F. D. Wheeler, and two specimens of *Lycæna argiades*, taken in August 1885, on Bloxworth Heath, Dorsetshire.—Mr. H. St. John Donisthorpe exhibited a collection of Coleoptera, comprising about thirty-six species, made in a London granary in 1890 and 1891. The genera represented included *Sphodrus*, *Calathus*, *Quedius*, *Creophilus*, *Omalium*, *Trogosita*, *Silvanus*, *Lathridius*, *Dermestes*, *Anthrenus*, *Ptinus*, *Niptus*, *Anobium*, *Blaps*, *Tenebrio*, *Calandra*, and *Bruchus*.—Mr. A. B. Farn exhibited a series of specimens of *Eubolia lineolata*, bred from a specimen taken at Yarmouth. The series included several remarkable and beautiful varieties, and the size of the specimens was much above the average.—The Rev. Dr. Walker exhibited specimens of *Argynnis ino* and *A. pales*, from Norway.—Mr. B. A. Bower exhibited, for Mr. J. Gardner, specimens of *Nephopteryx splendidella*, H. S., *Botys lupulinalis*, Clk., and *Bryotropha obscurella*, Hein., taken at Hartlepool.—Mr. R. Adkin exhibited two very dark specimens of *Peronea cristana*, from the New Forest.—Colonel C. Swinhoe exhibited, and remarked on, types of genera and species of moths belonging to the *Tineina*, all of which had been described by Walker, and placed by him amongst the *Lithosidæ*.—Mr. H. Goss exhibited specimens of *Callimorpha hera*, taken by Major-General Carden in South Devon in August last, and observed that the species appeared to be getting commoner in this country, as General Carden had caught seventeen specimens in five days. Mr. Goss said that the object of the exhibition was to ascertain the opinion of the meeting as to the manner in which this species had been introduced into this country. A discussion on the geographical distribution of the species ensued, in which Mr. G. T. Baker, Colonel Swinhoe, Mr. McLachlan, Mr. Verrall, Captain Elwes, Mr. Barrett, Mr. Fenn, and others took part.—Mr. C. J. Gahan contributed a paper entitled "On South American Species of Diabrotica," Part III.—Mr. McLachlan contributed a paper entitled "Descriptions of New Species of Holopthalmous *Ascalaphidæ*."—Mr. W. L. Distant communicated a paper entitled "Descriptions of Four New Species of the Genus *Fulgora*."—Mr. F. Enock read a paper entitled "Additional Notes and Observations on the Life-history of *Atypus piceus*." Every detail in the life-history of this spider was most elaborately illustrated by a large number of photographs, made by Mr. Enock from his original drawings, and shown by means of the oxy-hydrogen lantern. A discussion followed, in which Mr. C. O. Waterhouse, Dr. Sharp, Mr. G. C. Champion, the Rev A. E. Eaton, and Mr. P. Crowley took part.

Anthropological Institute, November 10.—Dr. Edward B. Tylor, F.R.S., President, in the chair.—Mr. Francis Galton exhibited, on behalf of Lady Brooke, a photograph of a human figure carved on a rounded sandstone rock in Sarawak; the rock is about twelve feet in height, and the sculpture is in high relief and of the size of life. Mr. Galton also exhibited some imprints of the hand, by Dr. Forgeot, of the Laboratoire Criminale, Lyon.—Dr. Tylor read a paper on the limits of savage religion. In defining the religious systems of the lower races, so as to place them correctly in the history of culture, careful examination is necessary to separate the genuine developments of native theology from the effects of intercourse with civilized foreigners. Especially through missionary influence since 1500, ideas of dualistic and monotheistic deities, and of moral government of the world, have been implanted on native polytheism in various parts of the globe. For instance, as has lately become clear by the inquiries of anthropologists, the world-famous Great Spirit of the North American Indians arose from the teachings of the Jesuit missionaries in Canada early in the seventeenth century. This and analogous names for a Supreme Deity, unknown previously to native belief, have since spread over North America, amalgamating with native doctrines and ceremonial rites into highly interesting but perplexing combinations. The mistaken attribution to barbaric races of theological beliefs really belonging to the cultured world, as well as the development among these races of new religious formations under cultured influence, are due to several causes, which it is the object of this paper to examine: (1) direct adoption from foreign teachers; (2) the exaggeration of genuine native deities of a lower order into a God or Devil; (3) the conversion of native words, denoting a whole class of minor spiritual beings, such as ghosts or demons, into individual names, alleged to be those of a Supreme Good Deity or a rival Evil Deity. Detailed criticism of the names and descriptions of such beings in accounts of the religions of native tribes of America and Australasia was adduced, giving in many cases direct proof of the beliefs in question being borrowed or developed under foreign influence, and thus strengthening the writer's view that they, and ideas related to them, form no original part of the religion of the lower races. The problems involved are, however, of great difficulty, the only hope of their full solution in many cases lying in the researches of anthropologists and philologists minutely acquainted with the culture and languages of the districts; while such researches will require to be carried out without delay, before important evidence, still available, has disappeared.

PARIS.

Academy of Sciences, November 9.—M. Duchartre in the chair.—On the use of chronophotography for the study of machines constructed for aërial locomotion, by M. Marey. It is known that, in the case of a plane moving in a fluid medium, the centre of pressure only coincides with the centre of figure if the plane be normal to the direction of its motion; but if the plane makes an angle with its trajectory, the centre of pressure occurs in advance of the centre of figure to an extent which increases as the plane forms a more acute angle with the direction of motion, and as its velocity of translation is increased. This principle is strikingly illustrated by some photographs of a specially constructed falling body taken at intervals of a twentieth of a second. The body first described a sensibly parabolic curve, it then rose slightly, and passed over a convex curve before reaching the ground. The figure accompanying the paper shows clearly that the inflexions of the body's trajectory depend on the variations of its velocity, and the inclination of its surface with respect to the direction of motion.—On the laws of the intensity of light emitted by phosphorescent bodies, by M. Henri Becquerel. The author develops formulæ to represent the relation between the intensities of light emitted by phosphorescent bodies and the duration of illumination, and compares the results of some of

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his father's observations with those obtained by calculation. The agreement of the two sets of numbers is very close, even when the intensity was taken some thirty minutes after the body had been emitting light. A relation is also established between the intensity and the time that the body was exposed to light.—Study of boron phospho-iodides, by M. Henri Moissan. (See Notes, p. 67.)—M. Haton de la Goupillière made some remarks on the paper read by Sir William Thomson before the Royal Society on April 9, "On Electrostatic Screening by Gratings, &c.," saying that he had published some similar results in 1859.—Experimental determination of the velocity of propagation of electro-magnetic waves, by M. R. Blondlot. Experiments have been made between wave-lengths 8·94 and 35·36 metres, and the results show that all electrical undulations have a velocity of propagation of about 297,600 kilometres per second.—On algebraic integrals of the differential equation of the first order, by M. Autonne.—On surfaces with rational generators, by M. Lelieuvre.—Theory of turbo-machines, by M. Rateau.—A simple method of verifying the centres of the object-glasses of microscopes, by M. C. J. A. Leroy.—On the existence of acid or basic salts of monobasic acids in very dilute solutions, by M. Daniel Berthelot. The author has studied very dilute solutions near the point of neutralization, using HCl and baryta water at a concentration of 0·01 equivalent per litre, by the method of measuring the electric conductivities. He concludes that acid and basic salts are not destroyed by dilution, even very dilute solutions containing traces undecomposed.—On the formation of hydrates at high temperatures, by M. G. Rousseau.—On a double chloride of copper and lithium, by M. A. Chassevant. A substance of the composition $2\{CuCl_2.LiCl\} + 5H_2O$ has been obtained. It is decomposed by water, but is soluble to a red-brown solution in a concentrated solution of lithium chloride from which it may be recrystallized.—Researches on digitaleïn, by M. J. Houdas.—On isocinchonines, by MM. E. Jungfleisch and E. Leger.—Estimation of fats in milk products, by MM. Lezé and Allard.—Ptomaïnes extracted from urine in cases of some infectious maladies, by M. A. B. Griffiths. The ptomaïne from scarlet fever has the composition $C_5H_{13}NO_4$, that from diphtheria $C_{14}H_{17}N_2O_6$; they have also been prepared from pure cultures on peptonized gelatine of *Micrococcus scarlatinæ* and *Bacillus diphtheriæ* respectively. The ptomaïne from the urine of a case of mumps has the constitution $NH:C(NH_2).N(C_3H_7).CH_2.CO_2H$. None of the three ptomaïnes described are constituents of normal urine.—On the exterior form of the muscles of man with respect to the movements executed (experiments made by chronophotography), by M. G. Demeny.—On the formation of the peripheral nervous system of vertebrates, by M. P. Mitrophanow.—On the effects of parasitism on *Ustilago antherarum*, by M. Paul Vuillemin.—Meteorological observations made at Rodez, by M. des Vallières.

AMSTERDAM.

Royal Academy of Sciences, October 31.—Prof. van de Sande Bakhuyzen in the chair.—Prof. P. H. Schoute offered some general remarks on Lemoine's two problems of stamps: In how many different ways a ribbon of p stamps and a rectangular sheet of pq stamps can be folded up in one? (compare vol. i. p. 120 of the "Théorie des Nombres" of E. Lucas). He reduced the first problem to a question in the theory of permutations, gave the number x_p of its solutions up to $p = q$, and showed why the number $x_{p,q}$ of the solutions of the second problem must surpass the expression $\binom{p+q-2}{p-1} x_p x_q$.—Prof. B. J. Stokvis made a contribution to our knowledge of mutual antagonism and the combined action of mutual antagonists. In experimenting on the isolated and freely pulsating frog's heart, he stated that digitaline on the one side, and muscarine (or chinine) on the other, were to be considered as mutual antagonists for this organ, and displayed their antagonistic action, whichever of the two might be applied first. In another series of experiments he studied the action of muscarine and digitaline flowing at the same time with the nourishing blood through the isolated frog's heart, and found that the greatest antagonistic action, for instance of digitaline, was displayed when very dilute solutions (1 : 25,000 or 1 : 33,333) were applied at the same time as moderately strong solutions of muscarine. Finally, he stated that the isolated frog's heart recovered much faster and much more easily by normal blood when it was previously poisoned by muscarine and digitaline combined than when it was poisoned by the same or even a lower dose of muscarine alone.—MM. S. Hoogewerff and W. A. van Dorp gave an account of the action of an aqueous solution of ammonia on phthalic chloride. If care is taken to keep the liquids cool in mixing, about 40 per cent. of the weight of the chloride is converted into orthocyanobenzoic acid, $C_6H_4\begin{matrix}CN\\COOH\end{matrix}$.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

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PAMPHLET.—A Memoir on the Coefficients of Numbers: B. Seal (Calcutta).

SERIALS.—Bacteriological World, vol. i. No. 10 (Battle Creek, Mich.).—Proceedings of the Aristotelian Society, vol. i., No. 4, Part 2 (Williams and Norgate).—Himmel und Erde, November (Berlin, Paetel).—Bulletin de l'Académie Impériale des Sciences de St. Pétersbourg, nouvelle série ii. (xxxiv.) (St. Pétersbourg).—Harvard University Bulletin, No. 50.

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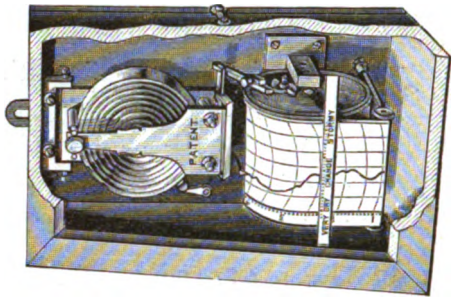

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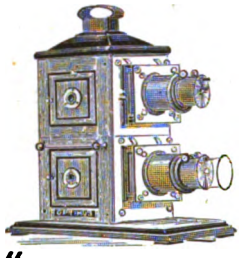

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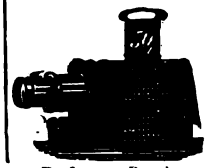

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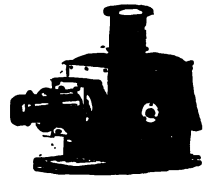

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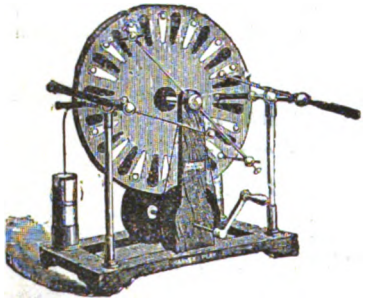

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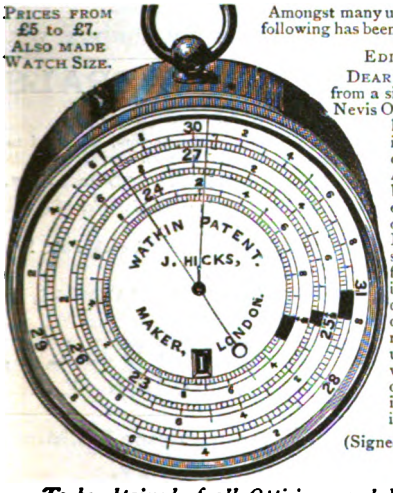

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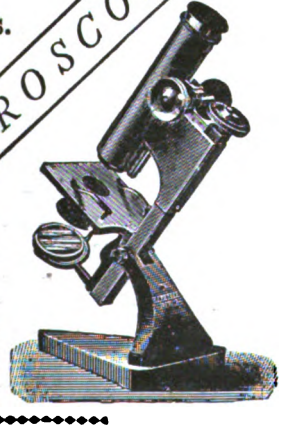

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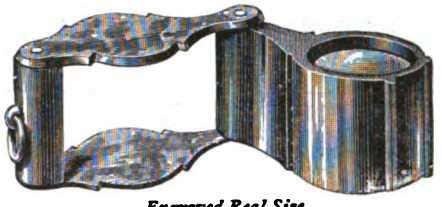

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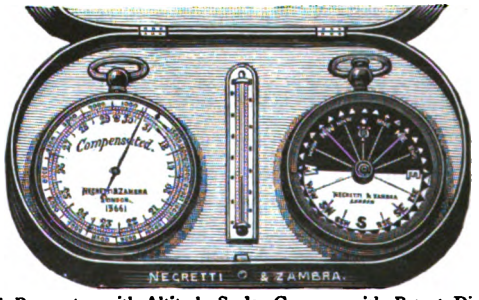

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THURSDAY, NOVEMBER 26, 1891.

ELECTRO-MAGNETISM.

The Electro-magnet and Electro-magnetic Mechanism. By S. P. Thompson, D.Sc., F.R.S. (London: E. and F. N. Spon, 1891.)

THIS is a reprint, with additions, of a course of Cantor Lectures which the author delivered during 1890 before the Society of Arts. The book, it may be stated at the outset, is an excellent account of electro-magnetic mechanism, and abounds in information at once of historical, scientific, and practical value. Evidently the author is willing to spare no pains to give completeness to any work of this kind he undertakes, and the present book, like his treatise on dynamo-electric machinery, will no doubt be widely read and appreciated.

In the preface, and elsewhere in the body of the book itself, Dr. Thompson indulges in some statements which we think are a blemish on an excellent and well-written treatise. It serves no good purpose to distribute in a scientific book praise or blame to certain classes of scientific investigators, and while glorifying a certain section of workers, to pour what seems little short of contempt and derision on the labours of another. We refer here to the statements (chiefly in the preface) regarding the earlier mathematical theories of magnetism, and their alleged influence in retarding electro-magnetic discovery.

It is quite true that some of the older theorists, concentrating their attention on the magnetic field of a system of permanent magnets, gave only a secondary attention to the problem of the internal constitution of a magnet. But it is hardly fair to put down against them, by inference, the errors of the persons who persisted in assuming that, because it had been proved that a magnet produces a field which can also be accounted for by a distribution of imaginary magnetic matter over the surface of the magnet, the magnetization of the magnet did consist in such a distribution. Such an equivalent distribution is a conception helpful in itself, inasmuch as it can be experimentally determined, and expresses exactly the manner in which the lines (unit tubes) of magnetic induction leave the surface. But to accuse it of misleading those who misunderstood it, is to put on the shoulders of Gauss and others who considered surface distributions a responsibility which is not theirs, and a blame which properly belongs to the perverse experimentalists who have insisted on determining the actual positions of "poles" in bar magnets, and on measuring other things equally indeterminate or non-existent.

But is the following statement quite free from possibility of misconstruction? "Gradually, however, new light dawned. It became customary, in spite of the mathematicians, to regard the magnetism of a magnet as something that traverses or circulates around a definite path, flowing more freely through such substances as iron than through other relatively non-magnetic materials." If any student of the subject does get into his head the idea that an actual material something flows round a magnetic circuit, he might quite as justly hold Dr. Thompson responsible for this false notion as blame the mathematicians for causing him to suppose a magnet to be a body plastered over with imaginary magnetic matter.

In tracing the evolution of the notion of magnetic permeability, Dr. Thompson might have made some mention of the contributions to molecular theory given by Poisson and others; for after all, these men really knew as well as we do "that magnetism, so far from residing in the end or surface of the magnet, is a property resident throughout the mass." But, as has already been stated, their attention was chiefly directed to the fields of permanent magnets; and to find a doctrine which "will afford a basis of calculation such as is required by the electrical engineer" is the business of the electrical engineer himself, and others interested in the proble m Hitherto, indeed, so far as such a doctrine has been found, it has been discovered in great measure by the physical mathematician! The mathematical doctrine o "magnetic permeability," or—to use Faraday's phrase—"conductivity for magnetic lines of force," was given so long ago as 1872 by Sir William Thomson, and fully illustrated by analogies and applications; and it is certainly curious that so accurate an historian as Dr. Thompson should, in his historical *résumé*, have made no mention of this very important paper. It is also the fact that our knowledge of the properties of iron, which, with the "simple law of the magnetic circuit," constitutes, according to Dr. Thompson, the stock-in-trade of the designers of dynamos, has come in no small degree from the same source. The most eminent investigator and improver of the dynamo is also a mathematician; but perhaps, as Sir William Hamilton (of Edinburgh, not of Dublin!) said of a certain mathematician who, he was forced to admit, reasoned correctly, he did it in spite of his mathematics, not because of them.

All honour to the great mathematicians who first attacked the difficult subject of magnetic action. But even if they had sinned so grievously by their unpracticality as it seems now the fashion to try to make out, their successors, entering into their labours, have been able to do much to atone by helping the engineer in the difficulties which beset his advance into a region, viewed indeed from afar, but hitherto untrodden. In this great work the engineer and mathematician are necessarily companions, and for either to reproach the other, as to what happened or did not happen in the past, is only to provoke useless recrimination, and delay their onward progress.

It is time now to come to the subject-matter proper of the book, and for this we have, on the whole, nothing but commendation. There are some things which might, perhaps, have been expressed differently with advantage, but this, of course, is only a matter of opinion. In the first chapter a very interesting account is given of the inventions and researches carried out by Sturgeon, Henry, and Joule, and ends with a description of notable electro-magnets. The sketch of Sturgeon's career, given in an appendix to this chapter, is of remarkable interest as a plain unvarnished record of heroic toil in the pursuit of knowledge, carried on amid uncongenial surroundings, and in spite of the hard pinch of poverty. In happier circumstances, this experimental genius would no doubt have done much for science. Still, to us there is some compensation, for, had it not

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been for the *res angusta domi* of his later years, we might not have had his "Annals of Electricity" or his "Scientific Researches." As it is, it seems well established that he was the pioneer of electro-magnetic discovery, the maker of the first electro-magnet and the first magneto-electric machine; and Dr. Thompson has done a simple act of justice in bringing Sturgeon's claims in this respect before the great public now interested in the progress of electrical science.

In chapter ii. we have generalities concerning electro-magnets and electro-magnetism, descriptions of typical forms of electro-magnets, and materials of construction. First are discussed such topics as the uses of electro-magnets, magnetic polarity, magnetic units, and elementary propositions in electro-magnetism. Here we would remark, as a merit in this work, the fact that the author has not loaded what is intended to be a thoroughly practical treatise with long discussions of purely theoretical matters, however important in relation to practice. The sketches of theory given in many works—which are really only collections of tables of numerical data, lists of formulæ, and workshop recipes—would be much better left out, and their places supplied by other matter, or the books lightened by their absence.

An excellent chapter follows, on the properties of iron. The various methods of measuring permeability are well explained, and for the space devoted to it a good account (with tables) is given of the results obtained by Hopkinson and Ewing in their researches. On p. 108 the effect of opening gaps in a magnetic circuit of iron is discussed, and a reference is given to an experiment described later in the work. On turning to this experiment (p. 212), it is found to be a description of an exploration of the effect produced at different parts of a horseshoe steel magnet by pulling off the keeper. A narrow coil of a few turns of wire is wound on a light frame capable of being slided round the magnet, and is connected with a ballistic galvanometer. The effect of pulling off the keeper is then tested with the coil in different positions on the horseshoe, and is shown by the deflection of the needle of the ballistic galvanometer. The theory of the author is, that putting on or taking off the keeper of a permanent steel magnet does not affect the magnetization at the middle of the horseshoe; that by putting on the keeper, and so diminishing the magnetic "reluctance" of the circuit, the lines of magnetic force are only collected, not altered in amount. Hence, if this theory be true, the coil, when placed at the middle of the magnet, should show no effect on the removal of the keeper. It is stated that careful and repeated experiments made at Finsbury gave an effect at the middle of the magnet which did not amount to 1/3000 of that found when the coil was close up to either end of the magnet.

Some time ago, when informed by a friend of this statement and result, the writer, feeling extremely doubtful of their general truth, had a magnet constructed for the purpose of repeating the experiments. As a large magnet was required for other purposes, one was constructed of eight steel bars of a mean length of about 3 feet. Each bar was 2 inches broad by ¼ inch thick, so that when put together they formed a large horseshoe of square cross-section 2 inches in side. A keeper, made of a block of soft iron, fitted between the ends of the horseshoe. The steel, which was tool steel obtained from a local firm, took magnetism readily, and an excellent horseshoe magnet was obtained.

A coil in circuit with a ballistic galvanometer was used in the manner described above, to test the effect of removing the keeper. Careful experiments made by students, and repeated by the writer, gave an entirely different result from that obtained by Prof. Thompson. A very large throw was obtained by placing the coil close to either end of the magnet and detaching the keeper; but with the coil as nearly as possible at the middle of the horseshoe, the throw was about *one-eighth* of the maximum. It was verified, moreover, that the minimum throw was obtained at the middle.

This result was exactly what the writer had expected would in general happen. The so-called free magnetism at the extremities of the magnet, in the absence of the keeper, produced a demagnetizing effect throughout the magnet, and thus diminished the induction through the coil, even when at the middle. This action was counteracted by the magnetizing effect of the keeper when in position, and therefore itself inductively magnetized, but had full play as soon as the keeper was removed. The much greater deflections near the ends were undoubtedly due to the cause to which Dr. Thompson would assign the whole effect—the alterations of the arrangement of the lines near the ends which accompanied the removal and replacement of the keeper.

It is certain that this effect will depend on the permeability of the magnet steel, which is a function of the magnetization; but that such an effect will in general be produced there does not seem to be any room for doubt.

The following chapters deal with specially designed electro-magnets, such as, for example, those used in relays and clockwork, electro-magnetic mechanism, alternate-current electro-magnets, electro-magnetic motors and machine tools, and the purely electro-magnetic part of the book winds up with a very interesting chapter on the electro-magnet in surgery. The last chapter of all is devoted to permanent magnets. To give a satisfactory account of these chapters is here impossible; but it may be mentioned that the electro-magnetic mechanism fully described and figured includes no less than nine classes, beginning with the different forms of magnet with moving armature or plunger, magnets with armatures moving against counterpoises of different kinds, polarized devices, electro-magnetic vibrators, magnetic brakes, &c.

In chapter xi., on alternate-current electro-magnets, the modes of laminating magnets for the prevention of eddy-currents are described; then follows a discussion of effects of alternating electro-magnets, depending on the difference of the phase relations of the magnets and the eddy-currents excited in conductors in the form of disks and rings placed near the extremities of the iron cores. Thus we have a very interesting account of Elihu Thomson's remarkable experiments. The throttling or impedance effect of electro-magnets included in circuits is next treated. It would have been worth noticing, where the relations of maximum current, maximum electromotive force, mean current, mean electromotive force, and impedance are given, that the true mean value of the total electrical activity in an alternating circuit, in which

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work is done against resistance only, as in lighting glow-lamps, is equal to the mean square of the current multiplied by the total resistance in circuit.

In the chapter on the electro-magnet in surgery, Dr. Thompson, as was to be expected, gives a careful summary of the history of the subject. It seems that hitherto the principal use has been in ophthalmic surgery, for the extraction of small particles of iron from the eyeball. A very interesting case is narrated, in which the author's brother, Dr. Tatham Thompson, of Cardiff, successfully removed, by a magnetic probe constructed according to his own design, a fragment of iron which had passed through the eye of a blacksmith and lodged in the retina. The probe, excited by a coil, was inserted through the vitreous humour of the eye along the track of the wound, and catching the bit of steel, drew it from its resting-place in the retina. With the exception of a slight limitation of the field of vision, the patient perfectly recovered his sight.

The writer has a lively remembrance of being present some years ago at a search made, by means of a magnetic probe which he had arranged for one of the surgeons of Glasgow Infirmary, for a fragment of iron which was alleged to have entered the knee of a blacksmith. The blacksmith was exceedingly lame, and an examination of the knee by means of a magnetometer for signs of the presence of iron had resulted in some very puzzling indications. All iron, it was thought, had been removed from the patient, and with the knee in certain positions very decided indications of magnetized iron or steel had been observed. The probe, magnetized by a battery current, was inserted in an incision made at the place where the iron was supposed to have entered, but without effect. An adhesion was found to exist in the joint, and was broken down by the surgeon, with the result that after the patient's recovery the lameness had disappeared. It was found on stripping the patient that he wore a truss, and hence the magnetometer effects!

It seemed rather unlikely, in any case, that the magnetic probe would be effective in removing a splinter deeply embedded in muscular tissue.

The last chapter, on permanent magnets, though no part of the subject-matter proper of the book, is nevertheless very complete, and full of valuable information regarding the magnetizability of different kinds of steel, effects of temperature, use of laminated magnets, lifting power of magnets, and permanence of magnets with lapse of time.

The chapter concludes with a short sketch of astatic arrangements of needles. In connection with these, it may be remarked that the vertical pair of astatic needles—which has the advantage of perfect and permanent astaticism, inasmuch as each needle is astatic—was described, with other astatic arrangements, in a paper by Mr. T. Gray and the present writer, in the Proceedings of the Royal Society for 1884 (vol. xxxvi.).

Here we must take leave of a most interesting volume. The outside of the book is tastefully got up, the printing and paper are excellent, and the whole is worthy of the reputation of both author and publishers.

A. GRAY.

FUNGUS EATING.

British Edible Fungi: how to Distinguish and how to Cook them. By M. C. Cooke. (London: Kegan Paul, Trench, Trübner, and Co., 1891.)

THERE is a fascination in the minds of many people for the eating of fungi, which has often been expressed in popular books on the subject by authors of various degrees of power. Some of these are enthusiasts, who suffer palpably from that malady of intolerance which characterizes so many propagandists, who would persuade us that those things are best to eat and drink which they have found so; others, again, temper their recommendations by the calm arguments of the scientific man, though, speaking as having found, they must write with a tinge of that persuasiveness necessary to overcome wide-spread prejudice, or ignorance, if they are to be understood by the multitude; others, again, are content to state the facts, and let the logic of their sentences make its own impression in due course.

It is not easy to say off-hand in which of these categories the writer of the book under review should be classed, nor is it of much consequence to look at his writings according to the decision arrived at in that connection. It is certainly no more than fair to say that Mr. Cooke has compiled a little book of no small value as a guide to common edible fungi of this country; and that, while the information is singularly clear, and told in plain and homely language, it loses little or nothing in the simple telling, so far as the facts about these cryptogams are concerned. In so far, the author's well-known reputation as a mycologist is not likely to suffer; but it must be added that, while we do not pretend to criticize in detail all that relates to the cooking of these delicacies, and while it must always be more or less a matter of individual opinion whether *Champignons* stewed with pepper and butter, or *Boletus* with sauce of lemon-juice and powdered lump-sugar, are the more delicious, there is something that smacks of prejudice in the more than hearty commendation of some of the cooked favourites. Be this as it may, Mr. Cooke is unquestionably a high authority on his subject, and he has done good service in supplying the public with a well-written account of it, and with excellent coloured drawings of about forty of the chief forms of edible fungi.

Few people in this country are aware that nearly two hundred of the things called toadstools are at least edible, and fewer still will be prepared to believe that there are people in the world who regard some fifty-odd of these as dainties. Mushrooms, truffles, and morels exhaust the list for most Englishmen, and many are dubious about even these, and eat them, when served, with various degrees of trustful confidence, or the reverse.

It is a severe shock to these persons when they learn that such species as *Amanita rubescens*, *Lepiota procerus*, *Coprinus comatus*, and other "horrid toadstools," may not only be eaten, but are even extolled as delicious; and when it comes to recommending the green-topped *Clitocybe odorus*, smelling of anise, or the purple *Tricholoma nudus*, and other violently coloured species, the amateur may be pardoned for hesitating. Nevertheless, these and many other forms popularly held as dangerous are not only edible, but are also capable of distinction with a

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little practice. It may, perhaps, be doubted whether specialists do not commonly underrate the difficulties in diagnosis of these forms to an amateur, but Mr. Cooke can at least be felicitated on having made the distinguishing characters as clear as popular language admits.

In addition to the chapters on edible fungi, there is an interesting one on poisonous species, which may convince the reader of error in supposing that these are so numerous as is vulgarly supposed. The fact is, we know very little about the poisonous forms. Some few are thoroughly established as poisonous, and great care should be exercised by amateurs in view of this fact; but by far the majority of toadstools are simply not known to be edible. The following paragraph, from p. 208, may be worth quoting:—

"Over and over again have we been urged to lay down some rules, or instructions, whereby poisonous may be distinguished from innocuous fungi. As often have we declared, as we do now, that such general instructions are impossible. No rules can be given whereby a poisonous can be distinguished from a harmless species, nothing except knowledge and experience. The poisonous species already known are known because they have a past history which has condemned them, and not from any evidence written upon them. The most experienced mycologist cannot tell by any character, feature, or behaviour, that this or that fungus is poisonous or the reverse. He only knows its antecedents and the company it keeps. A large order of flowering plants, such as the *Solanaceæ*, may be looked upon with suspicion; but the potato and tomato are not poisons. In the *Agarics* the sub-genus *Amanita*, with warted caps, have always been regarded with suspicion because of *Agaricus muscarius* and *Agaricus phalloides;* but two others of the same group, *Agaricus rubescens* and *Agaricus strobiliformis*, are most excellent food."

This, in spite of traces of obscurity, may be taken as sound talk, showing an appreciation of the position by the author, who is, moreover, quite alive to what generalizations can be made as to the dangerous character of pink-spored genera, and the bright red forms of *Agaricus* and *Russula*, and so on; and we recommend this chapter of warnings to all who are inclined to taste rashly.

The printing of the text and illustrations is decidedly good, and we regard this little work as a gain to the popular naturalist's library.

"EXTENSION" PSYCHOLOGY.

Psychology: a Short Account of the Human Mind. By F. S. Granger, M.A. (London: Messrs. Methuen and Co., 1891.)

THIS little volume, which forms one of Messrs. Methuen's "University Extension Series," is not a mere compilation from larger works on psychology. Short as it is, it presents an independent development of the subject; and it is not infrequently characterized by a freshness and vigour most helpful to the student. We feel sure that Mr. Granger's short account of the human mind will be a most useful little book in the "psychology circles" of that National Home-Reading Union to the existence and work of which Dr. Hill drew attention in a recent issue of NATURE. And few subjects are better suited than psychology for study in this way.

Mr. Granger lays great stress on the essential unity of the wave of consciousness that in its onward course constitutes our mental life. By introspection (which is also retrospection) we may analyze the waves that have passed; we may find elements of cognition, of feeling, and of will; we may separate out dominant and subdominant ideas, sensations from without the body and from within. But in the wave as it passes all these are fused into one state of consciousness from which no one of the constituent elements could be omitted without destroying its identity. Students are only too apt to fancy that the elements reached by psychological analysis have a reality more real than the state of consciousness with which they started. Mr. Granger does his best to help them to avoid this fallacy.

Welcome as the little book is from its independence of treatment, it shows signs of hurried composition. Apart from bits of sheer carelessness—*e.g.* where Mr. Granger speaks of "the facts with which we have to deal in"—the statements are occasionally ambiguous or misleading "The retina, for example, may be supposed to transform all sorts of stimuli into sight impressions." That is not well put. "The causes which give rise to states of mind may be divided into states of mind and states of body. Thus the sensation of red is due to a light-stimulus received from some object by way of the eye. Here, a state of mind, the sensation of red, is caused by a state of body—namely, a change in the retina. All cases of this kind, in which a mental effect follows upon a physical cause, are instances of physical laws. On the other hand, states of mind are very often due to previous states of mind," &c. This, again, is awkward and misleading. The physical basis of a "sensation of red" is found in certain molecular changes in the brain; and these may be induced by changes in the retina (external origin), or by other changes in surrounding parts of the brain (internal origin). So far as the molecular changes which are the basis of the "sensation of red" are concerned, whether they be due to causes external to the brain or to causes within the brain, they are alike "instances of physical laws." Here, and elsewhere in the book, the fundamental distinction between energy and consciousness, between the physical and the psychical, is not brought out with sufficient clearness.

In his preface, Mr. Granger tells us that "the illustrations are largely drawn from every-day experiences and familiar books." If the following illustration correctly represents Mr. Granger's every-day experiences, Nottingham must be a somewhat dangerous place to live in. "A group of ideas may come into the mind, and by the process of association may bring up an idea of some movement or series of movements which is forthwith realized. The sight of a stranger leads to the idea, and then to the act, of throwing half a brick at him." Even if the experience is of every-day occurrence, it can hardly be regarded as a happy illustration. C. LL. M.

OUR BOOK SHELF.

Arithmetical Exercises in Chemistry. By Leonard Dobbin, Ph.D. With Preface by Prof. Crum Brown. (Edinburgh: James Thin, 1891.)

THIS book is a short collection of problems dealing with chemical changes, and such physical changes as are

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of special importance to junior students of chemistry Examples are given on the metric system, thermometric scales, specific gravity, the gaseous laws, the weights and volumes of materials entering into chemical reactions, and on the calculation of the empirical formulæ and percentage composition of compounds. As distinguished from others on the same subject, the book contains no more than the beginner requires, and is therefore less apt to confuse than one more elaborate. The introductory part of each section, giving the principles involved in the exercises, is clear, and the examples, 128 in all, seem well chosen. A table of contents, index, and answers, are included. A few points in connection with specific gravity might with profit be attended to. On p. 6, density, in reality absolute density, is defined, and then specific gravity. Relative density, as it is referred to on p. 31, might here be introduced, and the account would be more complete if the relation between density and specific gravity were clearly stated. After the formula $s = w/v$, on p. 9. it might be definitely pointed out that, with the units chosen, specific gravity is the weight of unit volume. It is not quite accurate to lead the student, as on p. 8, to infer that specific gravities are usually given for the temperature of 4° C.

The Colliery Manager's Hand-book. A Comprehensive Treatise on the Laying-out and Working of Collieries, designed as a Book of Reference for Colliery Managers, and for the use of Coal-mining Students preparing for First-class Certificates. By Caleb Pamely. Pp. 552, Index, and 472 Woodcuts. (London: Crosby Lockwood and Son, 1891.)

NO doubt colliery managers, and students preparing for examinations, would find it convenient to have a cyclopædia of mining, but the hand-book under consideration will scarcely satisfy their wants. The work is another illustration of the proverb, "Ne sutor ultra crepidam." Instead of being satisfied with describing mining processes and mining plant, the author deliberately plunges into geology and chemistry, and then finds himself quite out of his depth.

It is evident that much labour has been expended in bringing together information upon various matters connected with coal-mining; but there are gaps which require to be filled up, and errors that should be corrected in a second edition. For instance, the chapter upon boring is meagre. The part relating to percussive boring is in the main borrowed from Greenwell, and refers solely to boring by hand. Not a word is said about free-falling tools. It is not correct to say that "a great advantage of the diamond-drill boring is that the hole is kept true and vertical." The surveys made with the ingenious "clinograph" of Macgeorge have shown beyond a doubt that this is not invariably the case.

The description of tools is insufficient; and strange to say, the book contains no figure of a pick. Probably no work on mining was ever written before without a figure of this characteristic miner's implement. It is not for want of space, because there are figures to illustrate the manner of preparing oxygen and nitrous oxide. These are really unnecessary: if the student wants to learn a little elementary chemistry, he had better have recourse to one of the numerous text-books on that subject.

The examples of the different methods of working coal are likely to confuse students, owing to the mass of details by which they are accompanied. As the original articles are available in the Transactions of the Mining Institute of Scotland, short abstracts would have been quite sufficient.

Serious omissions are somewhat numerous. Rittinger pumps, which are doing such excellent work on the Continent, are not mentioned; and the following important subjects are also entirely ignored: coal-washing, coking, utilization of the gases for making tar and ammonia, and manufacture of patent fuel. In these days, when warning notes are being sounded concerning the duration of our coal supply, the attention of mining engineers and students should be specially directed to methods of turning small coal and inferior seams to profitable account. The author of a hand-book ought to be in advance of the times, and point out the path of economy and progress.

In spite of its defects, Mr. Pamely's work is by no means destitute of value. It contains a great deal of information which managers and students will find of use to them, and the excellent index will enable them to lay their hands at once on any part they desire to consult.

C. L. N. F.

Photography applied to the Microscope. By F. W. Mills. (London: Iliffe and Son, 1891.)

THE subject of photo-micrography is one of such absorbing interest that it is no wonder it has become so popular among amateurs. For those commencing, and for those who have already made some steps in pursuing this subject, the present little book is intended, and it will be found to contain just that sort of information and advice that is so necessary to a beginner. The main point to insure good photographic results lies in the preparation of the object, which requires both patience and care; the chapter dealing on this has been written by Mr. Charters White, who gives good directions for cutting hard and soft tissues, and for bringing materials, that are too soft in their natural condition, to a state of firmness previous to cutting. With regard to the choice of the necessary apparatus, the author describes various forms that are cheap, and which with care can be made to yield fair results. In the remaining chapters all the photographic manipulations are dealt with, such as exposing, developing, and printing, &c., and at the end is added a useful list of works which treat of the subject under consideration.

Copernic et la Découverte du Système du Monde. By Camille Flammarion. (Paris: Marpon and Flammarion, 1891.)

AN interesting book is this, and one full of information. In ten chapters and 250 pages M. Flammarion traces the history of astronomy from Copernicus to Newton, with special reference to the life of the former and the development of his system. There is little doubt that this work will be as well received as others by the same writer, and it really deserves the favour. G.

Annals of British Geology, 1890. By J. F. Blake. (London: Dulau and Co., 1891.)

IT is intended that this shall be the first issue of an annual publication; and, if future volumes are prepared with as much care as the present one, the work ought to be of much service to geologists. Its scope is restricted to writings which have appeared in the United Kingdom. The author is not content with noting merely the titles of the works he records. When they are of the least importance, he gives a general idea of their contents, and presents what seems to him an adequate critical estimate of their value. The classification is by subjects. He begins with general geology; then come stratigraphical geology, palæontology, palæobotany, petrology, economics, maps and sections, and works relating to foreign geology, but published in Britain. A section headed "Personal Items" brings together a number of facts to which it may sometimes be convenient for the student of geology to refer. The volume deserves to be all the more cordially welcomed because Mr. Blake is not of opinion that he has at one stroke reached perfection. He hopes that future volumes may be improved by the co-operation of specialists in the several departments.

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LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Warning Colours.

IN the experiments on "Comparative Palatability," recorded in NATURE of November 19 (p. 53), Mr. E. B. Titchener refers to the unpalatability of the brimstone butterfly. The insect was "fairly seized several times," but "was always rejected," by a frog. Some of your readers may not be aware that Mr. F. Gowland Hopkins, of Guy's Hospital, has recently shown that the yellow pigment of this butterfly, and of several others of its allies, is due to a substance formed as a urinary pigment; it is also known that the colours of other butterflies, and other animals, bear a relation to the urinary pigments. These substances may be in many cases of a disagreeable flavour. Dr. Eisig, of the Naples Zoological Station, has suggested that if intense and varied coloration is primarily due to a great quantity and variety of such bitter-tasting pigments, we do not need to assume that the brilliant coloration has been brought about in order to advertise the nauseous taste. The bright and varied colour will be, in fact, a consequence of the deposition in the integument of bitter pigments. This view—which has for the most part escaped the attention of those who have written upon animal colours, owing doubtless to its having been put forward in a special monograph upon a group of worms (*Capitellidæ*)—better explains how it is that brightly-coloured unpalatable creatures are in so many (? the majority of) cases tasted before being refused. I have laid some stress upon this view of warning coloration in a forthcoming book upon "Animal Coloration," which is to be published by Messrs. Swan Sonnenschein and Co. FRANK E. BEDDARD.

Zoological Society's Gardens, Regent's Park.

The Salts in Natural Waters.

THE communication of Mr. Lyons, in NATURE of November 12 (vol. xlv. p. 30), giving an analysis of the water of the salt lake of Aalia Paakai, affords a suitable opportunity for asking a question, to which, I trust, some chemist among the readers of NATURE will be able to give a satisfactory answer.

The usual analysis of the "solid constituents" of a given specimen of natural water only *directly* determines, I believe, the respective quantities of the metallic bases—sodium, calcium, &c.—and of the non-metallic constituents—chlorine, carbonic acid, &c.—contained in the "total solids." How does the chemist, then, proceed to mate these two classes of constituents together, so as to be able to state with confidence what salts, and in what quantities, are held in solution in the water? The problem itself would appear to be an *indeterminate* one, at any rate if there are more than two of either class of constituents. What additional considerations are introduced to render the problem determinate? Are they definite chemical conditions; or is there more or less arbitrariness in the assumptions made, so that two chemists would not necessarily arrive at the same result?

In the case of the Honolulu lake, there are, according to Mr. Lyons's analysis, three non-metallic constituents (*chlorine, bromine, sulphuric acid*)—(is not the absence of *carbonic acid* remarkable?)—and four metals (*sodium, potassium, calcium, magnesium*); the quantities of which have, I suppose, been obtained by direct analysis. From the *twelve* possible combinations of these constituents to form simple salts, *five* have been excluded, the sulphates of sodium and potassium, and the bromides of these metals and calcium also, thus reducing the number to *seven*, the quantities of which can, of course, be definitely determined from the *seven* direct data of the analysis. Is it *certain*, however, on assured chemical grounds, that none of these excluded salts are contained in the water, and if not, on what principle has their possible existence been ignored?

I write, as is evident, with but very slight knowledge of chemical analysis; and possibly answers to my questions are to be found in some text-book. If so, I should be obliged by a reference to any easily accessible work in which the question is discussed. R. B. H.

Mental Arithmetic.

THE following method of multiplying large numbers together mentally, if new, may interest some of your readers. If it has been published before, I should be glad to learn where it may be found. The process is so simple that, though I have no special gift for mental arithmetic, I was able, almost without practice, to multiply together correctly seven figures by seven, and to write down the result from left to right.

Suppose it is desired to multiply 123 by 456. The sum is usually written thus:—

$$\begin{array}{r} 123 \\ 456 \\ \hline 738 \\ 615 \\ 492 \\ \hline 56088 \end{array}$$

If, instead of completing each step in the multiplication as we arrive at it, and carrying the tens to the left, the digits are merely connected by the multiplication sign and written down in their proper places, the result is:—

$$\begin{array}{llll} & & 1 \quad 2 \quad 3 \\ & & 4 \quad 5 \quad 6 \\ \hline & & 1\times6 \quad 2\times6 \quad 3\times6 \\ & 1\times5 & 2\times5 \quad 3\times5 & \\ 1\times4 & 2\times4 & 3\times4 & \end{array}$$

$$1\times4 \quad (1\times5+2\times4) \quad (1\times6+2\times5+3\times4) \quad (2\times6+3\times5) \quad 3\times6$$

If the figures in the lowest line are multiplied out and the tens carried to the left in the usual way, the result is, of course, the same as that given by the ordinary procedure. Thus, to obtain the first figure, beginning at the right, we say: "$3 \times 6 = 18$;—8 and carry 1." To obtain the second figure: "$2 \times 6 = 12$; $3 \times 5 = 15$; $12 + 15 + 1$ (which has been carried) $= 28$;—8 and carry 2." And so on. Thus each figure of the answer can be obtained by multiplying together certain digits of the multiplier and multiplicand, and adding the amount to be carried from the calculation of the previous figure, without the strain of remembering all the horizontal rows of results and their relative positions vertically. It remains only to show which digits of the multiplier and multiplicand must be combined. A consideration of the example worked out above will show that, to obtain the first figure of the answer, we multiply the 1st digit (from the right) of the multiplier (6) by the 1st of the multiplicand (3). To obtain the second, we multiply the 1st of the multiplier by the 2nd of the multiplicand and the 2nd of the multiplier by the 1st of the multiplicand (*i.e.* the first two of each line) crosswise, and add the products. Similarly, the third figure is obtained by multiplying the first three digits of each line crosswise, *i.e.* 1st by 3rd, 2nd by 2nd, and 3rd by 1st, and adding the products. The number of digits employed in the process is now at a maximum, and begins to diminish. To obtain the fourth figure, we multiply together crosswise all the digits except the first of each line, *i.e.* the group $\begin{smallmatrix}1&2\\4&5\end{smallmatrix}$. And to obtain the last figure, we multiply all except the first two of each line, *i.e.* the group $\begin{smallmatrix}1\\4\end{smallmatrix}$.

If the number of digits in the multiplier is less than that in the multiplicand, the procedure is the same till all the digits of the multiplier are used in the combination. For each successive figure, the group of digits in the multiplicand to be used shifts along one place to the left till it comes to the end. The group then diminishes as before, by dropping the right-hand digit in each line. For example: the groups, the digits of which are multiplied together crosswise, in multiplying 123456 by 789, are as follows:—

Digits—	8th.	7th.	6th.	5th.	4th.	3rd.	2nd.	1st.
	1	12	123	234	345	456	56	6
	7	78	789	789	789	789	89	9

It will be found, on trial, that this method is quite easy, and can be accomplished by anyone who can add together in his head the products of two digits, and can remember the string of figures which form the answer. This is most easily done by

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repeating the whole series from left to right, as each successive figure is arrived at.

The power of multiplying as many as seven figures by seven is not likely to be of much practical value, and when carried to this stage the method is merely a curiosity; but it may be of use in helping us to multiply together small amounts, consisting of two or three digits, which lie beyond the scope of the multiplication table.

I will conclude with a short example, showing what passes through the mind in working the method.

Multiply 987 by 654.

987
654

First figure.—4 × 7 = 28. Eight and carry two. Answer (so far) 8

Second figure.—4 × 8 = 32 + 2 (carried) = 34 + 5 × 7 (= 35) = 69. Nine and carry six. Answer (so far) 98

Third figure.—4 × 9 = 36 + 6 (carried) = 42 + 5 × 8 (= 40) = 82 + 6 × 7 (= 42) = 124. Four and carry twelve. Answer (so far) 498

Fourth figure.—5 × 9 = 45 + 12 (carried) = 57 + 6 × 8 (= 48) = 105. Five and carry ten. Answer (so far) 5498

Fifth and sixth figures.—6 × 9 = 54 + 10 (carried) = 64. Answer 645498

CLIVE CUTHBERTSON.

A Rare Phenomenon.

I HAVE read with much interest the accounts of "the rare phenomenon" observed by several of your correspondents published in NATURE, vol. xliv. pp. 494, 519), as I noticed a similar appearance here in Nova Scotia, at about the same time (September 11).

A narrow ray—apparently of auroral light—spanned the whole heavens from east to west, passing overhead a little to the south of the zenith. There was little or no display of auroral light in the north at the time.

A "harvest-home" was held here on September 11, and I noticed the appearance, I think, the same evening about 11 o'clock.

A number of persons in the town of Baddeck observed the same or a similar phenomenon "very shortly before September 12." ALEXANDER GRAHAM BELL.

Beinn Bhreagh, near Baddeck, Cape Breton, N.S., November 6.

HENRY NOTTIDGE MOSELEY, F.R.S.

I HAVE been asked to write for the readers of NATURE some account of my dear friend Moseley, who, after an illness which removed him from all active life and work for more than four years, died at Clevedon, in Somersetshire, on November 10. He was only forty-seven years of age; and when seized with the illness which necessitated his retirement from active life, was at the zenith of a wonderful career of scientific productiveness and value. He had for six years held the Linacre Professorship of Human and Comparative Anatomy in the University of Oxford; and by his great energy and commanding talent had succeeded in collecting around him a most promising band of younger men devoted to the investigation of embryological and morphological problems. Baldwin Spencer, Gilbert C. Bourne, S. J. Hickson, and G. Herbert Fowler, were his pupils, and have shown by their numerous published works the value of the teaching and impulse which he gave to them. In the early days of his illness (1887), he was cheered by receiving from the Royal Society the Royal Medal, in recognition of the value of his researches on Peripatus, the Hydrocorallinæ, the Land Planarians, and the Chitons. The blow caused by his serious illness was felt not only in the scientific and social life of Oxford, but in many other centres. We missed his valuable and practical help in carrying to completion the Plymouth Laboratory of the Marine Biological Association, of which he had been a most active and enthusiastic promoter; in the editorship of the *Quarterly Journal of Microscopical Science* I found myself once more deprived of the aid of my most valued comrade, as I had been but a few years previously when Frank Balfour died. The readers of NATURE and of the *Athenæum* missed his varied and always strongly-original contributions; and the Zoological, Anthropological, and Royal Societies had to regret his absence from their meetings and Councils. Moseley had, moreover, at this time made it a practice to give evening lectures in the larger provincial towns as well as in London: from all quarters came expressions of the deep regret which his retirement from public work excited. The amount and variety of work in which he engaged, in addition to the remarkable and extraordinarily minute course of lectures and laboratory work provided by him for his pupils, were certainly more than was wise for him to undertake. But it was a strange and to him a disastrous fact that he never felt tired. He was an exceedingly strong man, and I never saw him fatigued either by physical or mental exertion.

We made acquaintance in Rolleston's laboratory at the Oxford University Museum in 1866, and became fast friends and constant companions. Moseley's father was a distinguished mathematician and Canon of Bristol, Rector of Olvaston, near the Severn. Here, when I was staying with Moseley in 1871, we dissected and prepared the skeleton of a huge grampus which is now in the Oxford Museum; the carcass had made a tour of the neighbouring villages for three weeks before we obtained possession of it. Moseley was at school at Harrow, where he chiefly occupied himself in birds'-nesting and "bug-hunting," in conjunction with a small band of kindred spirits. He was essentially a sportsman, knew every kind of game and how to pursue it. He thoroughly disliked the ordinary routine of school work, such as it was in those days, and it was not until he had entered at Exeter College, and come under the teaching of the late Prof. Rolleston, that his really keen and remarkable intellectual powers began to show themselves. He had somehow developed in early youth the most deep-rooted scepticism which I ever came across among men of my own age; hence it was the *reality* of the work which he did in the dissecting-room at the Museum which delighted him and gave him confidence that there was "something in it" worthy of his intellectual effort. With unfeigned astonishment he would say, on dissecting out the nervous system of a mollusk or some such structure, "It is like the picture, after all!" He had a profound disbelief in the statements made in books unless he could verify them for himself, and it was this habit of mind, perceived and encouraged by Rolleston, which made him in after life so admirable an observer and so successful as a discoverer of new facts. Rolleston used to say that you had only to put down Moseley on a hill-side with a piece of string and an old nail, and in an hour or two he would have discovered some natural object of surpassing interest. He took great interest in all games, and was himself a first-rate racket-player. In the vacations he got a fair amount of shooting, and spent one "long" shooting and fishing in Norway. In the summer of 1867 we visited the Channel Islands together, for the purpose of studying marine animals. Whilst in Sark, after I had left him, he made the acquaintance of an American painter named Dix, and discovered himself to be no mean artist, bringing back a number of really clever water-colours, his first attempts in that direction. At this time and throughout his life, those who met him were struck by his singularly soft and agreeable voice, and by his great courtesy and power of interesting, I may even say fascinating, the most unpromising and unlikely of the companions amongst whom he chanced to find himself—I mean stiff old gentlemen and demure old ladies. To companions of his own age he

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was fond of adopting the free discourse and chaff of school-boy days. His friendship was like that of the explorers and prairie-hunters of whom he loved to read—absolutely staunch. If you had the good fortune to be his "chum," he would stand by you through thick and thin, and share all he had with you. I do not think there was any limit to what he would have done for his friend. We took our degrees together in 1868; and in the following spring—he having been elected Radcliffe Travelling Fellow, and I Burdett-Coutts Scholar—we spent six weeks in the Auvergne and the country between that and Marseilles. In the following winter (February 1870) we took up our quarters together at Vienna, and studied with Stricker, and in Rokitanski's laboratory. He entered, on our return, at University College, London, as a student of the Medical Faculty. In 1871, after his winter medical session, he joined me at Leipzig, where his great abilities were discerned and thoroughly appreciated by Prof. Ludwig, in whose laboratory we had the privilege of working. His first scientific memoirs were published whilst he was here—one, on the nerves of the cornea of mammals, as shown by the gold method (then not so familiar as it is now), and one on the circulation in the wing of the cockroach.

In the autumn of the same year, Moseley went, as member of the Government Eclipse Expedition, to Ceylon, under Mr. Norman Lockyer, whilst I joined Anton Dohrn at Naples. Moseley made valuable spectroscopic observations of the eclipse at Trincomali, and also brought home a large booty of Land Planarians, which he at once studied by means of sections, going to Oxford for the purpose of using the laboratory and the library attached to the Museum. This admirable piece of work delighted Rolleston, who communicated it to the Royal Society; it was published in the Philosophical Transactions after Moseley had sailed on the *Challenger*, as one of the naturalists of the Expedition, in December 1872. We did not see him again until May 1876, but I had frequent letters from him, and sometimes a small parcel, or some photographs. Of the scientific staff of the Expedition, Wyville Thomson and Suhm are dead, as well as Moseley; John Murray and J. Y. Buchanan are the two survivors. Moseley, although not a botanist, undertook the collecting of plants whenever the Expedition touched land; he also made important anthropological studies on the Admiralty Islanders, and has published a wonderful mass of notes and observations, accompanied by plates and woodcuts, in his "Notes of a Naturalist on the *Challenger*." He showed the stuff he was made of very soon after the Expedition started, viz. on the arrival of the *Challenger* at the Cape. He immediately started off in quest of Peripatus—a strange, imperfectly described beast which we had discussed together over some spirit specimens of it which I had received from Roland Trimen, of Cape Town. Moseley had made up his mind before he left England to "tackle" Peripatus, and he did so. He obtained living specimens, discovered the tracheæ and the most important features in the development, showing that the "jaws" are in-turned parapodia—and sent home a memoir which was at once published in the Philosophical Transactions. In the later part of the voyage he was occupied with the corals, and especially the Millepores and Stylasterids. The wonderfully elaborate plates, and the discovery they embodied, necessitating the formation of a new group of animals, the Hydrocorallinæ, were the first-fruits of his voyage which he produced on landing in 1876. During his absence both his father and his mother had died. His old College, Exeter—where I became a Fellow in the year of the *Challenger's* departure—now was inspired through the good offices of an eminent Greek scholar, with the happy thought of offering Moseley a Fellowship and a home in the College, so that he found on landing a welcome awaiting him, and a place in which to store for a while his treasures. I do not think that a College Fellowship was ever better bestowed: that was in the good old days before Lord Selborne's Commission. In his rooms in Exeter, Moseley displayed his Japanese and Melanesian curiosities, and wrote many papers embodying the observations made during his voyage, besides the book above mentioned. He was elected F.R.S. in 1879, and after a visit to Oregon (of which he published an account) was appointed (1879) Assistant Registrar of the University of London. He took up his residence in Burlington Gardens, but not for long. In 1881 he married the youngest daughter of Mr. J. Gwyn Jeffreys, F.R.S., the distinguished conchologist, and in the same year was elected, on the death of his teacher and close friend, Prof. Rolleston, to the Linacre Professorship in the University of Oxford.

Moseley had had no previous experience in teaching, but he set to work with that unbounded energy and strength which characterized him. He spared no pains to make his lectures absolutely up to date, and arranged a thorough laboratory course extending over two years to illustrate them. The regulations of the University as to examinations and curriculum were at that time not unfavourable to the study of animal morphology, and Moseley usually had ten or a dozen serious students besides the elementary class. Lincoln, University, and New Colleges encouraged his and their efforts by offering and awarding Fellowships to students of the University distinguished in animal morphology; and after six years all was progressing as satisfactorily as possible, when he was attacked by illness which brought his work to an end. Not only was he unable to carry on his work, but his absence naturally enough was unfavourable to the interests of those studies which he would have fostered and guarded, had he been able to take part in the legislation of the University.

During the happy and busy six years which Moseley spent as Linacre Professor at Oxford, he trained Bourne, Hickson, and Fowler to carry on his coral work; with Baldwin Spencer he investigated the pineal eye of Lacertilia, and himself published his remarkable discovery of eyes and other sense-organs in the shells of Chitonidæ. He was largely instrumental in securing the Pitt-Rivers collection of anthropological objects for the University, and superintended the preliminary arrangement of the collection in the building erected for it. He served twice on the Council of the Royal Society, was a founder and member of Council of the Marine Biological Association, and was President of Section D of the British Association at the Montreal meeting.

His love of travel was shared by his wife, who went with him from Montreal to Arizona to visit the town-building Indians of that remote region, and who, only a year before his illness, accompanied him on an Easter holiday trip to Tangier and Fez. During his illness she has been his constant companion. He leaves, besides her, two daughters and a son.

E. RAY LANKESTER.

ON THE VIRIAL OF A SYSTEM OF HARD COLLIDING BODIES.

A RECENT correspondence has led me to examine the manner in which various authors have treated the influence of the finite size of molecules in the virial equation, and I should like to lay a few remarks upon the subject before the readers of NATURE.

To fix the ideas, we may begin by supposing that the molecules are equal hard elastic spheres, which exert no force upon one another except at the instant of collision. By calling the molecules hard, it is implied that the collisions are instantaneous, and it follows that at any moment the potential energy of the system is negligible in comparison with the kinetic energy.

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If the volume of the molecules be very small in comparison with the space they occupy, the virial of the impulsive forces may be neglected, and the equation may be written

$$pv = \tfrac{1}{3}\Sigma m V^2, \quad \ldots\ldots\ldots \quad (1$$

where p is the pressure exerted upon the walls of the inclosure, v the volume, m the mass, and V the velocity of a molecule.

In his essay of 1873 Van der Waals took approximate account of the finite size of the molecules, using a peculiar process to which exception has been taken by Maxwell and other subsequent writers. It must be said, however, that this method has not been proved to be illegitimate, and that at any rate it led Van der Waals to the correct conclusion—

$$p(v - b) = \tfrac{1}{3}\Sigma m V^2, \quad \ldots\ldots\ldots \quad (2)$$

in which b denotes four times the total volume of the spheres. In calling (2) correct, I have regard to its character as an approximation, which was sufficiently indicated by Van der Waals in the original investigation, though perhaps a little overlooked in some of the applications.

In his (upon the whole highly appreciative) review of Van der Waals's essay, Maxwell (NATURE, vol. x. p. 477, 1874) comments unfavourably upon the above equation, remarking that in the virial equation v is the volume of the vessel and is not subject to correction.[1] "The effect of the repulsion of the molecules causing them to act like elastic spheres is therefore to be found by calculating the virial of this repulsion." As the result of the calculation he gives

$$pv = \tfrac{1}{3}\Sigma m V^2\left\{1 - 2\log\left(1 - 8\frac{\rho}{\sigma} + \frac{17\rho^2}{\sigma^2} - \ldots\right)\right\}, \quad (3)$$

where σ is the density of the molecules, and ρ the mean density of the medium, so that $\rho/\sigma = b/4v$. If we expand the logarithm in (3), we obtain as the approximate expression, when ρ/σ is small,

$$pv = \tfrac{1}{3}\Sigma m V^2(1 + 4b/v), \quad \ldots\ldots \quad (4)$$

or, as equally approximate,

$$p(v - 4b) = \tfrac{1}{3}\Sigma m V^2, \quad \ldots\ldots\ldots \quad (5)$$

which does *not* agree with (2).

The details of the calculation of (3) have not been published, but there can be no doubt that the equation itself is erroneous. In his paper of 1881 (*Wied. Ann.*, xii. p. 127), Lorentz, adopting Maxwell's suggestion, investigated afresh the virial of the impulsive forces, and arrived at a conclusion which, to the order of approximation in question, is identical with (2). A like result has been obtained by Prof. Tait (Edin. Trans., xxxiii. p. 90, 1886).

It appears that, while the method has been improved, no one has succeeded in carrying the approximation beyond the point already attained by Van der Waals in 1873. But a suggestion of great importance is contained in Maxwell's equation (3), numerically erroneous though it certainly is. For, apart from all details, it is there implied that the virial of the impacts is represented by $\frac{1}{3}\Sigma m V^2$, multiplied by some function of ρ/σ, so that, if the volume be maintained constant, the pressure as a function of V is proportional to $\Sigma m V^2$. The truth of this proposition is evident, because we may suppose the velocities of all the spheres altered in any constant ratio, without altering the motion in any respect except the scale of time, and then the pressure will necessarily be altered in the square of that ratio.

It will be interesting to inquire how far this conclusion is limited to the suppositions laid down at the commencement. It is necessary that the collisions be instantaneous, in relation, of course, to the free time. Otherwise, the similarity of the motion could not be preserved, the duration of a collision, for example, bearing a variable ratio to the free time. On the same ground, vibrations within a molecule are not admissible. On the other hand, the limitation to the spherical form is unnecessary, and the theorem remains true whatever be the shape of the colliding bodies. Again, it is not necessary that the shapes and sizes of the bodies be the same, so that application may be made to mixtures.

In the theory of gases, $\Sigma m V^2$ is proportional to the absolute temperature; and whatever doubts may be felt in the general theory can scarcely apply here, where the potential energy does not come into question. So far, then, as a gas may be compared to our colliding bodies, the relation between pressure, volume, and temperature is

$$p = T\phi(v), \quad \ldots\ldots\ldots \quad (6)$$

where $\phi(v)$ is some function of the volume. When v is large, the first approximation to the form of ϕ is

$$\phi(v) = \frac{A}{v}.$$

In the case of spheres, the second approximation is

$$\phi(v) = \frac{A}{v} + \frac{Ab}{v^2},$$

where b is four times the volume of the spheres.

Thus far we have supposed that there are no forces between the bodies but the impulses on collision. Many and various phenomena require us to attribute to actual molecules an attractive force operative to much greater distances than the forces of collision, and the simplest supposition is a cohesive force such as was imagined by Young and Laplace to explain capillarity. We are thus led to examine the effect of forces whose range, though small in comparison with the dimensions of sensible bodies, is large in comparison with molecular distances. In the extreme case, the influence of the discontinuous distribution of the attractive centres disappears, and the problem may be treated by the methods of Laplace. The modification then required in the virial equation is simply to add[1] to p a term inversely proportional to v^2 as was proved by Van der Waals; so that (6) becomes

$$p = T\phi(v) - av^{-2}. \quad \ldots\ldots\ldots \quad (7)$$

According to (7), the relation between pressure and temperature is *linear*—a law verified by comparison with observations by Van der Waals, and more recently and extensively by Ramsay and Young. It is not probable, however, that it is more than an approximation. To such cases as the behaviour of water in the neighbourhood of the freezing-point it is obviously inapplicable.

In their discussions, Ramsay and Young employ the more general form—

$$p = T\phi(v) + \chi(v); \quad \ldots\ldots\ldots \quad (8)$$

and the question arises, whether we can specify any generalization of the theoretical conditions which shall correspond to the substitution of $\chi(v)$ for av^{-2}. It would seem that, as long as the only forces in operation are of the kinds, impulsive and cohesive, above defined, the result is expressed by (7); and that if we attempt to include forces of an intermediate character, such as may very probably exist in real liquids, and must certainly exist in solids, we travel beyond the field of (8), as well as of (7). It may be remarked that the equation suggested by Clausius, as an improvement on that of Van der Waals, is not included in (8).

[1] In connection with this it may be worth notice that for motion *in one dimension* the form (2) is *exact*.

[1] It thus appears that, contrary to the assertion of Maxwell, p *is* subject to correction. It is pretty clear that he had in view an attraction of much smaller range than that considered by Van der Waals.

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Returning to the suppositions upon which (7) was founded, we see that, if the bodies be all of one *shape*, *e.g.* spherical, the formula contains only two constants—one determining the size of the bodies, and the second the intensity of the cohesive force; for the mean kinetic energy is supposed to represent the temperature in all cases. From this follows the theorem of Van der Waals respecting the identity of the equation for various substances, provided pressure, temperature, and volume be expressed as fractions of the critical pressure, temperature, and volume respectively. If, however, the *shape* of the bodies vary in different cases, no such conclusion can be drawn, except as a rough approximation applicable to large volumes. RAYLEIGH.

Terling Place, Witham, November 18.

THE IMPLICATIONS OF SCIENCE.[1]

II.

I MIGHT now at once return to further consider those implications of science to which I have called your attention, but I think it will be better to first briefly pass two important matters in review.

The first concerns our means of investigation as to such fundamental questions.

The second relates to our ultimate grounds for forming judgments about them. We have to consider how fundamental truth can be acquired and tested.

Evidently the only means of which we can make use are our *thoughts*, our reason, our intellectual activity. "Thoughts" may be, and should be, carefully examined and criticized; but however much we may do so, and whatever the results we arrive at, such results can only be reached by thoughts, and must be expressed by the aid of our thoughts. This will probably seem such a *manifest* truism that I shall be thought to have committed an absurdity in enunciating it. To suppose that by any reasoning we can come to understand what we can never think, may seem an utterly incredible folly; yet at a meeting of a Metaphysical Society, in London, a speaker, not long ago, expressly declared "thought" to be a misleading term, the use of which should be avoided.

Now I am far from denying that unconscious activities of various different orders take place in our being, yet whatever influence such activities may have they cannot affect our judgments save by and in thoughts.

If a man is convinced that thoughts are worthless tools, he can only have arrived at that conclusion by using the very tools he declares to be worthless. What, then, ought his conclusion to be worth even in his own eyes?

It is simply impossible by reason to get behind or beyond conscious thought, and our thoughts are and must be our only means of investigating problems however fundamental.

Even in investigating the properties of material bodies, it is to self-conscious reflective thought that our final appeal must be made.

For it is to our thoughts, and not to our senses only, that our ultimate appeal must be made, even with respect to the most material physical science matters.

Some persons may imagine that with respect to investigations about the properties of material bodies, it is to our sensations alone that we must ultimately appeal. But it is not so; anyone would be mad to question the extreme importance, the absolute necessity, of our sensations in such a case. Nevertheless, after we have made all the observations and experiments we can, how can we know we have obtained such results as we may have obtained, save by our self-conscious thought? By what other means are we to judge between what may seem to be the conflicting indications of different sense impressions?

Our senses are truly tests and causes of certainty, but not *the* test. Certainty belongs to thought, and self-conscious reflective thought is our ultimate, absolute criterion.

As to the ultimate grounds on which our judgments respecting such problems must repose, as Mr. Arthur Balfour has forcibly pointed out, that it is a question altogether distinct from that of the origin of our judgments, or from reasonings about their truth. Such matters are very interesting, but they are not here in point, since it is plain that no proposition capable of proof can be one the certainty of which is fundamental. For, in order to prove anything by reasoning, we must show that it necessarily follows as a consequence from other truths, which therefore must be deemed more indisputable. But the process must stop somewhere. We cannot prove everything. However long our arguments may be, we must at last come to ultimate statements, which must be taken for granted, like the validity of the process of reasoning itself, which is one of the implications of science. If we had to prove either the validity of that process or such ultimate statements, then either he must argue in a circle, or our process of proof must go on for ever without coming to a conclusion, which means there could be no such thing as "proof" at all.

Therefore the "grounds of certainty" which any fundamental proposition may possess cannot be anything *external* to it—which would imply this impossible proof. The only ground of certainty which an ultimate judgment can possess is its own *self-evidence*—its own manifest certainty *in and by itself*. All proof, all reasoning, must ultimately rest upon truths which carry with them their own evidence, and do not therefore need proof.

It is possible that some of my hearers may be startled at the suggestion of believing anything whatever on "its own evidence," fancying it is equivalent to a suggestion that they should believe anything *blindly*. This, I think, is due to the following fact of mental association. The immensely greater part of our knowledge is gained by us indirectly—by inference or testimony of some kind.

We commonly ask for some proof with regard to any new and remarkable statement, and no truths are brought more forcibly home to our minds than are those demonstrated by Euclid. Thus it is that many persons have acquired a feeling that to believe anything which cannot be proved, is to believe *blindly*. Hence arises the tendency to distrust what is above and beyond proof. We are apt to forget, what on reflection is manifest—namely, that if it is not blind credulity to believe what is evident to us by means of something else, it must be still less blind to believe that which is directly evident in and by itself.

And self-conscious reflective thought tells *me* clearly, that the law of contradiction is not only implied by all science, and necessary to the validity of all science, but that it is, as I said, an absolute, necessary truth which carries with it its own evidence. It must be a truth, then, applicable both to the deepest abyss of past time and the most distant region of space. But here, again, I think it possible that one or two of my hearers may be startled, and perhaps doubting how things in this respect may be in the Dog-star now, or how they were before the origin of the solar system. I fancy I hear someone asking: "How is it possible that we, mere insects, as it were, of a day, inhabiting an obscure corner of the universe, can know that anything is and must be true for all ages and every possible region of space?"

In the first place, I think the difficulty which may be thus felt is due to the abstract form of the law of contradiction. And yet, as I said before, it is but the summing up of all the particular instances, as to each one of which no difficulty at all is felt, but each is clearly

[1] Friday Evening Discourse delivered at the Royal Institution by Dr. St. George Mivart, on June 5, 1891. Continued from p. 62.

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seen to be true. Any man who really doubted whether, if his legs were cut off, they might not at the same time remain on, would have a mind in a diseased condition.

There is, however, another reason which indisposes some persons to see the necessary force of this law. It is due, I think, to a second fact of mental association.

Things which are very distant, or which happened a long time ago, are known to us only in roundabout ways, and we often feel more or less want of certainty about them. On the other hand, we have a practical certainty concerning the things which are about us at any given moment. Thus we have come to associate a feeling of uncertainty with statements about things very remote. But nothing can well be more remote from us than "the most distant regions of space" or "before the origin of the solar system." It is not surprising, then, that this mental association should call forth a feeling of uncertainty with respect to any statement about universal truth.

It is, no doubt, wonderful that we should be able to know any necessary and universal truths; but it is less exceptionally wonderful, when we come to think the matter all round, than it may at first sight appear to be. It is wonderful; but so, deeply considered, is all our knowledge. It is wonderful that through molecular vibrations, or other occult powers of bodies, we have sensations—such as of musical tones, sweetness, blueness, or what not. It is wonderful that through sensations, actual and remembered, we have perceptions. It is wonderful that on the occurrence of certain perceptions we recognize our own existence past and present. So, also, it is wonderful that we recognize that what we know "*is*," cannot at the same time "not be." The fact is so, and we perceive it to be so; we know things, and we know that we know them. HOW we know them is a mystery, indeed, but one about which it is, I think, perfectly idle to speculate. It is precisely parallel to the mystery of sensation. We feel things savoury, or odorous, or brilliant, or melodious, as the case may be, and with the aid of the scalpel and the microscope we may investigate the material conditions of such sensations. But *how* such conditions can give rise to the feelings themselves is a mystery which defies our utmost efforts to penetrate. I make no pretension to be able to throw any light upon the problem "How is knowledge possible?" any more than on the problem "How is sensation possible?" or on the questions "How is life possible?" or "How is extension possible." But "*Ignorantia modi non tollit certitudinem facti.*" And we know that we are living, that we feel, and that we do know something—if only that we know we doubt about the certainty of our knowledge.

And *à propos* of such doubt, let me here put before you the intellectual penalties which have to be paid for any *real* and *serious* doubt with respect to the implications of science. I think we shall see that nothing less than intellectual suicide or mental paralysis must be the result. And such a result must also be logically fatal to every branch of science. The first implication I put before you was the *validity of inference*.

Now, no one who argues, or who listens to or reads—with any serious intention—the arguments of others, can, without stultifying himself, profess to think that no process of reasoning is valid. If the truth of no mode of reasoning is certain, if we can make no certain inferences at all, then all arguments must be useless, and to proffer, or to consider, them must be alike vain. But not only must all reasoning addressed to others be thus vain, the silent reasoning of solitary discursive thought must be vain also. Yet what does this amount to save an utter paralysis of the intellect? It is scepticism run mad.

But the implication I regard as one of the most important of all is the implication of *our knowledge of our own continuous existence*, concerning which I said I must crave your permission to speak at some length. It was the mention of this implication which led me to refer to that system of thought it is my object here to controvert.

I have heard it proclaimed in this theatre by Prof. Huxley that we cannot have supreme certainty as to our own continuous existence, and that such knowledge is but secondary and subordinate to our knowledge of our present feelings or "states of consciousness."

Of course I am not thus accusing him of *originating* any such erroneous view. In that matter he is but a follower of that daring and playful philosopher Hume. I say "playful," because I cannot myself think that he really believed his own negations. He seems to me too acute a man to have been himself their dupe. But however this may be, I here venture directly to contradict Hume's and Prof. Huxley's affirmation, which is also adopted by Mr. Herbert Spencer, and to affirm that we have the *highest* certainty as to our own continuous existence.

It is, of course, quite true that we have complete certainty about our present feelings, as also that we cannot know ourselves apart from our feelings. But it is no less true that we cannot be conscious of feelings apart from the "self" which has those feelings. Now, it is assumed by those I oppose that we can know nothing with absolute certainty unless we know it by itself or "unmodified," or as existing "*absolutely*." But in fact nothing, so far as we know, exists apart from every other entity and unmodified—or "*absolutely*," as it is, in my opinion, absurdly called. No wonder, then, if we do not know things in a way in which they never do, and probably never can, exist. We can really know nothing by itself because nothing exists by itself. It is not wonderful, then, if we only know ourselves as related to our simultaneously known feelings, or *vice versâ*.

It is quite true that we never know our own substantial essential being alone and unmodified, but then we have never for an instant so existed. Our knowledge of ourselves in this respect is like our knowledge of anybody and everybody else. Most persons here present doubtless know Prof. Tyndall, yet they never knew him, no one ever knew him, except in some "state"—either at home or away from home, either sitting or not sitting, either in motion or at rest, either with his head covered or uncovered—and this for the very good and obvious reason that he never did or could exist for a moment save in some "state." But this does not prevent your knowing him very well, and the same consideration applies to our knowledge of ourselves. When I consider what is my primary, direct consciousness at any moment, I find it to be neither a consciousness of a "state of feeling" nor of my "continuous existence," but a consciousness of doing something or having something done to me—action or reaction. I have always, indeed, some "feeling" and also some sense of my "self-existence"; but what I perceive primarily, directly, and immediately is neither the "feeling" nor the "self-existence," but some concrete actual doing, being, or suffering then experienced. We can, indeed, become distinctly and explicitly aware of either the "feeling" or the "self-existence" by turning back the mind upon itself. But to know that one "has a feeling" or is in a "state," or even that a "feeling exists," is plainly an act by which no one begins to think. It is evidently a secondary act—an act of reflection. No one begins by perceiving his perception a bit more than he begins by expressly adverting to the fact that it is he himself who perceives it.

Let us suppose two men to be engaged in a fencing match. Each man, while he is parrying, lunging, &c., has his "feelings" or "states," and knows that it is "he" who is carrying on the struggle. Yet it is neither his "*mental states*" nor the "*persistence of his being*" which he directly regards, but his concrete activity—what he is doing and what is being done to him. He may, of course, if he chooses, direct his attention either to the feelings

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he is experiencing or to his underlying continuous personality. Should he do so, however, a hit from his adversary's foil will be a probable result.

But to become aware that one has any definite feeling is a reflex act *at least* as secondary and posterior as it is to become aware of the "self" which has the feeling. I say "*at least*," but I believe that of the two perceptions (1) of "feelings," and (2) of "self," it is the "self" which is the *more* prominently given in our primary, direct cognitions.

I believe that a more laborious act of mental digging is requisite to bring *explicitly* to light the *implicit* mental state, than to bring forward *explicitly* the *implicit* "self-existence." Men continually and promptly advert to the fact that actions and sufferings are *their own*, but do not by any means so continually and promptly advert to the fact that the feelings they experience are "*existing feelings*."

Therefore I am convinced that one of the greatest and most fundamental errors of our day is the mistake of supposing that we can know our "mental states" or "feelings," more certainly and directly than we can know the continuously existing self which has those feelings.

Our perception of our continuous existence also involves the *validity of our faculty of memory*, which is implied in this way, as well as in every scientific experiment we may perform. For we cannot obviously have a reflex perception either of our "feelings" or our "self-existence," without trusting our memory as to the past; since, however rapid our mental processes may be, no mental act takes place without occupying some period of time, and, indeed, nervous action is not extremely rapid. In knowing, therefore, such facts by a reflex act, we know by memory what is already past. Thus our certainty as to our own continuous existence necessarily carries with it a certainty as to our faculty of memory. Therefore, the mental idiocy of absolute scepticism is the penalty that has to be paid for any *real* doubt about our own existence or the trustworthiness of the *faculty of memory*, for all our power of reposing confidence in our observations, experiments, or reasonings, would, in that case, be logically at an end. On the other hand, the validity of our faculty of memory establishes once for all (as we have seen) the fact that we can transcend our present consciousness and know real *objective truth*.

Let us now see the consequences of the denial, or *real* doubt of the second implication of science—the "law of contradiction." Without it we can be certain of nothing, and it therefore lands us in absolute scepticism. And if we would rise from that intellectual paralysis we must accept that dictum as it presents itself to our minds; and the dictum presents itself to my mind, not as a law of *thought* only, but a law of *things*. It affirms, for example, that no creature anywhere or anywhen can at the same time be both bisected and entire.

An amusing instance of the way in which very distinguished men may be misled as to the question of our power of perceiving necessary truth is offered by an imaginary case which has been put forward by Prof. Clifford and Prof. Helmholtz. Their object in advancing it was to show, by an example, how truths which appear necessary to us are not objectively necessary. But the result appears to me to show the direct contradictory of what they intended. Their intention evidently was to support the proposition that we can know "*no truths to be absolutely necessary*," and the result is to show that, even according to them, "*some truths are absolutely necessary*." The necessary truths they propose to controvert are that "a straight line is the shortest line between two points," and that "two straight lines cannot inclose a space."

For this purpose, curious creatures, possessing length and breadth but no thickness, were supposed, by them, to be living on a sphere with the surface of which their bodies would coincide. They were imagined to have experience of length and breadth in curves, but none of height and depth, or of any straight lines. To such creatures, it was said, our geometrical necessary truths would not appear "truths" at all. A straight line for them would not be the shortest line, while two parallel lines prolonged would inclose a space.

To this imaginary objection I reply as follows:—"Beings so extraordinarily defective might, likely enough, be unable to perceive geometrical truths which to less defective creatures—such as ourselves—are perfectly clear. Nevertheless, *if they could conceive of such things* at all, as those we denote by the terms 'straight lines' and 'parallel lines,' then there is nothing to show that they could not also perceive those same necessary truths concerning them which are evident to us."

It is strange that the very men who make this fanciful objection, actually show, by the way they make it, that they themselves perceive the necessary truth of those geometrical relations the necessity of which they verbally deny. For how, otherwise, could they affirm what would or would not be the necessary results attending such imaginary conditions? How could they confidently declare what perceptions such conditions would certainly produce, unless they were themselves convinced of the validity of the laws regulating the experiences of such beings? If they affirm, as they do, that they perceive what must be the truth in their supposed case, they thereby implicitly assert the existence of some absolutely necessary truths, or else their own argument itself falls to the ground.

But this same implication of science, respecting the objective absolute validity of the law of contradiction, also refutes that popular system of philosophy which declares that all our knowledge is merely relative, and that we can know nothing as it really exists independently of our knowledge of it, the system which proclaims the "*relativity of knowledge*."

Of course anything which is "*known to us*" cannot at the same time be "*unknown to us*," and so far as this, our knowledge may be said to affect the things we know. But this is trivial. Our "knowing" or "not-knowing" any object is—apart from some act of ours which results from our knowledge—a mere accident of that body's existence, which is not otherwise affected thereby.

Again, as I before remarked, nothing, so far as we know, exists by itself, and unrelated to any other thing. To say, therefore, that "all our knowledge is relative" might only mean that knowledge concords with objective reality. But this is by no means what the upholders of the "relativity of knowledge" intend to signify. They deny the objective validity, the actual correspondence with reality, of any of our perceptions or convictions—even, as Mr. Herbert Spencer tells us, our cognition of "difference."

Every system of knowledge, *however*, must start with the assumption, implied or expressed, that something is true. By the teachers of the doctrine of the "relativity of knowledge" it is evidently taught that the doctrine of the relativity of knowledge is true. But if we cannot know that anything corresponds with external reality, if *nothing* we can assert has more than a relative or phenomenal value, then this character must also appertain to the doctrine of the "relativity of knowledge." Either this system of philosophy is merely relative or phenomenal, and cannot be known to be true, or else it is absolutely true, and can be known so to be. But it must be merely relative and phenomenal, if everything known by man is such. Its value, then, can be only relative and phenomenal, therefore it cannot be known to correspond with external reality, and cannot be asserted to be true; and anybody who asserts that we can know it to be true,

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thereby asserts that it is false to say that our knowledge is only relative. In that case some of our knowledge must be absolute; but this upsets the foundation of the whole system. Anyone who upholds such a system as this may be compared to a man seated high up on the branch of a tree which he is engaged in sawing across where it springs from the tree's trunk. The position taken up by such a man would hardly be deemed the expression of an exceptional amount of wisdom.

My time has expired, and I may say no more. The considerations I have put before you this evening, should they commend themselves to your judgment, will, I think, lead you to admit that, if we feel confidence and certainty in *any* part of *any* branch of physical science, we thereby implicitly affirm that the human mind can, by consciousness and memory, know more than phenomena—can know some objective reality—can know its own continuous existence—the validity of inference and the certainty of universal and necessary truth as exemplified in the law of contradiction. In other words, the system of the relativity of knowledge is untrue. Thus the dignity of that noble, wonderful power, the human intellect, is fully established, and the whole of our reason, "from turret to foundation-stone," stands firmly and secure. If I have succeeded in bringing this great truth home to one or two of my hearers who before doubted it, I am abundantly repaid for the task I have undertaken. It only remains for me now to thank you for the kind and patient hearing you have been so good as to accord me.

EXAMINATIONS IN SCIENCE.

THE Committee of the Privy Council on Education have just announced an important decision with regard to the examinations of the Science and Art Department in science.

The number of candidates presenting themselves for examination in science is already so large—about 190,000 papers in various branches of science were worked at the examination in May last, besides above 14,000 practical examinations—that the machinery of examination and registration is already severely strained. These numbers will in all probability soon be so increased as to render it impossible to make satisfactory arrangements for the examination of the candidates at the local centres, or for the examination of the worked papers under any system of central examination.

At the same time the means recently placed at the disposal of local authorities for providing or aiding instruction seem to render it unnecessary for the Science and Art Department to continue to give direct aid for very elementary instruction in science. Such instruction can now be more effectually organized and maintained locally.

Under these circumstances it has been decided that after the May examinations of 1892 the payments of £1 now made for the second class in the elementary stage of each science subject shall cease.[1] An elementary paper will continue to be set in each subject, but the results will be recorded simply as *pass* or *fail*, the standard for passing being about the same as that now required for a first class, *i.e.* about 60 per cent. of the marks obtainable.

At the same time, with a view to encourage more advanced instruction, which does not seem to be adequately provided for at present, the payments for the advanced stage and for honours will be considerably increased. The payments on results will then be £2 for a pass in the elementary stage; £5 and £2 10*s.* for a first or second class respectively in the advanced stage; and £8 and £4 for a first or second class respectively in honours, in each subject of science, and in each subdivision of subject 6, theoretical mechanics, or of subject 8, sound, heat, and light, with the following exceptions:—The payments for practical chemistry will be £3 for a pass in the elementary stage, and £6 and £3 10*s.* respectively for a first or second class in the advanced stage; the payments for mathematics will be £2 for a pass in stage 1, £3 and £2 respectively for a first or second class in stages 2 and 4, £4 and £3 for a first or second class respectively in stage 3, £5 and £4 for a first or second class respectively in stages 5, 6, and 7, and £8 and £4 respectively for a first or second class in honours. The payment for section 1 (geometrical drawing) of subject 1 will remain as at present, 10*s.*

The payment for attendance in an organized science school will be increased to £1 in the day school and 10*s.* in the night school.

As it is of great importance to prevent large numbers of wholly unqualified candidates being presented at the examinations, the examiners will be instructed to note the papers of all such as would not obtain above twenty-five per cent. of the marks, and a deduction will be made from the grant to each school for each such paper sufficient to cover the cost incidental to its examination.

The committee of a science school in a place in Great Britain with less than 5000 inhabitants which does not receive aid from the local authority, or of any science school in Ireland, will be allowed to continue until further notice on the present system, if they so desire it.

[1] The payments on the results of the examinations in 1892 will not be affected by this Minute.

NOTES.

THE subject of an International Congress of Electricity, to be held at Chicago in connection with the World's Fair, continues to attract much attention in America. A report about the matter has been presented to the Director-General of the Exhibition by Mr. J. Allen Hornsby, secretary of the department of electricity. During a recent visit to Europe, Mr. Hornsby discussed the question with several leading men of science in England and on the Continent, and he was encouraged by them to believe that, if certain conditions were complied with, the success of the Congress would be certain. They all agreed that the Congress should be held under the auspices of the U.S. Government. Invitations, they thought, should be issued by the Government to individual scientific men through the Governments of the countries to which the individuals belong. "This course of action," says Mr. Hornsby, "in the opinion of the authorities whom I consulted, will insure an official character to the proceedings of the scientific Congress, and will virtually pledge the various Governments to a recognition and adoption of the standards created."

PROF. JOSEPH WOLSTENHOLME, whose name was well known to mathematicians, died on November 18 in his sixty-third year. He graduated at Cambridge as third wrangler in the Mathematical Tripos of 1850, and became a Fellow first of St. John's College, then of Christ's, where he was for many years a member of the tutorial staff. After vacating his Fellowship by marriage in 1869, he was appointed the first Professor of Mathematics in the Engineering College at Cooper's Hill—a position from which failing health compelled him to withdraw a year or two ago. With the Rev. Percival Frost, he wrote a treatise on solid geometry, published in 1863. He also collected many original mathematical problems, devised by himself, in a volume which appeared in 1867, and again in 1878.

WE regret to announce the death of Mr. S. F. Downing Principal of the Civil Engineering College, Seebpur, Calcutta, which took place at Coonoor, Madras, on October 16 last, at the comparatively early age of forty-seven. The *Englishman* of October 24 says:—"The deceased gentleman was educated at Trinity College, Dublin, and was a graduate of Dublin

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University in Arts and Engineering. He came out to India in 1869 as Professor of Civil Engineering in the Engineering Department of the Presidency College, Calcutta, and when that Department was amalgamated in 1880 with the Dehree Training School, and transferred to Seebpur with the title of Government Engineering College, Mr. Downing was chosen as first Principal of the new College. In no College in Bengal has so strict a system of discipline been introduced. The beneficial results of that system, consistently adhered to in the face of strong native opposition, have long been apparent; and the present flourishing condition of the College affords the best monument which could be erected to the indomitable perseverance and uniform justice of the administration of its late Principal."

THE death of Mr. Thomas Wharton Jones, F.R.S., is announced. He was nearly eighty years of age. Prof. Huxley, who was one of his pupils forty years ago, gives in the *British Medical Journal* a bright and pleasant account of his intercourse with his "old master."

THE third series of Hooker's "Icones Plantarum" (vols. xi.–xx. of the whole work) is now complete, and the Bentham Trustees, who are continuing the work under the editorship of Prof. D. Oliver, are offering a limited number of sets of this series of ten volumes, for £5 the set. It contains figures of a thousand new plants, including the most interesting discoveries of the last thirty years, and the most striking of the new genera described by Bentham and Hooker during the progress of their "Genera Plantarum." As the whole impression consists of only 250 copies, the work will soon become unpurchasable. Thanks to the provision made by the late Mr. Bentham, the trustees are issuing a fourth series at the rate of one volume, of 100 plates, annually, at the very low price of 16*s*.. Persons wishing to secure a copy of the third series should apply at once to Dulau and Co., 37 Soho Square, W.

THE external part of the laboratory which is being built in the Paris Museum of Natural History for Prof. Chauveau, from the designs provided by him, is now being finished. This laboratory will be used only for original research in physiology and bacteriology, and when completed will be the finest laboratory in France. But the Museum is deeply in debt, and this may cause some delay.

MEMBERS of the Royal Microscopical Society, and the several London and provincial Societies of a kindred nature, have been invited to subscribe to a fund for the benefit of the family of the late Mr. John Mayall. An influential Committee has been formed to secure the success of the scheme. Communications should be addressed to Mr. T. Curties, treasurer to the Committee, 244 High Holborn, W.C. The Committee has issued a circular setting forth Mr. Mayall's great services to the science of microscopy.

ACCORDING to a telegram despatched to the *Standard* from Bangkok on Monday night, Chaiya and Bandon, towns situated on the coast of the Gulf of Siam, have been practically destroyed by a cyclone. The loss of life is estimated at three hundred.

SOME details of the earthquake which caused so much havoc in Japan at the end of October have been received. A large part of the Empire was affected, the shocks being strongly felt in no fewer than thirty-one provinces. In the provinces of Ezozi, Mino, and Owari, several towns and villages were ruined, 3400 persons being killed and 43,000 houses destroyed. An up train and a down train on the Tokaido Railway were just meeting at the station of Gifu when the first shock was felt there. It was accompanied by subterraneous rumblings and violent oscillation, which put the passengers of the train into a great state of alarm. They were further terrified by seeing cracks in the earth, two or three feet wide, opening and closing in all directions, some of which threw up volcanic mud and ashes. A number of the passengers alighted and made their way into the town. Many houses had already fallen, and immense heaps of ruins were visible on every side. Other buildings which were then standing were so severely shattered that further earth-tremors which followed threw them to the ground. There was a marked subsidence of the earth for a considerable area round Gifu. Very soon after the houses collapsed, and while hundreds of persons remained buried under the ruins, flames burst out and spread with such rapidity that the citizens were compelled to desist from the work of rescue. The fire was not subdued until the next morning, when it was found that almost the whole town had been destroyed. The potteries in the prefectures of Owari and Mino, and at Seto and other towns, were reduced to ruins. At Gobo, a temple belonging to the Shin sect of the Buddhists, which was crowded with persons, suddenly collapsed, burying fifty of the worshippers. A slight shock occurred at Nagerio on the night of October 25. On the following Wednesday morning, while forty Christians were assembled in the Methodist school, the building began to totter, and the worshippers fled, several being killed or fatally injured. Many streets were blocked with fallen houses, and others were rendered all but impassable by the crowds of panic-stricken people who were endeavouring to make their escape. Hundreds of persons were killed by the collapse of a thread factory, and a large brick building. A castle four hundred years old, however, remained intact and suffered no damage. It is estimated that in the three towns comprising the city of Nagoya from 750 to 1000 persons lost their lives. From the time of the first disturbance up to the morning of October 30, no fewer than 368 distinct shocks were reported. Fissures 2 feet wide and several feet deep appeared in the earth, while railway metals were twisted, iron bridges broken, river embankments engulfed or destroyed, and fields flooded. A lake 600 yards long and 60 wide was formed at the foot of the Hukusan Mountain in the Gifu prefecture, and great cracks were formed in the ground near the hills. Water sprang from the cracks, and that in the wells was changed to a brownish tint and rendered unfit for drinking. The embankments of most of the rivers were destroyed, and in the Gifu district it will be necessary to rebuild them for a distance of 350 miles. The general appearance of the Mizushima division of the Mortosu district underwent a complete transformation, and at Nogo in one district there was a marked subsidence of the earth. Of 700 temples in the Gifu prefecture, over one-third were destroyed, and it will take many months to repair the river embankments. In some parts of the town of Gifu boiling mud spouted from the fissures for over two hours. The top of the sacred mountain of Fusiyama was rent asunder, a chasm being formed 1200 feet wide and 600 feet deep.

IN a special report to the Secretary of Agriculture, Mr. Mark W. Harrington, Chief of the U.S. Weather Bureau, has presented a general summary of the operations of the Weather Bureau during the three months which followed its transfer to the Department of Agriculture on July 1, 1891. The Service has been reorganized with a view of carrying out the expressed intention of Congress to develop and extend its work with special reference to agriculture. The office force in Washington has been formed into three principal divisions, called respectively the Executive Division, the Records Division, and the Weather-Crop Bulletin and State Weather Service Division. Outside of Washington, local forecast officials have been appointed, the person chosen being in every case selected from the most experienced and competent observers of the Service These officials have been placed in the larger cities, with authority to make predictions for their stations and vicinity, giving the weather more in detail than the Washington fore-

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casts. They are instructed to make a careful study of the climatology of their respective sections, both for their own use as an aid in predicting and for publication for the information of the public; and they are directed to give particular attention to the effect of the weather on the principal crops at their various stages of growth, so as to be able to include in their forecasts reference to this all-important subject. A vast improvement has been effected in the weather maps issued at nearly all the more important stations. They contain not only the forecasts prepared at Washington and the local forecasts, but the data on which the forecasts are based. With regard to weather-signal display stations, Mr. Harrington makes a most striking statement. On June 30 there were about 630 stations to which the forecasts were telegraphed. On September 30 the number was 1200—an increase of about 100 per cent.; and large numbers of new stations are being rapidly established. Altogether, the Bureau is evidently in a state of high efficiency, and has profited largely by the attention which has lately been devoted to it by Congress.

MR. HARRINGTON refers in his report to the enormous accumulation of meteorological records now in the U.S. Weather Bureau. These include the observations for the twenty years during which the meteorological work was in the charge of the Signal Service, and also those for the many preceding years when it was in the charge of the Smithsonian Institution. Mr. Harrington proposes to utilize these data by special studies by officers of the Bureau. He also desires that they may be thrown open to all students of meteorology who are competent to use them, subject only to such restrictions as may suffice to preserve them from injury.

REFERRING to the International Conference of Meteorologists at Munich, Mr. Harrington notes that it was attended by four American delegates, of whom he himself was one. He was much pleased with the cordial way in which European meteorologists expressed appreciation of the meteorological work done in the United States. He speaks especially of the interest excited among students on this side of the Atlantic by the international bibliography of meteorology, begun by General Hazen and published in part by General Greely. "Evidently," he says, "the general sentiment in Europe is to the effect that the work thus far done by the Signal Office is too important to be left unfinished, and that the interests of meteorology and of climatology alike demand that the Weather Bureau should publish the complete work in proper style, after obtaining from European co-labourers all possible corrections to the manuscript that has already been milleographed." Mr. Harrington studied closely the meteorological methods adopted in Europe; and he was particularly struck by the fact that the study of climate has, in general, been prosecuted by European meteorologists to a degree of refinement that has not yet been attained, and is, perhaps, scarcely appreciated, in America. For instance, an eminent climatologist, criticizing the location of some instruments on a rise of ground and amid trees, possibly a hundred feet above the surrounding plain, objected that these instruments could not represent properly the climate of the surrounding country, but that they should have been placed in the open flat fields near at hand. "If this person be correct," says Mr. Harrington, "it is evident that the demands of agricultural climatology are very different from those of dynamic meteorology or the study and prediction of daily weather, and it will be an important result of our European journey if we shall have received a decided stimulus in the direction of minute climatology."

DR. E. BIESE, the Director of the Meteorological Office of Finland, has published the observations taken at Helsingfors during the year 1890. In addition to the ordinary hourly observations and summaries, the volume contains hourly values of atmospheric electricity. Owing to want of funds, the publication of the observations had ceased with those for 1883; but a fresh subsidy to the institution has been granted by a decree of the Emperor, so that the publication will be continued regularly in future, and the arrears also worked off. A summary shows that in 1890 rain fell on 178 days and snow on 84 days. The temperature varied from 74°·5 in June to 5°·3 in November, giving an annual range of 69°·2.

IN a recent paper on the camel (*Zeits. für wissen. Geogr.*) Herr Lehmann refers, among other things, to its relations to temperature and moisture. Neither the most broiling heat, nor the most intense cold, nor extreme daily or yearly variations hinder the distribution of the camel. It seems, indeed, that the dromedary of the Sahara has better health there than in more equably warm regions; though, after a day of tropical heat, the thermometer sometimes goes down several degrees below freezing point, and daily variations of 33°·7 C. occur. In Semipalatinsk again, where the camel is found, the annual variation of temperature sometimes reaches 87°·3. In Eastern Asia, winter is the time the animals are made to work. In very intense cold, they are sewn up in felt covers. Of course each race of camel does best in the temperature conditions of its home: a Soudan camel would not flourish in North-East Asia. Camels are very sensitive to moisture. In the region of tropical rains they are usually absent, and if they come into such with caravans, the results of the rainy season are greatly feared. The great humidity of the air explains the absence of the camel from the northern slopes of the Atlas, and from well-wooded Abyssinia. This sensitiveness expresses itself in the character of different races. The finest, most noble-looking camels, with short silk-like hair, are found in the interior of deserts (as in the Tuarek region, in North Africa), and they cannot be used for journeys to moist regions. Even in Fezzan (south of Tripoli) the animals are shorter and fatter, with long coarse hair; and in Nile lands, and on coasts, it is the same. These animals, too, are less serviceable as regards speed and endurance. Herr Lehmann states it as a law that the occurrence of the camel finds its limits wherever the monthly average vapour tension in the air exceeds 12 mm.

LAST week Prof. Cossar Ewart lectured on "Scottish Zoology" to the newly-formed Edinburgh University Darwinian Society, of which he is President. Having given an account of some of the eminent investigators who have devoted themselves to zoology in Scotland, Prof. Ewart spoke of the need for the encouragement of research at the Scottish Universities. In the case of his own department, it ought, he thought, to be possible for him to say to any exceptionally able student, after the completion of his curriculum, "If you are willing to remain for a year or more, I shall be glad to recommend your being elected a research scholar, and to arrange for your obtaining a small sum from a research fund to provide material, &c., required in any investigation you may undertake." Were there two research scholars, or even but one, at work in each of the scientific departments, Prof. Ewart thinks the Scottish Universities would, before long, have a reputation altogether higher and grander than they at present enjoy, to the gain of science and, in all probability, the further amelioration of humanity.

IN his interesting Rectorial address, at Edinburgh, on the use of the imagination, Mr. Goschen referred to the need for imaginative activity in the exact sciences. It would have been difficult for him to say anything new on a subject with which so many distinguished thinkers have dealt; but the ideas he set forth about science and the imagination were sound and well expressed. Referring to the work of Sir William Thomson, he said: "When I think of your fellow-countryman, Sir William

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Thomson, engaged on atoms and molecules, piercing the secrets of the smallest entities, brooding over the mystic dance of ethereal vortices, while his magic wand summons elemental forces to reveal the nature of their powers to his scientific gaze, I forget the disciplined accuracy of the man of science, while lost in wonder at the imaginative inspiration of the poet."

THE trustees of the Missouri Botanical Garden have issued their third announcement concerning garden pupils. The object of the trustees, as we have already stated, is to provide adequate theoretical and practical instruction for young men desirous of becoming gardeners. It is not intended at present that many persons shall be trained at the same time, nor that the instruction shall resemble exactly that given by many State Colleges, but that it shall be quite distinct, and limited to what is thought to be necessary for training practical gardeners. Three scholarships will be awarded by the Director of the Garden before April 1 next. The course extends over six years, so the trustees are particularly anxious that scholarships shall be won by boys who are not much over fourteen years of age.

THE Bulletin of the Botanical Department of Jamaica, for September, contains a report, by Mr. W. Fawcett, Director of Public Gardens and Plantations, on a disease causing the death, on a large scale, of the cocoa-nut palms in the neighbourhood of Montego Bay. The disease first attacks the tissues of the youngest parts. There is no evidence that it is produced by an insect, and Mr. Fawcett considers it is due to an "organized ferment." In the supplement of the *Jamaica Gazette* for September is the remark that the disease is "rapidly destroying the cocoa-nut walks in the parish of St. James, and that, if not checked, in a very few years the cocoa-nut will cease to be a product of this parish, indeed if not of the island."

THE Batavian Society of Arts and Sciences publishes in its Proceedings (Part 46) a list of the chief relics of the Hindu period in Java, and along with it an archæological map indicating the sites of the ruins of temples, statues, and other antiquities. Both list and map are the work of Dr. R. D. M. Verbeek, a well-known engineer.

A PAPER on water and water-supply, with special reference to the supply of London from the chalk of Hertfordshire, by Mr. John Hopkinson, appears in the Transactions of the Hertfordshire Natural History Society (vol. vi., Part 5, October 1891), and has now been published separately. Mr. Hopkinson insists that instead of more water being taken from Hertfordshire for the supply of London the amount at present taken should be reduced. London, he thinks, must sooner or later follow the example of other and much less wealthy towns by obtaining a supplementary supply from a distant source. Liverpool obtains its water from the Vyrnwy, Manchester from Thirlmere, Glasgow from Loch Katrine, and there is a project on foot for Birmingham to obtain a supply from Central Wales. The most feasible scheme for London appears to Mr. Hopkinson to be to obtain a supplementary supply from Bala Lake, or some other lake or lakes in North Wales, or from Central Wales or Dartmoor.

THE White Star liner *Teutonic*, which arrived the other day from New York, after a rapid passage, brought particulars of a collision between the Anchor Line steamer *Ethiopia* and a large whale, eight hundred miles east of Sandy Hook, on the 15th inst., on the passage to New York from Glasgow. At 10.45 a.m. Captain Wilson and Second Officer Fife were on the bridge keeping a close watch ahead. Suddenly a whale came to the surface directly in the path of the ship, and only a few feet ahead. The ship was rushing towards the whale at the rate of sixteen miles an hour. There was no time to check the speed of the vessel, and almost before the astonished officers realized it, the ship's sharp iron prow crashed into the monster. The blow was a square, incisive one. The ship seemed to sail right through the whale, which disappeared almost immediately, leaving a trail of crimson as far as the eye could see. Shortly afterwards the whale was sighted astern, floating lifelessly. When the ship came into collision with the whale the shock caused the vessel to tremble from stem to stern, and startled the passengers for a moment. The passengers who were below rushed on deck, and a panic seemed to be imminent. Captain Wilson hurriedly left the bridge and appeared on deck. "Have no fear," he said, "we have only killed a whale. The ship is not hurt." His words allayed the fears of the passengers.

IN his recent Presidential address to the Royal Society of New South Wales, Dr. A. Leibius referred with satisfaction to the progress made by the cause of scientific and technical education in New South Wales. In addition to the opportunities given by the University of Sydney for the study of science, the Government, by the establishment of a technical college and technological museum at Sydney, with branches in different parts of the colony, have brought within the reach of all who desire it the means of acquiring scientific and technical knowledge. As an illustration of the extent to which the colony is developing this part of its educational system, Dr. Leibius mentioned that contracts already let in connection with the Sydney College alone amount to close upon £48,000, while £20,000 have been voted by Parliament for technical colleges and technological museums at Bathurst, Broken Hill, Maitland, and Newcastle.

THE Michigan Mining School, at Houghton, sends us its "Catalogue" for 1890–91. The course of instruction for the regular students at this institution extends over a period of three years, the work continuing through most of the year. The authorities of the school express an earnest desire to secure as students young men who, before beginning their professional studies, have obtained "an education of the broadest and most liberal character." Every regular student is required "to spend seven hours a day for five days each week in the laboratory or field work, or in recitation or lecture." His "recitations" are prepared "in time taken outside of the seven hours a day." On Saturdays, or on other days, as occasion may require, excursions are made to the mines, mills, and smelting-works in the neighbourhood.

AT a meeting of the Pharmaceutical Society at Edinburgh on November 11, a capital address was delivered by Prof. I. Bayley Balfour, on botanical enterprise in relation to pharmacology. Prof. Balfour devoted himself especially to the task of showing how vast are the obligations of pharmacologists to the Royal Gardens, Kew. The address is printed in the current number of the *Pharmaceutical Journal*.

MR. J. E. DIXON records, in the *Victoria Naturalist* for October, a curious fact which came under his own observation. During a ramble along the Kooyong Creek, Oakleigh, on August 15, he was somewhat surprised to see a specimen of the ring-tailed opossum, hanging, as he thought, by her claws, to a sharp-pointed limb of a gum-tree, about twenty feet from the ground. Upon closer observation he found that the creature was dead, and that death was due to the fact that in her flight she had become impaled by her pouch. In the pouch were two young ones almost old enough to leave her.

MR. ANGELO HEILPRIN contributes to the New York *Nation* of November 12 an interesting paper in which he describes the charms of a summer tour to Greenland. A journey to the 75th parallel of latitude, or thereabouts, could, he says, be arranged annually with much of the certainty of a trans-

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Atlantic trip, and would involve neither hardship nor danger. During the latter part of July and throughout the whole of August the coast is mainly free of ice, and even the passage of the much-dreaded Melville Bay can very generally be effected during this season of the year without danger from a "nip," and frequently with not so much as an acre of ice to interfere with the traveller's journey. Once beyond Cape York, the free North Water opens up a passage to the 79th or the 80th parallel of latitude, or to within some 700 miles of the Pole. In the course of such a trip the traveller would see much that is novel and interesting, much that is grandly picturesque, and still more that is striking in its deviation from the rest of the earth. A country inhabited by a race of people so remarkable as are the Eskimos is always worthy of a visit, especially at a time when a greatly increasing interest in the science is fostering the study of ethnology. But merely in the contemplation of the forms of the almost endless number of icebergs, the vacation tourist would probably consider himself amply repaid for a journey to this easily-reached land of the midnight sun, with its almost numberless glaciers, its sky-splitting mountains, and a boundless ice-cap. The artist, too, would find abundant suggestion for his brush and palette.

PROF. AUGUST WEISMANN'S "Amphimixis: oder, Die Vermischung der Individuen," has been published at Jena by Herr Gustav Fischer. An English translation, we believe, will shortly be issued.

A FRENCH translation—edited by Dr. H. de Varigny—of Weismann's "Essays on Heredity" (Reinwald) has been issued in Paris.

THE third volume of Dr. McCook's "American Spiders and their Spinning Work," will be ready for delivery in the coming spring. The numerous lithographic plates are many of them prepared and in the colourists' hands. The cost of preparing the numerous engravings and plates has greatly exceeded the expectations of the author (who is also the publisher).

DR. ADOLF FRITZE contributes to the *Mittheilungen der Deutschen Gesellschaft für Natur- und Völkerkunde Ostasiens, in Tokio* (Heft 46) a valuable paper on the fauna of Yezo in comparison with that of the rest of Japan. He does not, of course, profess to give a complete account of the subject; but the natural history of Yezo has hitherto been so imperfectly investigated that his work will be very welcome to zoologists.

MR. ROBERT E. C. STEARNS gives in the Proceedings of the U.S. National Museum (vol. xiv., pp. 307–335), a valuable list of shells collected on the west coast of South America, principally between latitudes 7° 30′ S., and 8° 49′ N., by Dr. W. H. Jones, Surgeon, U.S. Navy. This collection, with various other treasures, was presented to the National Museum in 1884; but until lately Mr. Stearns had not an opportunity of preparing a list. A great part of the shells were picked up on the beaches, and in poor condition; but our knowledge of the distribution of west South American species is so limited that the collection, Mr. Stearns says, has its special value for the information it furnishes on this point.

THE following science lectures will be given at the Royal Victoria Hall on Tuesday evenings during December:—December 1, "North Wales," by A. Hilliard Atteridge; 8, "The Ways in which Animals hide Themselves," by E. B. Poulton; 15, "Old Stones," by H. G. Seeley.

AT the meeting of the Chemical Society on Thursday last some further particulars were given by Mr. Mond concerning his work in conjunction with Dr. Langer upon iron carbonyl. They have succeeded in isolating two distinct compounds of iron and carbon monoxide. One of them is a liquid of the composition $Fe(CO)_5$, to which the name ferro penta-carbonyl is given. The other is a solid corresponding to the formula $Fe_2(CO)_7$, and is termed di-ferro hepta-carbonyl. Liquid ferro penta-carbonyl is obtained by heating finely-divided iron, obtained by reduction of ferrous oxalate, in a stream of carbon monoxide. The operation is a very slow one, 100 grams of metallic iron yielding one gram of the liquid in twenty-four hours. Ferro penta-carbonyl is a light amber-coloured liquid, which may be distilled without decomposition. It boils constantly at 102°·8 C. Its specific gravity, compared with water at 18°, is 1·44. It solidifies at −21°, forming yellow acicular crystals. Its vapour density has been determined, the number obtained being 6·5, agreeing fairly well with the value 6·7 calculated for $Fe(CO)_5$. The liquid is quite stable in the dark, but when exposed to light an important change occurs. Gold-coloured crystals rapidly form in it, which upon analysis are found to consist of a second iron carbonyl, the di-ferro hepta-carbonyl $Fe_2(CO)_7$. These crystals are almost insoluble in the ordinary solvents. When warmed to 80°, however, they decompose, the products of decomposition being the penta-carbonyl metallic iron, and carbon monoxide. It appears, therefore, that iron does not exactly resemble nickel in its behaviour with carbon monoxide, for the carbonyl compound of the latter metal, it will be remembered, possesses the composition $Ni(CO)_4$.

A NOTE upon the products of oxidation of nickel carbonyl is contributed by M. Berthelot to the current number of the *Comptes rendus*. M. Berthelot states that nickel carbonyl behaves towards oxygen in a manner somewhat similar to an organic radicle. The products of its spontaneous oxidation do not consist entirely of the oxides of nickel and carbon. The liquid may be preserved in a glass vessel under a layer of water without change so long as air is excluded; but as soon as air is admitted, the compound slowly oxidizes, and a quantity of apple-green hydrated oxide of nickel free from carbon is deposited. At the same time a portion of the nickel carbonyl volatilizes and oxidizes in the air, forming a white cloud which deposits upon all the objects in the neighbourhood. M. Berthelot has succeeded in collecting a considerable quantity of this white deposit, and has subjected it to analysis. He considers it to be the hydrate of the oxide of an organic radicle containing nickel. The numbers obtained from the analysis agree with the formula $C_2O_3Ni_3 . 10H_2O$, but as it appears likely that the preparation contained more or less nickel hydrate this formula is not considered final. M. Berthelot is of opinion that the substance probably contains an organo-nickel compound of the composition C_2ONi, belonging to a type derived from ethylene. He is continuing the study of this interesting substance.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus* ♀) from India, presented by Mr. J. Robinson; a Rhesus Monkey (*Macacus rhesus* ♀) from India, presented by Mrs. K. Clark-Ourry; a Macaque Monkey (*Macacus cynomolgus* ♀) from India, presented by Captain J. F. C. Hamilton; two Ourang-outangs (*Simia satyrus* ♂ ♂) from Borneo, a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, four —— Pelicans (*Pelecanus* sp. inc.) from India, deposited; a Bronze-winged Pigeon (*Phaps chalcoptera* ♂) from Australia, a Blood-breasted Pigeon (*Phlogœnas cruentata* ♀) from the Philippine Islands, purchased.

OUR ASTRONOMICAL COLUMN.

DETERMINATION OF THE SOLAR PARALLAX.—A. Auwers, in *Astronomische Nachrichten* (No. 3066), gives the results obtained in the determination of the solar parallax from the heliometer observations made by the German Transit of Venus Expedition, in the years 1874 and 1882. The number of measurements taken amounted to 754, of which 308 were

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made from the 1874, and the remaining 446 from the 1882 transit. Taking each series of measurements of each transit separately, and applying the corrections of Leverrier's tables,

Transit of 1874 Dec. 8	$\Delta\alpha = +4''.69$	$\Delta\delta = +2''.30$
,, 1882 Dec. 6	$+9''.13$	$+1''.99$

he obtains the following values for the parallax—

Transit of 1874 $\pi = 8''.873$
,, 1882 $\pi = 8''.883$

Both the above numbers are subject to the mean errors $\pm 0''.062$ and $\pm 0''.037$ respectively, and are computed in the first case from 307, and in the second from 444 measurements.

By taking now the two series together, and finding the most probable number, he obtains the following result subject to the two adjoined errors—

$\pi = 8''.880$
Mean error $= \pm 0''.032$
Probable error $= \pm 0''.022$

A comparison of the above results with those of other observers, taking the transits of 1874 and 1882, may be gathered from the following list—

Transit 1874.		Transit 1882.	
Harkness	8".888	Auwers	8".883
Todd	8·883	Cornu	8·86
French measures ...	8·88	Harkness	8·842
Stone	8·88	Faye	8·813
Auwers	8·873	Todd	8·803
Tupman	8·81		
Airy	8·76		

PHOTOMETRIC OBSERVATIONS.—The *Publications of the Potsdam Astro-Physical Observatory*, No. 27, contains a series of photometric measurements made by Dr. Müller at a station on the Säntis, situated 2500 metres above sea-level, with a Zöllner's photometer. The observations extend over two months, and they show that the form of the curve of extinction from the zenith to a point very near the horizon is satisfactorily represented by Laplace's Theory. But a comparison of the curves calculated separately for the various days of observation shows considerable differences, which approach and even exceed 0·4 of a magnitude near the horizon. The superiority of the Säntis station over Potsdam as regards conditions of atmospheric transparency is very striking. For a star in passing from the zenith to an altitude of about 2° has its light diminished nearly by a whole magnitude more in the plain than on the top of the mountain. From the observations, according to Laplace's Theory, the loss of light produced by the atmosphere in the zenith at Säntis is about 12 per cent.; or, in other words, a star viewed from a point above the atmosphere would appear brighter by about 0·14 of a magnitude. Since the corresponding value for Potsdam is 0·2 magnitude, it follows that the absorption produced by a stratum of atmosphere between sea-level and a height of 2500 metres amounts to 0·06 magnitude. Before this value, however, can be accepted as definite, simultaneous observations of stellar magnitudes must be made at stations lying closer together than the two between which the comparison is instituted.

THE PAMIRS.

AT the meeting of the Royal Geographical Society on Monday the paper read was on a recent journey across the Pamir by Mr. and Mrs. Littledale. In introducing the paper, Mr. Douglas Freshfield made some remarks on the subject generally.

The Pamir or Pamirs (Mr. Freshfield said)—for Pamir is a generic term, the different strips of tableland are distinguished by separate names—is a vast tableland averaging 12,000 feet in height and 200 miles in length by 120 to 150 miles in breadth, ringed by a rough horseshoe of mountain ranges, and intersected by snowy ridges and shallow trenches that deepen westwards, where the streams of the Oxus descend towards Bokhara. The numerous photographs taken by Mr. Littledale exhibit a characteristic type of landscape:—tent-shaped, glacier-coated ridges, bare heights naked of verdure and shorn of forests by the bitter winds and frosts, desolate bituminous lakes; a region where for the most part there is neither fuel nor fodder; an Engadine of Asia, with nine months winter and three months cold weather; the home of the wild sheep, the summer haunt of a few wandering shepherds; nomads' land if not no man's land. Long ago Marco Polo described it well. That is the scene of Mr. and Mrs. Littledale's adventures; that is the region where the emissaries of three nations are now setting up rival claims. "The half-way house to heaven" is a Chinese appellation for the Pamirs. "Cœlum ipsum petimus stultitiâ" our and the Russian soldiers and diplomats may now almost say of one another. For the tales of summer pastures of extraordinary richness, told to Marco Polo and repeated to Mr. Littledale, refer, so far as they are true at all, only to isolated oases. The country in question cannot feed the caravans that cross it; far less could it sustain the baggage animals of an army on the march. No one in his senses could consider that in itself the Pamir is a desirable acquisition. Any value it may have is in relation to adjoining lands. From the north there is comparatively easy access to it from Russian Turkistan. From the east the Chinese and their subjects climb up the long ascent from the Khanates, and pass through easy gaps in the encircling horseshoe of mountains on to the portions of the tableland they claim. From the south, a route which seems from Mr. Littledale's experience to be anything but a military route, leads over glaciers, passes, and through well-nigh impassable gorges into Gassin and Chitral, and so to Kashmir. To the south-west easier routes, little known or little described as yet, lead into the wild regions of Kaffiristan and Afghanistan. We do not here deal with politics, but we do deal with the geographical and cartographical facts on a knowledge of which politics and policy ought to be—but unfortunately for our country have not always been—based. Certain portions of the Pamir have been more or less closely attached to Afghanistan. The Amir lays claim to Wakhan, Chignan, and Roshan, tracts stretching along the sources of the Oxus. It is obvious that England will claim an interest in these, but probably, owing to the deficiencies in exact knowledge of the geographers of Cabul, we have not as yet formulated publicly our claims.

In 1873 the Russian Government, at the time of their advance to Khiva, undertook never to pass the Oxus. Shortly afterwards, Sir Henry Rawlinson argued with great force that the Murgabi, the stream that cuts the Pamirs in two, and not the Pandja, which flows along their southern skirts, was the true and proper source of the Oxus. Seven years ago, in the negotiations which followed the Penjdeh incident, the negotiators deliberately left this portion of the frontier out of their calculations.

Why, undeterred by the experiences of which that entertaining traveller and Anglophobe, M. Bonvalot, had lately given so alarming a picture, should an Englishman and his wife cross this desert? Mr. and Mrs. Littledale are eager in the pursuit of rare game. They were old travellers; they had sojourned in the forest wildernesses of the western Caucasus; they had, on a previous occasion, penetrated Central Asia. A pair of horns were to them what a bit of rock from a maiden peak is to others.

And lastly, why did Mr. and Mrs. Littledale go from north to south? Why did they, being English, make Russian territory their starting-point? Thereby hangs a tale. Because our Anglo-Indian Government prohibits all independent travel in its trans-frontier lands. Something may be said for this course, but it does not stop there. It also gags its own official explorers. It carries yearly farther and farther the policy deprecated by Sir H. Rawlinson in this hall, when he said: "Russia deserves all honour for her services to geographical science in Asia. I only wish I could say as much for ourselves as regards our own frontiers."

No one, least of all the Council of this Society, would ask for the publication of any tactical information our military authorities desired to withhold. But the military authorities go along with us in asking for an intelligent censorship in place of a wholesale system of suppression of the mass of knowledge, general and scientific, acquired by the servants of the State in our frontier and trans-frontier lands. We believe, and the Council have represented to H.M. Government, that the present practice is not in accordance with the existing official rules, that it was intended and has been ordered that expurgated copies of all official reports of public interest should be given to the public. They hope that the departments concerned will before

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long be instructed to give practical effect henceforth to any such instructions that may exist, and thus that the forward march of English power may once more, as it should, be accompanied by a general advance of scientific knowledge.

Leaving Samarcand early in May, Mr. and Mrs. Littledale drove in Russian post-carts up the beautiful valley of the Syr-Daria, which reminded them in parts of the Vale of Kashmir, as far as Osh, the last post-station. Here they organized their caravan for their great adventure, the crossing of the Pamirs into Kashmir. They had the advantage of previous experience of Central Asian travel, and of the cordial assistance of the Russian Commandant, Colonel Deubner, who could hardly have done more for the travellers had they been his own nearest relatives. After much hesitation from the difficulty of obtaining any trustworthy information as to the state of the Alai passes, they selected the Taldik, 11,600 feet, before crossing which, they left behind the last tree and bush they were to see untill reaching the valley of the Gilgit.

Crossing the Alai plateau they proceeded by the Kizil Art Pass to Karakul Lake. Thence their route led over passes of 15,500 feet, in sight of the great Mustag Atta to the Murgab or North Oxus, which they struck at 12,300 feet, their correct elevation between the Alai and Sarbad. Another pass of 14,200 feet led over the Alichur Pamir—where *Ovis poli* horns lie about in hundreds—to the Boshgumbaz Valley. The pass of the same name was found impracticable. Mr. and Mrs. Littledale made a long detour to visit the Victoria Lake, one of the sources of the South Oxus, for purposes of sport. Thence they turned eastwards and crossed by the Little Pamir Lake into the Valley of Wakhan. When near Sardab they met with their first misadventure, and this was the encounter with the troops of our ally the Ameer. The civil authorities detained Mr. and Mrs. Littledale for many days, and only let them go at last grudgingly, and after having despoiled them as far as they could without open robbery.

ELIZABETH THOMPSON SCIENCE FUND.

THIS fund, which has been established by Mrs. Elizabeth Thompson, of Stamford, Connecticut, "for the advancement and prosecution of scientific research in its broadest sense," now amounts to $26,000. As accumulated income will be available in December next, the trustees desire to receive applications for appropriations in aid of scientific work. This endowment is not for the benefit of any one department of science, but it is the intention of the trustees to give the preference to those investigations which cannot otherwise be provided for, which have for their object the advancement of human knowledge or the benefit of mankind in general, rather than to researches directed to the solution of questions of merely local importance.

Applications for assistance from this fund, in order to receive consideration, must be accompanied by full information, especially in regard to the following points:—

(1) Precise amount required. Applicants are reminded that one dollar ($1.00 or $1) is approximately equivalent to four English shillings, four German marks, five French francs, or five Italian lire.

(2) Exact nature of the investigation proposed.

(3) Conditions under which the research is to be prosecuted.

(4) Manner in which the appropriation asked for is to be expended.

All applications should reach, before December 10, 1891, the Secretary of the Board of Trustees, Dr. C. S. Minot, Harvard Medical School, Boston, Mass., U.S.A.

It is intended to make new grants at the end of 1891.

*** The trustees are disinclined, for the present, to make any grant exceeding three hundred dollars ($300); decided preference will be given to applications for smaller amounts.

(Signed) HENRY P. BOWDITCH, President.
WILLIAM MINOT, JR., Treasurer.
EDWARD C. PICKERING.
FRANCIS A. WALKER.
CHARLES-SEDGWICK MINOT, Secretary.

List of Grants hitherto made.[1]

1. $200, to the New England Meteorological Society, for the investigation of cyclonic movements in New England. [*American Meteorological Journal* for 1887, and May 1888.]
2. $150, to Samuel Rideal, Esq., of University College, London, England, for investigations on the absorption of heat by odorous gases.
3. $75, to H. M. Howe, Esq., of Boston, Mass., for the investigation of fusible slags of copper and lead smelting. [*Trans. Amer. Institute of Mining Engineers*, Feb., 1890.]
4. $500, to Prof. J. Rosenthal, of Erlangen, Germany, for investigations on animal heat in health and disease, [*Sitzungsber. K. Akad. Wiss.*, 1888, 1309–1319; 1889, 245–254. *Arch. Anat. u. Physiol.*, Suppl. 1888, 1–53.]
5. $50, to Joseph Jastrow, Esq., of the Johns Hopkins University, Baltimore, Md., for investigations on the laws of psycho-physics. [*American Journal Psychology*, 1890, III., 43–58.]
6. $200, to the Natural History Society of Montreal, for the investigation of underground temperatures. [*Canadian Record of Science.*]
7. $210, to Messrs. T. Elster and H. Geitel, of Wolfenbüttel, Germany, for researches on the electrization of gases by glowing bodies. [*Sitzungsber. K. Akad. Wiss. Wien.*, xcvii., Abth. ii., 1175–1264, 1889.]
8. $500, to Prof. E. D. Cope, of Philadelphia, Penn., to assist in the preparation of his monograph on American fossil vertebrates.
9. (Withdrawn.)
10. $125, to Edw. E. Prince, Esq., of St. Andrews, Scotland, for researches on the development and morphology of the limbs of Teleosts. ["Inaugural Dissertation," Pp. 24, Pls. II., Glasgow, 1891.]
11. $250, to Herbert Tomlinson, Esq., of University College, England, for researches on the effects of stress and strain on the physical properties of matter. [*Philos. Magazine*, Jan., 1890, 77–83.]
12. $200, to Prof. Luigi Palmieri, of Naples, Italy, for the construction of an apparatus to be used in researches on atmospheric electricity.
13. $200, to Wm. H. Edwards, Esq., of Coalburg, W.Va., to assist the publication of his work on the butterflies of North America. ["Butterflies of North America," 3rd Series, Part V.]
14. $150, to the New England Meteorological Society, for the investigation of cyclonic phenomena in New England.
15. $25, to Prof. A. F. Marion, for researches on the fauna of brackish waters.
16. $300, to Prof. Carl Ludwig, for researches on muscular contraction, to be carried on under his direction by Dr. Paul Starke. [*Abhandl. math. phys. Classe K. sächs. Ges. Wiss.*, xvi., 1890, 1–146, Taf. i.–ix.]
17. $200, to Dr. Paul C. Freer, for the investigation of the chemical constitution of graphitic acid.
18. $300, to Dr. G. Müller, for experiments on the resorption of light by the earth's atmosphere. [*Publicationen Astrophys. Observ. Potsdam.*, viii., 1–101, Taf'n II.]
19. $300, to Prof. Gerhard Krüss, for the investigation of the elementary constitution of erbium and didymium. [*Liebig's Annalen*, Bd. 265, 1–27.]
20. $50, to Dr. F. L. Hoorweg, for the investigation of the manner and velocity with which magnetism is propagated along an iron bar.
21. $150, to Mr. W. H. Edwards, to assist the publication of his work on North American butterflies. ["Butterflies of North America," 3rd Series, Part VIII.]
22. $250, to Dr. Ernst Hartwig, for researches on the physical libration of the moon (see Grant No. 27).
23. $200, to Prof. Charles Julin, for researches on the morphology of Ascidians.
24. $250, to Prof. M. Nencki, for researches on the decomposition of albumenoids by microbes. [*Arch. Exp. Path. Pharmak.*, xxviii., 311-350, Taf. IV.–V.]
25. $200, to Prof. Carl Frommann, for researches on the minute organization of cells.
26. $300, to Edward Atkinson, Esq., for experiments on cooking, to be carried on under the direction of Mrs. Ellen H. Richards. [Proc. Amer. Assoc. Adv. Sci., 1890.]
27. $250, to Dr. Ernst Hartwig, to continue the work of Grant No. 22.

[1] The results published are given within brackets.

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28. $200, to Edward S. Holden, Esq., for researches on stellar spectroscopy, to be carried on at the Lick Observatory.
29. $150, to Prof. J. Kollmann, for investigations on the embryology of monkeys.
30. $25, to Prof. J. P. McMurrich, Clark University, Worcester, Mass., to study embryology of Aurelia.
31. $200, to Dr. Johannes Dewitz, Zoolog. Institute, Berlin, Germany, for researches on the laws of movement of Spermatozoa.
32. $150, to Alexander McAdie, Clark University, Worcester, Mass., for experiments on atmospheric electricity.
33. $250, to Prof. Julien Fraipont, University of Liége, Liége, Belgium, for the exploration of the cave of Engihoul.
34. $50, to Prof. M. E. Wadsworth, Houghton, Michigan, for observations on the temperature in mining-shafts.
35. $50, to Prof. A. B. Macallum, University of Toronto, Toronto, Canada, to study the digestion and absorption of chromatine.
36. $250, to Dr. G. Baur, Clark University, Worcester, Mass., for the exploration of the Galapagos Islands.
37. $300, to Prof. Edw. S. Holden, Lick Observatory, Cal., for astronomical photography.
38. $250, to Prof. Louis Henry, Louvain, Belgium, for researches on the fundamental solidarity of carbon compounds.
39. $300, to Prof. L. Hermann, Königsberg, Prussia, for phonographic experiments on vowels.
40. $50, to Prof. Alpheus Hyatt, Cambridge, Mass., for researches on the evolution of Cephalopoda.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—Convocation on Tuesday arrived at the following decision:—

"That the University accept the offer of Mr. G. J. Romanes, F.R.S., Christ Church, to give an annual sum of £25 for a lecture to be delivered once a year on some subject approved by the Vice-Chancellor relating to science, art, or literature. The lecturer to be called the Romanes Lecturer, and to be appointed by the Vice-Chancellor annually in the Michaelmas Term, the lecture to be delivered in the next following Easter or Trinity Term on a day to be fixed by the Vice-Chancellor, who shall give public notice thereof to the University in the usual manner. Also, that the thanks of the House be given to Mr. Romanes for his liberality."

We understand it was Mr. Romanes's wish that the foundation should be anonymous; but as such a course was found to be without precedent, and otherwise impracticable, he yielded the point to the University authorities.

Mr. H. T. Gerrans, Fellow of Worcester College, has been elected by the Board of the Faculty of Natural Science a member of the Committee for nominating Masters of the Schools from Hilary Term 1892 to Hilary Term 1895. Mr. C. H. Sampson, Fellow of Brasenose College, has been elected by the same Board of Faculty a member of the Committee for nominating Mathematical Honour Moderators.

SCIENTIFIC SERIALS.

A GOOD deal of interesting geological information is given in the last number of the *Izvestia* of the East-Siberian Branch of the Russian Geographical Society (vol. xxii., 2 and 3). M. Obrutcheff gives an orographical and geological sketch of the highlands of the Olekma and the Vitim, with the exploration of which he was intrusted by the mining administration. Besides the upheavals of these highlands, which have a general direction from the south-west to the north-east, M. Obrutcheff found another series of upheavals stretching west-north-west to east-south-east, the chief ridge of that system (named Kropotkin's ridge by the author) rising to the height of from 1300 to 1500 metres, and separating the tributaries of the Lena from those of the Vitim. Several lower chains seem to have the same direction. The whole series consists of metamorphic slates and limestones, intersected by granites and gneisses, and belongs to the Lower Silurian and Cambrian system, a closer definition of its age being difficult on account of a total want of fossils. M. Obrutcheff also confirms the glaciation of the whole of these highlands. The valleys are filled up with morainic deposits, with polished and striated boulders, and there are traces of inter-glacial layers. The *dômes arrondis* and the *roches moutonnées*, so familiar to the glacialist, are frequent, and the author gives interesting facts to confirm the transport of boulders at great distances over the mountain-ridges, which cannot be explained without admitting that the whole of the highlands was covered with a mighty ice-cap. The same number contains a note by the same author on the Jurassic fossil plants recently discovered on the Bureya River (a tributary of the Amur), and a list of 290 flowering plants collected by Mme. Klements in South Yeniseisk and Tomsk, and described by M. Preyn.

SOCIETIES AND ACADEMIES.

LONDON.

Chemical Society, November 5.—Mr. W. Crookes, F.R.S., Vice-President, in the chair.—The following papers were read:—The magnetic rotatory power of solutions of ammonium and sodium salts of some of the fatty acids, by Dr. W. H. Perkin, F.R.S. Ostwald has argued that the peculiar results obtained by the author in the case of solutions of acids and of ammonium salts, &c., are in accordance with the electrolytic dissociation hypothesis; and has suggested that since salts formed from weak acids are as good conductors as those formed from strong ones, we may expect in this case also, marked deviations from the calculated values. He also considers that such salts as ammonium formate, &c., when in aqueous solution would show molecular rotations which would not be the sums of the rotations of the components of the salts, as must nearly be the case if the view put forward by the author be correct, that such salts are almost entirely dissociated into acid and base. The author has obtained results which show that the rotatory powers of the ammonium and sodium salts do not vary with dilution; and on comparing the experimental values obtained in the case of ammonium salts with those afforded by the constituent acid and ammonia, as might be expected, as reduction of rotatory power always attends combination, the values are slightly less in the case of the salts. This reduction is very nearly the same as that which takes place in the formation of the corresponding ethereal salts, and as the latter are anhydrous, the results show that the values for ammonium salts in solution are practically those of the dry salts, and therefore that Ostwald's views are inapplicable.—Note on the action of water gas on iron, by Sir H. E. Roscoe and F. Scudder. Whilst making experiments on the application of water gas for illuminating purposes, the authors have observed that occasionally the Fahnehjelm comb becomes coated with a deposit of ferric oxide, and a further examination of the tips of the steatite burners showed that the deposit of ferric oxide was "coralloid," and therefore could not be produced from dust in the atmosphere. They also observe that water gas which has been standing in steel cylinders at a pressure of 8 atmospheres for about a month contains a much larger quantity of iron. A preliminary determination of the iron in this gas amounted to 2·4 milligrams per litre. Although the compound, which is doubtless the iron carbonyl of Mond and Quincke, is only present in this small quantity, the authors have succeeded in proving that it can readily be liquefied. In the discussion which followed, the Chairman referred to the fact that at the recent British Association meeting at Cardiff, Mr. Mond had exhibited specimens not only of liquid iron carbonyl, but also of a solid compound of iron with carbonic oxide. Prof. Ramsay stated that he had found that the compound of nickel with carbonic oxide was formed in the cold.—The dissociation of liquid nitrogen peroxide, by J. Tudor Cundale. The author has determined by colorimetric methods the relative amount of NO_2 formed in liquid nitrogen peroxide, (1) by dilution with chloroform, (2) by rise of temperature. He has also ascertained the absolute amounts of dioxide by comparing the colour of the liquid solution with that of the gas containing a known amount of nitrogen peroxide. The results show that, on dilution, (1) dissociation takes place very slowly at first, but more rapidly when less than 5 per cent. of the peroxide is present; (2) that solutions of the peroxide dissociate more rapidly than the pure liquid on rise of temperature.—Ortho- and para-nitro-orthotoluidine, by A. G. Green and Dr. T. A. Lawson. The authors find that when ortho-toluidine sulphate is nitrated in a large

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excess of sulphuric acid at a low temperature, three isomerides are formed—namely, *para*-nitro-ortho-toluidine (about 75 per cent.), *meta*-nitro-ortho-toluidine, $C_6H_3Me(NH_2)(NO_2)[1:2:5]$ (about 3 or 4 per cent.), and *ortho*-nitro-ortho-toluidine, $C_6H_3Me(NH_2)(NO_2)[1:2:6]$ (about 20 per cent.). The separation of the ortho-nitro-ortho-toluidine from the mixture is effected by taking advantage of the greater solubility of this isomeride in slightly warm water. The authors give a table of the properties of the ortho- and para-nitro-ortho-toluidines, and of their products on reduction and other derivatives.—Researches on the gums of the arabin group: Part ii. Geddic acids—Gedda gums; the dextro-rotatory varieties, by C. O'Sullivan. The Gedda gums described consist of the calcium, magnesium, and potassium salts of gum acids, the calcium salt predominating, and more or less nitrogenous matter, which is probably combined with a true gum acid. They dissolve easily in water, forming a yellow or reddish syrup, neutral to test-paper, which is dextro-rotatory. The gum acids are obtained pure by dialyzing their acidified solution, and by fractional precipitation with alcohol. The gum acids in any one sample of gum bear a very simple relation to one another, and are closely related to the gum acids contained in other samples. A table of their relationships is given. The composition and partial constitution of any one of the gum acids which have been as yet examined may be expressed by the general formula, $C_{23}H_{38-2n}O_{22-n}.nC_{12}H_{20}O_{10}.pC_{10}H_{16}O_8$. These gum acids, when heated at 80°–100° for 10–30 minutes with a solution containing 2 per cent. H_2SO_4, are hydrolyzed, yielding arabinon and a gum acid of lower molecular weight. The gum acids thus produced closely resemble the gum acids existing in the natural gums, but are less optically active and more insoluble in weak alcohol. The most marked difference between these gum acids and those existing in the natural gums is that they are only hydrolyzed with difficulty with 2 per cent. sulphuric acid. They are, however, slowly broken down by several hours' digestion, and acids of successively lower weight are formed. The lowest stage of the hydrolysis is represented by the general equation:—

$$C_{23}H_{38-2n}O_{22-n}.nC_{12}H_{20}O_{10} + 3nH_2O = C_{23}H_{38}O_{22} + 2nC_6H_{12}O_6.$$

The compound $C_{23}H_{38}O_{22}$ has not yet been obtained in sufficient quantity for an examination of its properties. Those gum acids obtained from Gedda gum are highly dextro-rotatory, whilst those from gum arabic, although otherwise identical, are inactive.—Some compounds of the oxides of silver and lead, by Emily Aston. The author finds that on following the directions given by Wöhler for the preparation of the compound $Ag_2O,2PbO$ the product varies in composition. A substance of the composition $2Ag_2O,PbO$ is obtained when a mixture of lead and silver hydroxides is allowed to stand in presence of caustic soda, and also by precipitating the mixed nitrates of lead and silver, and exhaustively extracting with caustic soda.—The electrolysis of potassium acetate solutions, by Dr. T. S. Murray. On electrolyzing a dilute aqueous solution of potassium acetate only hydrogen and oxygen are evolved; with concentrated solutions a mixture of ethane, hydrogen, oxygen, methyl acetate, and carbon dioxide is evolved. On diluting the solution the amount of ethane decreases, at first very slowly, but finally with great rapidity. Reducing the current has a similar influence. With rise in temperature, the ethane diminishes, and ceases to be formed at 100°. In contradiction to Jahn, the author finds that the employment of a large anode reduces the yield of ethane; the largest yield is obtained with a very small anode; variations in the cathode do not influence the electrolysis. The results of the experiments are illustrated by curves. The author believes that the ethane is formed, not by partial oxidation of acetic acid, but by a simple interaction of the acetions (CH_3COO). He finds that the yields of ethane from equivalent solutions of potassium, sodium, and calcium acetates are equal.—A new method of preparing β-dinaphthylene oxide, and the constitution of its tetra-sulphonic acid, by W. R. Hodgkinson and L. Limpach. Beta-dinaphthalene oxide is obtained by heating 2:3′ β-naphthol-sulphonic acid to low redness; the distillate is freed from β-naphthol by extraction with alkali, and the residue crystallized from acetic acid. It crystallizes in rhombic plates, and melts at 153°. On sulphonation it yields a tetra-sulphonic acid, which is identical with the product obtained by the continued action of sulphuric acid on β-naphthol.

Linnean Society, November 5.—Prof. Stewart, President, in the chair.—On behalf of a number of subscribers, Mr. Carruthers presented to the Society a half-length portrait in oils of Sir John Lubbock, Bart., M.P., P.C., F.R.S., a former President, painted by Mr. Leslie Ward; and the remarks which he made on the services rendered to biological science by Sir John Lubbock drew from the latter a graceful acknowledgment of the honour conferred upon him.—Amongst the exhibitions which followed, Mr. E. M. Holmes showed some new marine Algæ from the Ayrshire coast; Mr. J. G. Grenfell showed some Diatoms with pseudopodia, illustrating his remarks with diagrams, upon which an interesting discussion followed.—The President exhibited and made some observations on a tooth of the walrus, which illustrated in a curious manner the periods of growth.—Mr. R. V. Sherring called attention to a large series of framed photographs which had been taken under his direction in Grenada, and illustrated the general character of the West Indian flora as well as the physical features of that particular island.—Mr. J. E. Harting exhibited a specimen of Wilson's Petrel which had been picked up in an exhausted state in the Co. Down on October 2 last, and had been forwarded for inspection by Mr. R. Patterson, of Belfast. Mr. Harting gave some account of the species, and remarked upon the unusual number of Petrels, Shearwaters, Skuas, and other marine birds which had been driven inland to a considerable distance during the recent gales.—A paper was then read by the Rev. Prof. Henslow, entitled "A Theory of Heredity based on Forces instead of any special form of Matter." The author maintained that no special form of matter (as is generally supposed) other than protoplasm is required; the latest discoveries of the organized structure of protoplasm militating against the idea of any other special form of matter. Taking illustrations from the animal and vegetable kingdoms, he inquired why two varieties of chickens fed from the first day to full growth were different? It seemed to him more probable that the results were due to different arrangements of the same kinds of molecules rather than to different kinds of "germ-plasm." *Ranunculus heterophyllis*, he pointed out, produced a "land-form" and a "water-form" according to its environment; it therefore exhibited both "heredity" and "acquired characters." As the materials of its structure were the same in both cases, the different results, he considered, must be due to different arrangements of its molecules, and must be effected by forces. The sudden appearance of stomata on the "land-form" illustrated a case of forces normally "potential" while the leaf is submerged, becoming "actual" when the leaf developed in air. After some further deductions, Prof. Henslow concluded that protoplasm and the forces bound up with it were perfectly able to do all the work of transmitting parental characters, as well as to acquire new characters, which in turn might become hereditary as well.

Physical Society, November 6.—Dr. E. Atkinson, Vice-President, in the chair.—Prof. Sydney Young read a paper on the generalizations of Van der Waals regarding "corresponding" temperatures, pressures, and volumes, in which he gave the results of an investigation made with a view of testing whether these theoretical deductions agree with experimental facts. From his virial equation,

$$\left(p + \frac{a}{v^2}\right)(v - b) = R(1 + \alpha t),$$

Van der Waals showed that, if the absolute temperatures of various substances be proportional to their absolute critical temperatures, their vapour pressures will be proportional to their critical pressures, and their volumes, both as liquid and as saturated vapour, will be proportional to their critical volumes. These deductions have now been put to the test of experiment. Some years ago, Prof. Ramsay and the author published data relating to the temperatures, pressures, and specific volumes of methyl-, ethyl-, and propyl-alcohols, ether, and acetic acid. Since then, experiments have been made on benzene and its halogen derivatives—fluor-, chloro-, bromo-, and iodo-benzene—carbon tetrachloride and stannic chloride, and in a few cases the observations have been carried to the critical points. The critical volumes being in many cases difficult to determine with any exactness, the author, instead of expressing the temperatures, pressures, and volumes of each substance in terms of their critical values, found it necessary to compare the various substances with one of them taken as a standard. Fluorbenzene was chosen as standard on account of the very simple relations observed between the monohalogen derivatives of benzene, and the fact of its critical constants (temperature, pressure, and

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volume) having been determined with considerable accuracy. Some of the critical constants of the various substances examined are given in the following table, the brackets indicating calculated values—

Substance.	Formula.	Temperatures in °C.	Pressures in mms. of mercury.	Volumes in c.c.'s of a gramme	Volumes in c.c.'s molecular.
Fluorbenzene ...	C_6H_5F	286·55	33,912	2·43	233
Chlorobenzene ...	C_6H_5Cl	(360)	(33,912)	2·34	(262)
Bromobenzene ...	C_6H_5Br	(397)	(33,912)	1·76	(275)
Iodobenzene	C_6H_5I	(448)	(33,912)	1·47	(298)
Benzene	C_6H_6	288·5	36,395	2·82	219
Carbon tetrachloride	CCl_4	283·15	34,180		
Stannic chloride ...	$SnCl_4$	318·7	28,080		
Ether	$(C_2H_5)_2O$	194·4	27,060		
Methyl alcohol ...	CH_3OH	240·0	59,760		
Ethyl alcohol ...	C_2H_5OH	243·1	47,850		
Propyl alcohol ...	C_3H_7OH	263·7	38,120		
Acetic acid	$CH_3.COOH$	321·6	43,400	2·46	147

Other tables of experimental data—including boiling-points at corresponding pressures, vapour pressures at corresponding temperatures, molecular volumes of liquid and saturated vapours at corresponding pressures and at corresponding temperatures, and ratios calculated therefrom, accompany the paper. From these the author infers: (1) that Van der Waals's generalizations are nearly true for chloro-, bromo-, and iodobenzene when compared with fluorbenzene; (2) that for benzene, carbon tetrachloride, stannic chloride, and ether, the generalizations may only be taken as rough approximations to the truth; and (3) that for the three alcohols and acetic acid, they do not hold good at all. The tables further show that more consistent results are obtained when the comparisons are made at corresponding pressures rather than at corresponding temperatures, particularly in the case of molecular volumes of saturated vapours. The subject of saturated vapours is also treated by another method. If Van der Waals's deductions were strictly true, then the ratios of the actual densities of the saturated vapours of different substances to their theoretical densities should be equal at corresponding pressures. These ratios have therefore been calculated, and show an approximate agreement amongst benzene and its halogen derivatives, carbon tetrachloride, stannic chloride, and ether. For the other substances the agreement is less satisfactory. It is also noted that the ratio of the actual critical density to the theoretical density is for many substances about 4·4. The alcohols differing so widely from the other compounds, were compared amongst themselves instead of with fluorbenzene, with the result that somewhat closer agreement was found, but the deviations were still far outside the limits of experimental error. Of the critical constants the volumes are the most difficult to determine, because at the critical point the curves connecting temperature and volume, and pressure and volume, are parallel to the axes of volume. Accordingly, the author, in some cases, has deduced this quantity by plotting against temperature the numbers representing the ratios of the molecular volumes both of liquid and saturated vapour to those of fluorbenzene at corresponding temperatures and also at corresponding pressures. Four curves result, which should intersect at the critical temperature, and the point of intersection gives the ratio of the molecular critical volume of the substance to that of fluor-benzene. This method leads to results in fair accord with direct determinations. In the discussion which followed the reading of the paper, Prof. Ramsay said the results proved that Van der Waals's generalizations were only rough approximations, and he suggested that some force had been neglected or a term omitted from the equations. Perhaps the assumption that the molecules are incompressible was not correct. He also strongly protested against the tacit assumption of Van der Waals's laws, and deductions made therefrom, which had recently become so common, particularly in German text-books. Prof. Perry inquired whether the quantities *a*, *b*, and *a*, had been determined for different substances and found to be constant. Prof. Ramsay said that for substances in states analogous to those of perfect gases, the quantities were approximately constant, but when the liquid state was approached this was no longer true. According to Prof. Tait, the two states were not continuous. Prof. Herschel remarked that Prof. Tait had established his law on the assumption that the co-volume is four times the volume occupied by the molecules. This law, he said, had been amply verified by experiments on explosions. Dr. Burton, referring to Prof. Ramsay's remarks on the compressibility of molecules, said the law of force between attracting molecules should be accurately known before any deductions were made; and he pointed out that, at constant volume, the pressure should be proportional to the absolute temperature, if allowance be made for the negative pressure of attraction. Mr. Blakesley, in speaking of molecular forces, said he had observed that, when water is allowed to evaporate from glass, a furrow is formed in the glass, which marks out the original boundary of the liquid. To all appearance, the particles of glass are torn away by the molecular forces acting along the boundary.

Geological Society, Nov. 11.—Sir Archibald Geikie, F.R.S., President, in the chair.—The following communications were read:—On *Dacrytherium ovinum* from the Isle of Wight and Quercy, by R. Lydekker. The author described a cranium and mandible of *Dacrytherium Cayluxi* from the Quercy phosphorites, which proved the identity of this form with the *Dichobune ovina* of Owen from the Oligocene of the Isle of Wight. This species should thus be known as *Dacrytherium ovinum*. It was shown tha tthe mandible referred by Filhol to *D. Cayluxi* belongs to another animal.—A discussion followed, in which Mr. Charlesworth and Mr. E. T. Newton took part.—Supplementary remarks on Glen Roy, by T. F. Jamieson. The author discusses the conditions that preceded the formation of the Glen Roy Lake, and appeals to a rain-map of Scotland in support of his contention that the main snowfall in glacial times would be on the western mountains. He gives reasons for supposing that, previously to the formation of the lake, the valleys of the Lochaber lakes were occupied by ice, and that the period of the formation of the lakes was that of the decay of the last ice-sheet. He supports the correctness of the mapping of the terraces by the officers of the Ordnance Survey, and shows how the absence of the two upper terraces in Glen Spean and of the highest terrace in Glen Glaster simplifies the explanation of the formation of the lakes by ice-barriers. The alluvium of Bohuntine is considered to be the gravel and mud that fell into the lake from the front of the ice when it stood at the mouth of Glen Roy during the formation of the two upper lines. During the last stage of the lake, the ice in the valley of the Caledonian Canal is believed to have constituted the main barrier, whilst the Corry N'Eoin glacier played only a subordinate part. The author suggests the possibility of a *débâcle* during the drop of water from the level of the highest to that of the middle terrace, and in support of this calls attention to the breaking down of the moraines of the Treig glacier at the mouth of the Rough Burn. He believes that when the water dropped to the level of the lowest terrace, it drained away quietly, at any rate until it receded from Upper Glen Roy. In discussing Nicol's objections, he maintains that notches would not be cut at the level of the *cols*, and observes that the discrepancy between the heights of the terraces and those of the *cols* has probably been increased by the growth of peat over most of the ground about the watersheds. The horizontality of the terraces is stated to be a fact, and cases are given where waterworn pebbles are found in connection with the "roads," these being especially noticeable in places where the south-west winds would fully exert their influence, and the structure of the terraces is considered to be such as would be produced at the margins of ice-dammed lakes. Further information is supplied concerning the distribution of the boulders of Glen Spean syenite. These are found on the north side of the Spean Valley, at the height of 2000 feet above the sea and 1400 feet above the river, and fragments of the syenite have been carried towards the north-east, north, and north-west. In an appendix, the author discusses Prof. Prestwich's remarks on the deltas, and his theory of the formation of the terraces. After some remarks from Prof. Bonney and Mr. Marr, the President said he agreed that no explanation that had yet been proposed for the parallel roads of Lochaber was free from difficulties. Yet he had long felt that these were far fewer and less formidable in the glacier theory than in any other. Had the terraces been marine, there ought surely to be similar terraces in some at least of the hundreds of sheltered glens in the Scottish Highlands, where the conditions for their formation and preservation were at least as favourable as in Glen Roy and its adjacent valleys. And though the absence of marine shells

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in the Lochaber shelves might not be a serious difficulty, it was hard to understand why such shells should not be found in many localities had the whole country been submerged to the height of the highest Glen Roy "road." Then no satisfactory explanation on the marine theory had ever been given of the coincidence of the terraces with well-marked *cols;* while a further formidable objection to this theory lay in the nature and distribution of the detritus of the shelves, which, in his opinion, was very unlike material arranged in a tidal sea, but was quite what might be looked for in a freshwater lake. He thought that the author's present paper lessened some of the difficulties of the glacier theory by simplifying the grouping of the ice-dams. There still remained the objection that if the Great Glen and the valleys round Ben Nevis were choked up with ice, Glen Roy and its neighbours could hardly have been filled with water. But this difficulty, which every glacialist must have felt, was probably more formidable in appearance than in reality. As Mr. Marr had pointed out, conditions did actually now exist in Greenland very similar to those which, according to the theory so ably expounded by the author, formerly existed in Lochaber.

Royal Meteorological Society, November 18.—Mr. Baldwin Latham, President, in the chair.—Mr. R. H. Scott, F.R.S., gave an account of the proceedings of the International Meteorological Conference, which was held at Munich from August 26 to September 2.—The following papers were also read:—Account of an electric self-recording rain-gauge, by Mr. W. J. E. Binnie. This is a very ingenious instrument, and has been constructed on the assumption that all drops falling from an orifice or tube are identical in weight, as long as the dimensions of the orifice are not varied.—On wet and dry bulb formulæ, by Prof. J. D. Everett, F.R.S. This is a criticism of the methods investigated some years ago by M. August and Dr. Apjohn for determining, by calculation, the maximum vapour tension for the dew point from the temperatures of the dry and wet bulb. Prof. Everett also criticizes the values adopted by Regnault, and says that, in presence of the uncertainty as to a rational formula, he thinks Mr. Glaisher did wisely in constructing his table of factors, which give the dew point approximately by the most direct calculation which is admissible. The inherent difficulties of hygrometric observation and deduction are great, and have not yet been fully overcome.—Results of meteorological observations made at Akassa, Niger Territories, May 1889 to December 1890, by Mr. F. Russell, F.R.G.S. This is in continuation of a former communication respecting the same place. After detailing the results of the various observations, the author says that this period was very unhealthy, and the year 1890 especially so. The weather was exceptionally dry, with small-pox and phthsis amongst the native population. The West Coast reports generally were also unfavourable in reference to the condition of resident Europeans, and at the principal ports quarantine regulations were put in force consequent upon an outbreak of yellow fever in places situated to the south-west. At Bonny ten deaths occurred from November to February out of a population of some sixteen Europeans.

SYDNEY.

Royal Society of New South Wales, September 2.—H. C. Russell, F.R.S., President, in the chair.—The following papers were read:—On a wave-propelled vessel, by Lawrence Hargrave.—Notes on a spontaneous disease among Australian rabbits, by M. Adrien Loir.—Notes on recent celestial photographs, by H. C. Russell, F.R.S.—Some folk-songs and myths from Samoa, by Rev. G. Pratt and Dr. John Fraser.—A quick filter without the aid of pumps was exhibited and described by W. M. Hamlet.

October 7.—H. C. Russell, F.R.S., President, in the chair.—The following papers were read:—Notes on the use, construction, and cost of service reservoirs, by C. W. Darley.—Dr. Fraser presented some myths and historical records from Samoa. The myths had reference to an ancient practice of offering every day a human sacrifice to the sun, and to a chief called "Malietoa the Fierce," and showed how that was stopped. The histories were chiefly genealogies of the kings of Manu'a.

PARIS.

Academy of Sciences, November 16.—M. Duchartre in the chair.—On the secular acceleration of the moon and the variability of the sidereal day, by M. F. Tisserand. From the author's investigation it seems that the increase in the length of the day produced by tidal action is almost the same in amount as the decrease resulting from the secular contraction of the earth due to its cooling, so the length of the sidereal day remains practically invariable.—On the research on the number of roots common to several simultaneous equations, by M. Émile Picard.—On the law of intensity of light emitted by phosphorescent bodies, by M. Henri Becquerel. A correction to a formula given in a previous paper.—The heat of formation of hydrazine and of hydrazoic acid, by MM. Berthelot and Matignon. *Hydrazine*—(1) Heat of solution of hydrazine sulphate at 10°·6 is − 8·70 cal. per molecule. (2) Heat of neutralization: (*a*) by sulphuric acid + 5·55 cal. per equivalent; (*b*) by hydrochloric acid + 5·2 cal. per equivalent. Hydrazine is therefore a weak base comparable to ferric oxide. (3) Heat of combustion of 1 mol. crystallized hydrazine sulphate = + 127·7 cal. (4) Heat of formation $\frac{1}{2}N_2H_4$ = − 4·75 cal. *Hydrazoic acid*—(1) Heat of solution of ammonium salt, = − 7·08 cal. per mol. (2) Heat of neutralization: (*a*) by baryta water + 10·0 cal.; (*b*) by ammonia + 8·2 cal. Hydrazoic acid, dilute, is comparable to amidobenzoic acid, superior to hyponitrous acid. (3) Heat of combustion of the am. salt + 163·8 cal. per mol. at constant volume, and + 163·3 cal. at constant pressure, by explosion in compressed oxygen. (4) Heat of formation of am. salt: (*a*) crystallized − 25·3 cal.; (*b*) in solution − 32·3 cal. Heat of formation of the free acid in dilute solution = − 61·6 cal.—Oxidation of nickel carbonyl, by M. Berthelot. (See Notes.)—Tables of Vesta, by M. G. Leveau. A comparison of the meridian observations made of the minor planet Vesta, from January to April 1890, with positions given in the *Nautical Almanac*, and in an ephemeris computed by means of M. Leveau's tables of this planet. The tables are founded on 5000 meridian observations made between 1807 and 1888, and the masses taken for Jupiter and Mars, respectively, are 1/1045·63 and 1/3,648,000. The mean differences of position are greater in the *Nautical Almanac* than in M. Leveau's ephemeris, both in right ascension and declination.—On secular variations of eccentricities and inclinations, by M. J. Perchot.—On linear differential equations, by M. André Markoff.—On the dielectric power, by M. Julien Lefebvre. From experiments described, the following mean dielectric constants have been derived: sulphur (flower and roll), 2·6; sulphur, cast in rolls six months previously, 3·9; ice, different specimens, 3·45 and 2·4; ebonite, 2·3; paraffin, brown, 2·1, white, 2·0; petroleum, 1·9; carbon bisulphide, 1·7; spirits of turpentine, 1·5. The results agree fairly well with those obtained by Gordon. M. Lefebvre also finds that the dielectric constant of sulphur increases with the time.—On an application of photography to the polariscope, by MM. Chauvin and Charles Fabre.—Action of light on ruthenium peroxide, by M. A. Joly.—Salts formed by oxygen compounds of ruthenium inferior to ruthenic and heptaruthenic acids, by M. A. Joly.—On the iodonitroso and bromonitroso compounds of platinum, by M. M. Vèzes.—On the coloration of solutions of cobalt, and the state of its salts in solution, by M. A. Étard.—The nitration of silk, by MM. Léo Vignon and P. Sisley.—On the implanting of large pieces of decalcified bone to fill up losses of the substance of the skeleton, by M. Le Dentu. It has been found that pieces of decalcified bone substituted for a portion or the whole of a diseased bone plays the part of a temporary support which, before disappearing, allows the periosteum or the osseous tissue sufficient time to reconstruct a new bone.—On some phenomena of reproduction of Cirrhipedes, by M. A. Gruvel.—On the age of the fauna of Samos, by Mr. Forsyth Major.—On a Neolithic flint working (*exploitation*) of a new type, by M. Armand Viré.

BERLIN.

Physiological Society, October 30.—Prof. du Bois-Reymond, President, in the chair.—Prof. Gad reported on experiments, conducted under his direction by Dr. Schtscherbak, on the alteration of the movements of eating in rabbits which result from removal of certain parts of the cerebrum.

Meteorological Society, November 3.—Prof. Schwalbe, President, in the chair.—Dr. Zenker spoke on the relationship of solar radiation, as it would really occur if the sun were directly overhead and there were no atmosphere, to the actually existing and observed temperatures of stations, taking into account their proximity to oceans and continents.—Prof. Hellmann made a short communication on the recent experiments in America on the artificial production of rain.

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Physical Society, November 6.—Prof. du Bois-Reymond, President, in the chair.—Dr. Raps explained certain modifications which he had introduced into his automatic mercurial air-pumps, and demonstrated the action of the pump on a Geissler tube, which he rapidly exhausted so completely that a phosphorescent light made its appearance in it.—The President made some remarks on photographs of the human retina.—Prof. Kundt described Dr. Zehnder's new and simple differential-refractor, an instrument by means of which the two rays destined to produce interference may be kept some 50 to 100 cm. apart, and be subjected separately to varying experimental conditions.

DIARY OF SOCIETIES.

LONDON.

THURSDAY, NOVEMBER 26.

ROYAL SOCIETY, at 4.30.—On Instability of Periodic Motion: Sir William Thomson, Pres.R.S.—A New Mode of Respiration in the Myriapoda: F. G. Sinclair.—Further Observations on the Gestation of Indian Rays: J. Wood-Mason and A. Alcock.—On some Variations observed in the Rabbit's Liver under certain Physiological and Pathological Circumstances: Dr. Brunton, F.R.S., and Dr. Delépine.—On the Electromotive Phenomena of the Mammalian Heart: W. M. Bayliss and Dr. E. H. Starling.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—Description of the Standard Volt and Ampere Meter used at the Ferry Works, Thames Ditton: Captain H. R. Sankey (late R.E.) and F. V. Andersen.

LONDON INSTITUTION, at 6.—On the Spread of Commerce in Europe in Prehistoric Times: Prof. Boyd Dawkins, F.R.S.

CAMERA CLUB, at 8.30.—Some Analogous Aspects of Painting, Music, and Poetry (Musical and Pictorial Illustrations): Rev. F. C. Lambert.

FRIDAY, NOVEMBER 27.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—Modern Railway Carriages: Walter Clemence.

CAMERA CLUB, at 8.—Retouching: Redmond Barrett.

SATURDAY, NOVEMBER 28.

ROYAL BOTANIC SOCIETY, at 3.45.

ESSEX FIELD CLUB, at 6.30 (at Loughton).—On some Ancient Lake Remains at Felstead, with Notes on other similar Remains in the District: J. French.—The Life-History of the Hessian Fly: F. Enock.

SUNDAY, NOVEMBER 29.

SUNDAY LECTURE SOCIETY, at 4.—How came the Great Ice Age? (with Oxyhydrogen Lantern Illustrations): Sir Robert S. Ball, F.R.S.

MONDAY, NOVEMBER 30.

ROYAL SOCIETY, at 4.—Anniversary Meeting.

SOCIETY OF ARTS, at 8.—The Pigments and Vehicles of the Old Masters: A. P. Laurie.

ROYAL MICROSCOPICAL SOCIETY, at 8.—*Conversazione*.

ARISTOTELIAN SOCIETY, at 8.—Croll's Philosophical Basis of Evolution: Arthur Boutwood.

LONDON INSTITUTION, at 5.—Recent Progress in Astronomy (Illustrated): Sir Robert Ball, F.R.S.

CAMERA CLUB, at 8.30.—Lantern Evening.

TUESDAY, DECEMBER 1.

ZOOLOGICAL SOCIETY, at 8.30.—Notes on Transcaspian Reptiles: G. A. Boulenger.—Further Descriptions of New Butterflies from British East Africa, collected by Mr. F. J. Jackson during his Recent Expedition, Part II.: Miss E. M. Sharpe.—On the Association of Gamasids with Ants: A. D. Michael.—Notes on the Bornean Rhinoceros: Edward Bartlett.

INSTITUTION OF CIVIL ENGINEERS, at 8.—Monthly Ballot for Members.—Renewed Discussion on Portland Cement and Portland-Cement Concrete: Messrs. Bamber, Carey, and Smith.

WEDNESDAY, DECEMBER 2.

SOCIETY OF ARTS, at 8.—Secondary Batteries: G. H. Robertson.

ENTOMOLOGICAL SOCIETY, at 7.—Notes on Lycæna (*recte* Thecla) Rhymnus, Tengstræmii, and Pretiosa: George T. Baker.—The Effects of Artificial Temperature on the Colouring of Vanessa urticæ and certain other Species of Lepidoptera: Frederic Merrifield.—On the Variation in the Colour of the Cocoons of Eriogaster lanestris and Saturnia carpini: W. Bateson (communicated by Dr. D. Sharp, F.R.S.).

THURSDAY, DECEMBER 3.

CHEMICAL SOCIETY, at 8.—Ballot for the Election of Fellows.—Phosphorus Oxide, Part II.: Prof Thorpe, F.R.S., and A. E. Tutton.—On Frangulin, Part II.: Prof. Thorpe and Dr. A. K. Miller.—The Structure and Character of Flames: A. Smithells and H. Tingle.—The Composition of Cooked Vegetables: Miss K. J. Williams.—On the Occurrence of a Mydicatic Alkaloid in Lettuce: T. S. Dymond.—On some Metallic Hydrosulphides: S. E. Linder and H. Picton.—On the Physical Constitution of some Solutions of Insoluble Sulphides: Harold Picton.—Solution and Pseudo-Solution: H. Picton and S. E. Linder.

LINNEAN SOCIETY, at 8.—A Contribution to the Freshwater Algæ of the West of Ireland: W. West.—The Tick Pest in Jamaica: Dr. W. H. W. Strachan.

LONDON INSTITUTION, at 6.—The Tower of Babel and Confusion of Tongues (Illustrated): Theo. G. Pinches.

CAMERA CLUB, at 8.30.—On Toning Bromide Paper and Transparencies (with Demonstration and Illustrations): A. R. Dresser.

FRIDAY, DECEMBER 4.

PHYSICAL SOCIETY, at 5.—On a Permanent Magnetic Field: W. Hibbert.—Note on the Production of Rotatory Currents: Prof. Ayrton, F.R.S.

GEOLOGISTS' ASSOCIATION, at 8.—Supplementary Observations on some Fossil Fishes from the English Lower Oolites: Arthur Smith Woodward.—Organic Matter as a Geological Agent: Rev. A. Irving.

INSTITUTION OF CIVIL ENGINEERS, at 2.—Students' Visit to inspect the New Refrigerating Plant at Nelson's Wharf, Commercial Road, Lambeth.

CAMERA CLUB, at 8.—Retouching: Redmond Barrett.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Leçons sur les Métaux, 2nd fasc.: A. Ditte (Paris, Dunod).—Hand-book to the Geology of Derbyshire, 2nd edition: J. M. Mello (Benrose).—Annals of British Geology, 1890: J. F. Blake (Dulau).—The Ouse: A. J. Foster (S.P.C.K.).—Hand-book of Psychology—Feeling and Will: J. M. Baldwin (Macmillan).—Index-Catalogue of the Library of the Surgeon-General's Office, U.S. Army, vol. xii. (Washington).—Electricity tested Experimentally, 3rd edition: L. Cumming (Longmans).—Problems in Chemical Arithmetic: E. J. Cox (Percival).—An Account of British Flies, vol. i. Part 2: F. W. Theobald (Stock).—A Treatise on the Geometry of the Circle: W. J. McClelland (Macmillan).—Beast and Man in India: J. L. Kipling (Macmillan).—Principles of Agriculture: edited by R. P. Wright (Blackie).—Elementary Inorganic Chemistry, new edition: A. H. Sexton (Blackie).—Euclid, Book XI.: A. E. Layng (Blackie).

PAMPHLETS.—Water and Water-Supply: J. Hopkinson (Hereford).—History of Liberia: J. H. T. McPherson (Baltimore).—The Nuptial Number of Plato: J. Adam (Clay).

SERIALS.—Zeitschrift für Wissenschaftliche Zoologie, 53 Band, 1 Heft (Williams and Norgate).—Cyclone Memoirs, Part 4; W. L. Dallas (Calcutta).—Journal of the Anthropological Institute, November (K. Paul).—Government of India Meteorological Department, Monthly Weather Review March and April 1891 (Calcutta).—Indian Meteorological Memoirs, vol. iv., Part 7 (Calcutta).

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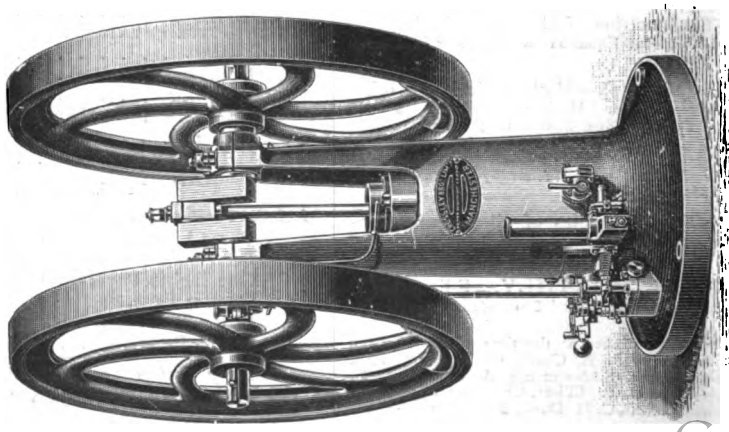

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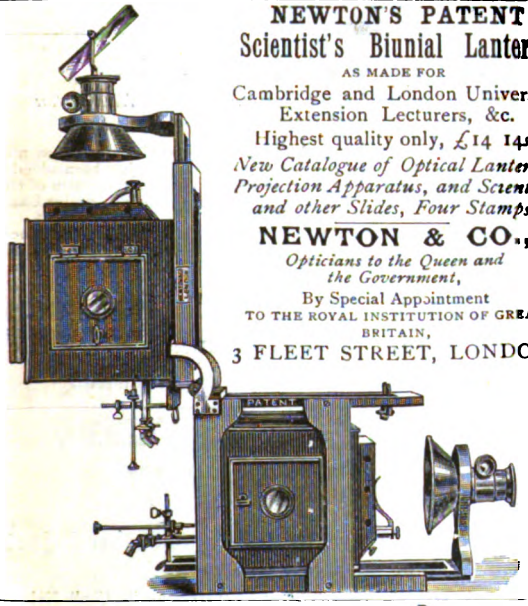

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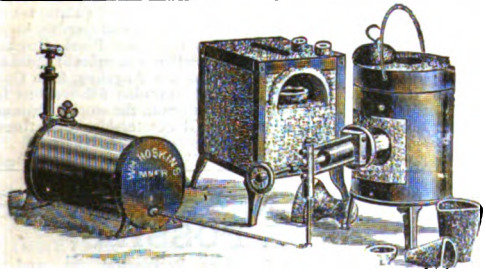

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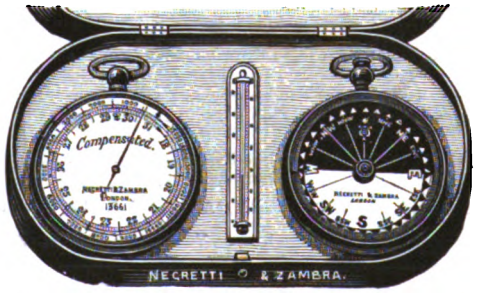

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THURSDAY, DECEMBER 3, 1891.

FIELD GEOLOGY.

Outlines of Field Geology. By Sir Archibald Geikie, F.R.S. (London: Macmillan and Co., 1891.)

GEOLOGISTS, we are sometimes told, are a combative race. Geologists may fearlessly allow that the impeachment has a spice of truth in it. They may take comfort when they reflect that no serious consequences have ever followed from this tendency, in spite of the facilities which the formidable weapons they carry with them offer for pushing it to an extreme. Their healthy out-door life prompts banter, and the passes are apt to be so quick and keen that the uninitiated may be pardoned if they think the buttons are off the foils.

The meetings of the Geological Society have witnessed many a sharp passage of arms. It may be permissible to recall one. A well-known member of the brotherhood, safe long ago among the majority, of large and varied experience, was indulging in just a little brag about the broad areas he had surveyed. The retort came sharp and quick from one whose quips and cranks are now alas heard no more: "Where are your maps?" And the contemptuous answer was, that the chief requisites for geological mapping were a stout pair of legs and sound wind. There were elements of truth in this lively sparring, despite its extravagance.

It is a truism that need hardly be repeated, that geology cannot be learned without out-door work, and geological excursions are a necessary item in all geological teaching. But what do they amount to? There is a leader who knows the country well. He selects a line along which sections follow one another in close succession. The exposures are so plentiful and near together that even the beginner realizes without difficulty the order in which the several rock-groups follow one above the other, and there are ample opportunities for mastering their lithological character and fossil contents. A longitudinal section is readily constructed, and figures with more or less of misconception in the note-book of each of the party. An admirable start this. But what is it compared with the mental discipline that goes along with the making of a geological map, and the grip of the subject that results from this form of geological work? There is as much difference between the two as between that form of sport which consists in riding behind a pack of hounds who follow a trailed herring, and the stalking of deer in their native wilds.

It is in mapping more than in any of its other branches that geology rises to the level of an educational tool. Here there must be the instinctive skill, acquired by long practice, which leads the surveyor to select in his preliminary work the traverses most likely to give a broad view of the structure of the district he is working over; the patience which forbids, when the first rough sketch comes to be filled in, that a single square yard of ground shall be left unvisited, lest some bit of evidence should be missed; and the constructive power which pieces together the accumulated mass of multifarious data into a consistent whole. Keenness of eye, neatness of hand, judgment, unwearied application, and chastened imagination figure among the requisites for the work, and grow in strength as it proceeds. Surely the finishing touch in a geologist's education is given by the making of a geological map.

That the art cannot be learned from books alone, goes without saying; that books can do but little towards teaching it, may be safely maintained. But there is no reason why a master should not give us in print all the aid that a book can afford, and lure us to the fascinating pursuit by an eloquent description of its charms. And that a book which deals with field-geology has been found of service, and that geologists are not averse to hear the praises of their favourite employment, are proved by the fact that the little book on this subject by the Director-General of the Geological Survey is now in its fourth edition.

The work is primarily addressed to geologists, but it appeals also to those who have no claim to so distinctive a title, and no wish for it. It shows how much pleasure may be derived from an acquaintance with the science no larger than any intelligent person may easily acquire; how even this moderate amount of knowledge enhances the enjoyment of travel and of the daily walk. But let the author speak for himself.

"To those who are fond of country rambles geology offers many attractions. Few men are so unobservant as not to be struck, now and then, by at least the more salient features of a landscape. Even in a flat, featureless country, the endless and apparently capricious curvings of the sluggish streams may occasionally suggest the question why such serpentine courses should ever have been chosen. But when the ground rises into undulations, and breaks out into hills or crags; still more, when it towers into rugged mountains, cleft by precipices down which the torrents are ever pouring, and by ravines in the depths of which the hoarse streams ceaselessly murmur, one can hardly escape the natural curiosity to know something about these singular aspects of the landscape, when and how they arose, and why they should be precisely as they are."

Our author goes on to say that "the day is now happily past when the sterner features of the land awakened only a feeling of horror; when they were styled hideous and unsightly; when they were never visited save under the necessities of travel, and were always left behind with a sense of relief." That the appreciation of the beauty of mountain scenery is a taste of modern growth can hardly be disputed. It is open to question whether the comforts of modern travel have not done as much to foster it, as a scientific curiosity to know how the forms which charm our eye were produced. But, however this may be, the awakening and the satisfying of such curiosity are added items to the stock of pleasures which the lover of Nature derives from her wilder aspects.

Some of the inducements to field-work having been thus attractively put forward, the author defines the aim of the book. The student

"must betake himself to Nature from the first. His lessons in the field should accompany his lessons from the text-book or lecture-room. In many cases he must grope his way without guide or assistance. . . . The following chapters are offered for his help. . . . Their aim is to point out how observations may be made, what kinds of data should be looked for, what sort of evidence should be sought to establish a conclusion, and

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what deductions may be drawn from particular facts. In short, they are to be regarded as sign-posts pointing out some of the highways and byways of geological inquiry, but leaving the reader to perform the journey in his own fashion."

At the outset it is made clear that, though some localities supply more material than others, even the most unpromising are far from barren of interest to those who have eyes to see. The necessary equipment is fully described. In addition to the "cheese-taster" mentioned on p. 29, the portable boring-apparatus devised by Belgian geologists has been found useful. So much stress has been already laid on the value of mapping, that it will be enough to say that, in the author's opinion, the geologist "acquires by it a thoroughness of grasp attainable in no other way." It would have been well if the meaning of contour lines, and the way in which hill-shading is used to denote the shape of the surface, had been treated of more fully. The illustrations of the sinuosities of outcrop across uneven ground, on p. 101, are beautifully distinct, but nothing is said as to the value of models in making clear this point to the beginner. Of course they tell most when he makes them himself: a little modelling clay can generally be obtained; but, in default of better material, an apple or turnip can be made to do good service. In the very handy approximate rule, on p. 116, for determining the thickness of a bed when its dip and the breadth of its outcrop are known, it is not stated that the surface on which the breadth is measured must be horizontal; or, if it be an inclined plane, we must substitute for dip the sum or difference, as the case may be, of the dip of the bed and the inclination of the plane to the horizon. The error committed in using this rule for a dip of 5° is a little over 4 per cent. in defect; it decreases as the dip increases up to 30°: for that angle the rule is accurately true. For dips over 30° the rule gives the thicknesses too great; the error increases rather rapidly with the dip, and at 45° amounts to 6 per cent.; for a dip of 50° it is nearly 9 per cent.

The great value of the book consists in this: from beginning to end it embodies the experiences of one who has spent a lifetime in field-work, and so it is full of "wrinkles" which could not possibly have been picked up in any other way. Most of these anyone who has in him the making of a field-geologist would sooner or later have found out for himself; but it is no small gain to the beginner to be put on the right track to start with, and to have his instructions conveyed with the life and freshness that practical acquaintance can alone give. The present writer feels proud that in one respect he can claim even a more extended experience than the author of the book: the latter, apparently, has never been suspected of being an incendiary (see p. 20), a fate which once befell him who pens this notice. A. H. GREEN.

THE LAND OF THE LAMAS.

The Land of the Lamas: Notes of a Journey through China, Mongolia, and Tibet. By William Woodville Rockhill. With Maps and Illustrations. (London: Longmans, Green, and Co., 1891.)

MR. ROCKHILL has for so long a time taken special interest in Tibet that he is able to speak of it as his "life hobby." Accordingly, when, in 1884, he was attached to the U.S. Legation at Peking, he looked forward with pleasure to the chance of exploring the country; and he began to make the best of all preparations for the undertaking by studying the Tibetan language. At this he worked for four years, during which he also gave some time to the study of Chinese.

In the winter of 1888 he resigned his post of Secretary of Legation, and made ready for his long-anticipated journey. He determined to enter Tibet from the north, and started from Peking on December 17. The choice of this route made it necessary for him to make in the first instance for Lan-chou, the capital of the province of Kan-su; so he arranged with a cart firm to supply him with two carts, with two mules to each, to take him there in thirty-four days. For every day over this they were to pay him two taels, he giving them the same amount for every day gained on the date agreed upon. This plan worked admirably, and he reached his destination two days ahead of time. The distance was 1350 miles.

From Lan-chou, Mr. Rockhill advanced to Hsi-ning, the residence of the Chinese Imperial Controller-general of the Koko-nor, an official known to the Tibetans and Mongols as the Amban. As this magnate was strongly opposed to foreigners, Mr. Rockhill made off from Hsi-ning as soon as possible, going to Lusar, a village near Kumbum. Kumbum is a great religious centre, and he was fortunate enough to be present on the 12th of the first moon, when the Chinese in every village and town of the empire celebrate the dragon festival. After looking through one of the temples, he and those who accompanied him walked round it, keeping it on the right hand—"a mode of showing respect for sacred things observed in all lamaist countries." At a lamasery in the neighbourhood, where he found a native who had taught him Tibetan at Peking, he had much pleasant talk with various lamas. These authorities were, he says, "immensely amused" by what he had to tell them about esoteric Buddhism and the Mahatmas.

Mr. Rockhill's final preparations were made at Tankar, the most westerly border town in that part of Kan-su. From this place, with his men, he set out on March 24, 1889, and was soon across the frontier. His intention was to visit Lh'asa; and had he succeeded in carrying out this plan, his narrative would probably have taken rank among the foremost modern books of travel. Unfortunately, the difficulties in the way proved to be insuperable, and he was obliged to content himself with a much less ambitious enterprise. The first country traversed by the party was Koko-nor, their route taking them past the northern shore of the great lake from which the region derives its name. This lake is some 230 miles in circumference, and its altitude is about 10,900 feet above sea-level. Looking at it from a low pass to the north-east, Mr. Rockhill describes it as "a glistening sheet of ice, stretching as far as the eye could reach to the west, and bounded to the south by a range of high, bleak mountains with snow-tipped peaks." To the north and west of the lake there is fine grazing-land, which is watered by many streams. Here nomads find excellent camping grounds in swales and hollows, which afford protection from violent west and north-west winds.

After leaving Koko-nor, the party passed in a southerly direction through Ts'aidam, crossing the Timurté

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Mountains, beyond which they entered the desert of Koko Beileh. Starting from the village of Shang, in Ts'aidam, Mr. Rockhill explored a district which Prjevalsky did not find it convenient to visit. Here he followed the course of the Yohuré, on the left bank of which a range of mountains culminates in a peak which Mr. Rockhill estimated to be between 17,000 and 18,000 feet above sea-level. This peak he called Mount Caroline. The district was everywhere "literally alive with game," yak and wild asses being particularly plentiful. Of the Mongols of Ts'aidam, Huc and Prjevalsky have given very unfavourable accounts. Mr. Rockhill found them honest and hospitable; and he says they are much more devout Buddhists than the Koko-nor Tibetans. Among the latter, the laity "do not bother themselves about praying, thinking that they pay the lamas quite enough to do all that is necessary for their good," whereas the Ts'aidam Mongols "are continually mumbling prayers, twirling prayer-wheels, or perhaps doing both at the same time." At Shang a pole supporting two prayer-wheels was attached to the roof of nearly every house. The wheels were put in motion by the wind, which was caught by a simple arrangement of wooden cups fixed on the ends of horizontal sticks, and looking like our anemometers.

From Ts'aidam the party entered North-Eastern Tibet, and here they traversed for the most part a bleak country until they were some way beyond Jyékundo. The country between the range to the south of Alang-nor and the source of the Yellow River has an average altitude of about 14,500 feet; and at this height the horses and dogs soon showed signs of great fatigue. "We felt no brighter than the animals," says Mr. Rockhill; "our clothes seemed to weigh tons, our guns loaded us down, and walking, even on the level, was such a violent effort that perspiration poured down our faces." They were repeatedly warned that it would be impossible for them to cross the Dré Ch'u or upper Yang-tzu Kiang. The passage, however, was effected without serious difficulty. At the point where they first saw it, the river was of a beautiful blue colour. It was about 120 yards wide and 20 feet deep, and flowed swiftly between high, bare, reddish mountains. South of the Dré Ch'u they crossed the Zonyik Valley (altitude, 16,300 feet), at the head of which they saw twelve argali, a kind of sheep which is said to be not uncommon in the wilder gorges along the river. The snow was so deep, and any exertion so exhausting, that Mr. Rockhill did not try to get a shot at them. Beyond this valley the party came to Taglung-la, (altitude, 16,650 feet), the highest pass crossed in the course of the whole journey.

Jyékundo proved to be a pretty place, nestling at the foot of a high, steep hill crowned with the brightly coloured walls of a lamasery. After the fatigues the party had undergone, Mr. Rockhill wished very much to spend a week or two at this village; but the abbot of the lamasery, who was the chief of the district, was so bitterly hostile that they had to depart in haste. During the first day's journey beyond this point the country remained bare and bleak; but when, on the following day, they entered the Dren-kou valley, which leads down to the Dré Ch'u, the scenery "changed as if by magic." They found themselves in a fertile and picturesque glen, where juniper and pine trees grew on the mountains, while by the roadside were plum, gooseberry, honeysuckle, and other shrubs, the fragrance of their blossoms filling the air. A brook flowed between banks covered with soft green grass "powdered over with little white and pink flowers." During the remainder of the journey Mr. Rockhill was often troubled by the lamas, but in other respects his difficulties were less formidable. The route passed through many villages, and he often has occasion to express admiration for the charm of the scenery. Advancing in a south-easterly direction, he came to Dérgé, the richest agricultural and manufacturing region of Eastern Tibet, and the most densely populated. It is especially famous for the excellence of its metal-work. Mr. Rockhill spent some days at Kanzé, the chief city of the Horba country, the inhabitants of which seemed to him the best-looking people he had seen in Tibet. Lu, the Chinese official stationed at Kanzé, warned him that he might encounter serious dangers beyond that city, and insisted on his taking an escort of four Chinese soldiers. Talking of wild tribes to the north of the Horba country, this official assured Mr. Rockhill that men in a state of primitive savagery were found in Tibet. Lu himself had seen a number of wild men who had been driven out of woods by a forest-fire on the flank of Mount Ka-lo, east of Kanzé. "They were very hairy, their language was incomprehensible to Tibetans, and they wore most primitive garments made of skins. He took them to belong to the same race as the Golok, of whom many lived in caves in a condition of profound savagery." From Jyékundo to Dawo, Mr. Rockhill followed the route which had been taken by Pundit A— K—, or Krishna, in 1882; but from Dawo to Ta-chien-lu he chose a different way, and thus had an opportunity of studying a new section of country, which he carefully describes. At Ta-chien-lu, his travels, so far as Tibet was concerned, came to an end. He had still before him, however, a long journey in China, and of the more striking part of it he gives an excellent account, presenting with special vigour the incidents of a trip down the Yang-tzu Kiang, which had "just enough of danger in it to give it zest."

Mr. Rockhill writes simply and clearly, and geographers will read with interest all he has to say of the more remote regions through which he passed. His remarks on the people are not less valuable. It is stated by Chinese writers of authority that for every family in Tibet there are three lamas, and Mr. Rockhill does not believe that this is an exaggerated estimate. From Jyékundo to Ta-chien-lu, a distance of about 600 miles, he passed forty lamaseries, in the smallest of which there were 100 monks, and in five of them from 2000 to 4000. Their landed property is in many cases enormous, and their serfs and bondsmen swarm. There are four lamaist sects, called by the Chinese, yellow, red, black, and white lamas; but the laity do not attribute much importance to the differences between them. In Eastern Tibet there is also a creed known as "Bön." It represents the pre-Buddhist Shamanism of the country. The "Bönbo" are held in great scorn by the lamas; but as their charge for "beating the drum" is exceptionally low, they are readily invited to the houses of the common people for religious services.

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Mr. Rockhill is of opinion that polyandrous marriages, although frequently met with, are not by any means so numerous in Tibet as we have hitherto been led to suppose. Polyandry exists only in agricultural districts, and he suggests that it is maintained there because the tillable lands are of small extent, and every family feels it to be important that the ancestral estate should not be divided. This, however, as he himself points out, can at best explain, not the origin of polyandry, but merely the fact that the custom is still permitted to survive.

We may note that the illustrations are excellent, and that two good maps make it easy for the reader to trace the author's route.

SCIENCE AND BREWING.

A Text-book of the Science of Brewing. By E. R. Moritz, Ph.D., and G. H. Morris, Ph.D. Based upon a Course of Six Lectures delivered by E. R. Moritz at the Finsbury Technical College. (London: E. and F. N. Spon, 1891.)

BREWING is an industry which, as a rule, does not excite the interest in scientific minds that it deserves. The reason is difficult to explain, for there is no industry which involves more problems of general scientific moment, or makes more varied calls on the different sciences. As an illustration—noting very briefly a few points in the manufacture of beer—we have in malting a study of the embryological development of the barley plant, and the secretion and use by the growing embryo of those curious enzymes which render both the carbohydrate and proteid food of the endosperm available; in the mashing, or infusion of malt with water, we meet with the action of the enzyme, diastase, upon starch, involving some of the most complex molecular changes known; and in fermentation, produced both by the Saccharomyces and Bacteria, we have all the interesting difficulties connected with the morphology and zymotic powers of these organisms. It is evident that any technical scientific work on such subjects as those just mentioned, involves questions of the greatest general scientific interest, and touches on points at the extreme limit of our present knowledge; consequently, it is not surprising to find that science owes some most important advances to scientific workers in the field of brewing. For instance, our knowledge of the constitution of starch, and the changes it undergoes during hydrolysis by the action of acids and diastase, is almost entirely due to the researches of C. O'Sullivan and of Horace Brown, both connected with the industry of brewing.

Dealing, then, as the science of brewing does, with some of the most complex problems known, investigations in this field of work carry with them more than ordinary technical interest, and should excite more general interest in the scientific aspect of the industry than seems to be accorded to it at present. A perusal of Drs. Moritz and Morris's "Text-book of the Science of Brewing" has induced us to make these remarks, for contained in this work we find for the first time a lucid and correct account of the important scientific principles involved in the brewing industry, and the work that has been done upon them. Although, of course, mainly written for technical purposes, the authors have treated the whole of their subject in such a manner that the book undoubtedly has a general scientific value beyond the circle of those for whom it is mainly intended. We are sure that anyone wishing to look up such subjects as starch and its transformations, or fermentation, would do well to consult this work, for, apart from the admirable *résumés* of our present knowledge on such subjects, the abundant references given, to the authors quoted, are themselves of much value.

Hitherto there has been great want of a technical guide to the scientific principles of brewing, nothing in the least worthy of such a name having been published; and the unfortunate student of this subject has been compelled to attempt the almost impossible task of collecting his information from a literature scattered far and wide, with no guide to teach him how to do it, or how to select the good from the bad when it was done. His difficulty is now over, we are pleased to say, for in Drs. Moritz and Morris's work we find a technical guide that ranks with the best of those written on any subject, and we feel sure that it will assist in a marked way in spreading a more general knowledge of the real principles of the brewing industry. One aim of the authors has been, not only to lay before their readers the present state of scientific knowledge with regard to brewing, but also by their experience as brewers to draw practical deductions from this knowledge. This part of their subject they have approached in a very fair and impartial spirit, and they have not hesitated to call attention to those points on which knowledge is at present too restricted to justify drawing deductions of any value. Those who are acquainted with the *quasi*-scientific writing that prevails in some of the brewing trade periodicals, will thoroughly appreciate this. We trust that the appearance of Drs. Moritz and Morris's work will raise the general tone of technical brewing literature, an end much to be desired.

In attempting to write a text-book bringing the scientific principles and the practice of brewing together, the authors undertook a difficult task, and one that could only be done by those who have a thorough grasp of both branches of the subject. They have been most successful in their effort, and we commend their work to the notice of all students of brewing, and to all those brewers who take a rational interest in their own business; such cannot fail to derive much benefit from a careful study of it.

A THEORY OF GRAVITATION.

Fresh Light on the Dynamic Action and Ponderosity of Matter. By "Waterdale." (London: Chapman and Hall, 1891).

THE original aim of this work was the discovery of some reason, other than the hypothesis of attraction, to account for the gravitation of one body towards another.

The writer thus takes up the subject of gravitation where it was left by Newton two hundred years ago. The "Principia" concludes with these words—

"Rationem vero harum Gravitatis proprietatum ex Phænomenis nondum potui deducere, et Hypotheses non fingo. Quicquid enim ex Phænomenis non de-

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ducitur, *Hypothesis* vocanda est; et Hypotheses, seu Metaphysicæ, seu Physicæ, seu Qualitatum occultarum seu Mechanicæ, in *Philosophia Experimentali* locum non habent."

"Adjicere jam liceret nonnulla de Spiritu quodam subtilissimo corpora crassa pervadente, et in iisdem latente, cujus vi et actionibus particulæ corporum ad minimas distantias se mutuo attrahunt, et contiguæ factæ cohærent." "Sed hæc paucis exponi non possunt; neque adest sufficiens copia Experimentorum, quibus leges actionum hujus Spiritus accurate determinari et monstrari debent."

Now the present work of "Waterdale" is all Hypothesis from beginning to end; and there is no careful detailed experiment to be found described in the book, by which the various Hypotheses brought forward by him can be tested.

At a first glance the theory seems a revival of the Cartesian Theory of Vortices, advanced in Newton's day by Descartes to account for the motion of the celestial bodies, and the difficulties attending this Theory are pointed out by Newton in his "Scholium Generale"—

"Hypothesis Vorticum multis premitur difficultatibus. Ut Planeta unusquisque radio ad Solem ducto areas describat tempori proportionales, tempora periodica partium Vorticis deberent esse in duplicata ratione distantiarum a Sole," &c.

This ancient theory is attributed by our author to F. Major, in his recent work, "Spacial and Atomic Energy," Parts I. and II.; but the author himself gives, as the primary reason for gravity, the *mutual shelter* to opposite wave-energy afforded by two spheres or bodies; and now, if "Waterdale" is anxious to convert the scientific world, he must utilize the quantitative theoretical results, worked out by Lord Rayleigh, on the Apparent Attraction due to Vibration.

The book abounds with curious unfamiliar dynamical expressions, such as *real*, *vested*, *imposed*, and *specific ponderosity*, *force of diversion*, *rectangular velocity*, *concentering preponderating energy*, *film of transplacement*, &c., of which no definitions are given; and altogether the treatment is unconventional in the extreme.

"Waterdale" concludes by asking that the question of *mechanical perpetual motion* should be reopened, and that pure mathematics should be once more applied to the subject—

"*Perpetual motion* has already been granted to us. By the burning of coal and evaporation of water we have work performed for us by Nature. Perpetuate the process, and the work is also perpetuated. We have many ways of acquiring this gift from Nature's stores, and one more possible method need not startle the human mind."

This method of *quasi*-scientific argument is familiar to us, in the newspapers, in the account and explanation of Spiritualistic Phenomena. A. G. G.

OUR BOOK SHELF.

Indischer Ozean: ein Atlas die Physikalischen Verhältnisse, und die Verkehrs-Strassen darstellend. (Hamburg: Deutsche Seewarte, 1891.)

THESE maps of meteorological and other physical data for the Indian Ocean, while giving a very fair idea of the prevailing conditions, are scarcely equal to the scientific requirements of the present year of grace.

While it may be freely confessed that our knowledge of the area dealt with is yet very imperfect, and that the scale of this handy atlas does not permit of great refinements, there are many details to which exception may fairly be taken. A few instances may be given.

In the map of general depth no indication is furnished of the extreme sparseness of the soundings from which the various coloured areas are drawn.

The current charts are depicted with a hardness and regularity with regard to the different streams that are scarcely consistent with nature. The ever-varying circumstances of the monsoons render the currents of this ocean especially changeable, and it would be preferable to indicate this characteristic by lines more broken. In the sheet of the north-east monsoon period, the meeting of the two main currents on the East African coast never takes place so far south as is shown, nor is there any justification for the peculiar direction of the line between them to the eastward.

It is a bold thing to attempt to portion the sea into areas of definite surface specific gravities. The data are very scanty.

The pressure charts, which are given for the same months as those published by the Meteorological Office, and the map showing the relative prevalence of winds, are good; but here again the absence of the data on which the various quantities in different areas are founded is a serious flaw.

The different maps are well got up, and bear further witness to the general excellence of German lithography.

Mechanics for Beginners. Part I. Dynamics and Statics, By the Rev. J. B. Lock. (London: Macmillan and Co., 1891.)

MR. LOCK states that the work before us contains the more elementary parts of the dynamics of a particle and of the statics of parallel forces, arranged with some additions from his "Elementary Dynamics and Statics." The author's mode of treatment will be familiar to many of our readers, and we need hardly say that Mr. Lock slurs over no difficulty that presents itself to the young student of this difficult subject. We have read the whole of the text with much interest, and pronounce it to be excellent. A boy who has gone through this, and worked out the examples in the manner shown him by the author, will be well equipped for more advanced treatises. A novelty, to which Mr. Lock draws attention, is a new form of "that proof of the formula of accelerated motion which depends upon the idea of average velocity." This proof appears to be a satisfactory one. There is an interesting combination of Morin's and Atwood's machines, which is likely to furnish a useful illustration to students. The work is split up into eight sections—rectilinear motion, motion in one plane, forces acting at a point, parallel forces, machines, uniform motion in a circle, energy, and the pendulum. The arrangement has been made to meet the special wants of the Science and Art Department. It is suited for any junior students. Article 16 appears to us to be likely to be too difficult for a boy; if so, he can pass on, and return to it subsequently. We have not worked out the examples which accompany the several chapters, and to which answers are given at the end. The following errors we have noted: p. 17, l. 8 up, dele a in anT; p. 19, l. 5 up, the first 2 N's should be N′; p. 20, last line, for 252 read 162; p. 52, l. 1, it would seem to follow that "when one mass meets another mass of the *same* velocity" they would not be said to *impinge*. What would they be said to do? P. 67, the term *resolute* is defined, reference might be made to p. 92; p. 68, l. 1, dele a; p. 72, l. 8 up, for Q read H; p. 94, l. 7, for an = read +; p. 115, l. 7 up, read $10-x$; p. 139, l. 2, for 4 read 3; p. 205, last line, numerator, for cos a read sin a; p. 208, l. 20, supply g; p. 246, l. 8 up, for second g read g_1. The greater number of these errors are trifling, and will not give the private student much trouble; we have pointed

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them out because we know what a stumbling-block even slight mistakes are to such students. Their reverence for printed results is often wonderful. The utility of Mr. Lock's "Higher Trigonometry" is greatly hindered by the number of typographical blunders.

The Physical Geology and Geography of Ireland. By Edward Hull, F.R.S. Second Edition. (London: Edward Stanford, 1891.)

THE first edition of this book was reviewed in NATURE rather more than thirteen years ago (vol. xviii. p. 354). Of the second edition, which will be welcomed by all students of the subjects it deals with, we need only say that Prof. Hull has embodied in it the additions which have lately been made to our knowledge of the geological structure of Ireland. The more important of these additions he sums up under the following heads:—(1) The determination of the occurrence of Archæan rocks in certain districts of the west and north of Ireland. (2) The determination of the peculiar relations subsisting between the Lower Devonian (or Devono-Silurian) strata and the Upper Old Red Sandstone and Carboniferous series of the southern districts. (3) Additional evidence regarding the relative ages of the trachytic and basaltic lavas of Antrim. (4) Evidence of the invasion of Ulster by a great ice-sheet from the Grampian Mountains of Scotland during the earliest stage of the Glacial period.

The Ouse. By A. J. Foster, M.A. (London: Society for Promoting Christian Knowledge, 1891.)

IN this little book the course of the Ouse is traced from its source to the point where it enters the sea, and some account is given of the various elements of interest that are to be met with on the way. The idea is good, and the author has worked it out skilfully. Any boys or girls who may read the volume will find at the close that they have obtained from it much sound geographical knowledge.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

A Difficulty in Weismannism.

IN your number of October 29 (vol. xliv. p. 613), Prof. Hartog depicts a dilemma in which a study of Weismann's theories has placed him.

Prof. Hartog sums up the main points of Weismann's theories in five theses, but, considering the great importance which the latter attaches to the operation of *natural selection*, he might well have added a *sixth* to the list.

There can be no doubt that, of the two hypotheses brought forward in the letter, hypothesis B is the one adopted by Weismann for the explanation of the problems of heredity. We are therefore not concerned with hypothesis A.

"According to hypothesis B," Prof. Hartog states, "the Ahnenplasmas of all Metazoa being similar and Protozoan, if the *numbers are equal* and the shuffling fair, any two parents may beget any offspring whatever; . . . a lioness might be expected to bring forth a lobster or a starfish, &c."

What does Prof. Hartog mean by *fair shuffling?* Surely not such shuffling as is resorted to in the game of whist, but such shuffling as he himself describes in thesis 4. He states here that the "process is comparable to the shuffling of two packs of cards by taking half from each and joining the talons or remainders to form a new pack."

It surely cannot be imagined that Weismann ever intended to assert that with each sexual act there was a rearrangement of the Ahnenplasmas comparable to the shuffling of a pack of cards during the game of whist.

Did he anywhere asert this, we should naturally expect him to believe that a lioness might as well bring forth human beings or lobsters as normal cubs.

With the evolution of sexuality, the excessively numerous Ahnenplasmas of our variable Protozoan ancestors became *arranged* in more and more complex, ever-varying *combinations*. At the very outset, natural selection operated. The variations (due to the combinations) most advantageous to the species were perpetuated. Unfavourable variations involved extinction. It is the *special combination* of the units of ancestral germ-plasm which predetermines the structure of the mature individual. This combination, of course, is very closely related to the two combinations from which it arose, and it is just this closeness of relationship which prevents us from supposing that a lioness can ever produce anything but cubs. Changes in the combinations are only slowly effected. The influence of the mother is due to the fact that one-half of the maternal combination is present in the offspring, and similar statements can of course be made concerning the influences of father, grandfather, great-grandfather, &c.

Do not these two considerations—(1) that the nature of the individual depends upon the *peculiar combination* of units of ancestral Protozoan germ-plasm, a combination very closely related to two previous ones (owing to the fact that, in sexual union, two halves of immediately preceding combinations are united to make one whole); (2) that the operation of natural selection provides for the *extinction* of *useless*, and the *preservation* of *useful* variations—afford to Prof. Hartog the means of escaping from his dilemma?

A. H. TROW.

Penarth, Cardiff, November 14.

THE contributions of Mr. Trow and Dr. Poulton to this discussion render necessary an explanation that should, perhaps, have accompanied my first letter. After rough-drafting this, I felt misgivings lest I might have misconceived Weismann's meaning, and set up a man of straw to knock down. Accordingly, I wrote to Prof. Weismann to ask if I rightly understood his meaning, explaining my object in doing so; and he answered my queries with great kindness, courtesy, and fulness. As I wrote back to him, I then thought it better, relieved from my misgivings, to state the point without reference to his letter. But Mr. Trow and Dr. Poulton have both blamed my use of the word shuffling, and appear to think that my hypothesis A is a purely imaginary conception of the straw man order. I hope, therefore, I shall not be accused of having wilfully kept a trump card up my sleeve if I now quote the two essential passages of Prof. Weismann's letter, which were written in definition of the points at issue.

"Ich denke mir dass das Keimplasma eines Individuum's aus einer gewissen Zahl von Einheiten besteht, welche untereinander sehr ähnlich, aber nicht gleich sind. Die Unterschiede zwischen ihnen entsprechen meist den Unterschieden zwischen je zwei Individuen derselben Species. *Jedes derselben würde im Stande sein ein Individuum der Art hervorzubringen falls es sich zu der dazu nöthigen Masse vervielfältigen könnte oder würde.*" The sentence I have italicized corresponds, I think, very fairly to my hypothesis A: "Each Ahnenplasma unit corresponds to an individual of the species itself; and if put under suitable trophic conditions would, singly, reproduce such an individual." Dr. Poulton writes: "I agree with Prof. Hartog in considering it [Hypothesis A] as valueless." I am far from considering any hypothesis as valueless which upsets a wrong theory of which it should be the mainstay.

Prof. Weismann goes on: "Sie können ganz wohl die geschlechtliche Fortpflanzung mit dem Mischen eines Kartenspiels vergleichen, aus dem immer die Hälfte der Karten entfernt wird. Nur ist nicht zu vergessen dass die Karten selbst nicht völlig unveränderlich sind." It is obvious that Prof. Weismann accepts the peculiar mode of shuffling I have described (not the ordinary mode at whist), as a fair illustration of his conception of fertilization and its antecedents. He always speaks of *combinations* in his "Essays," and not *permutations*. The reason is obvious: the figured elements of the living nucleus are constantly changing their relative position; and it is these that are the outward and visible sign of the mysterious ancestral units.

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Hence Dr. Poulton's very pretty kaleidoscope simile involves new suppositions, which are worse than gratuitous because they involve throwing overboard the very facts on which the theory was originally based. It is plain that Dr. Weismann goes very much further in admitting the changeability of the ancestral units than his disciples are willing to do; and I have shown that hypothesis A involves the conclusion that these are indefinitely changeable, not merely "not completely unchangeable," as Prof. Weismann writes.

Another point for consideration is that we can hardly doubt the monophyletic origin of Metazoa, and that, at least excepting Cœlenterates and Sponges, they all originated from some one primitive form. The Protozoan ancestors of this form must have belonged to the same species with one another, and their representative ancestral units cannot have been more different than the members of a single species. Hence, without selection, the germ-plasm composed of a number of these units associated together would give an average resultant, so that the majority of individuals would be more similar than the ancestral units of their germ-plasms, and amphigony would produce uniform offspring on the whole. Divergence from the average type could only occur by the duplication or further repetition of single ancestral units of special character; and these variations would be the material for natural selection to act upon. Thus, among words of eleven letters, such a word as *abracadabra*, with its 5 *a*'s, 2 *b*'s, and 2 *r*'s, would have a marked divergence from the type as compared with groups in which no letter occurred twice over.[1] If, then, natural selection goes on to form a species according to Weismann's theory, it can only do so by eliminating certain ancestral plasms and duplicating or further repeating others to take their place. Once an ancestral plasm eliminated in the formation of a race it can never be reintroduced, or replaced by a new one. But as soon as we repeat certain members of a group of limited number we reduce the possible number of permutations or combinations that can be formed from that group. Anyone with a fair head for the work, and a Todhunter's "Algebra," can see for himself how very rapidly the number of combinations is reduced in this way. Thus natural selection could only result in arrangements of ever-increasing simplicity and similarity instead of complexity and divergence. The ultimate product would be a limited number of well-marked species, whose individual members had lost all power of variation. This I offer as an alternative to the variable offspring of the lioness.

Mr. Trow is extremely anxious to show me a path out of my dilemma. It presents no difficulty to those biologists who consider the conception of a germ-plasm independent of the somato-plasm as more or less mythical. For those who follow Weismann, the way out of the difficulty will not lie through the ascription to natural selection of powers which it cannot possibly exert.

MARCUS HARTOG.

Cork, November 28.

[1] The argument above was suggested to me by a chemical friend.

The Mexican Atlatl or Spear-Thrower.

THE note in NATURE of November 19 (p. 66) recording the important discovery at Lake Patzcuaro, Mexico, of "a modern atlatl (not altatl, as misprinted) well worn and old-looking, accompanied with a gig for killing ducks," is very interesting. It may not be out of place to call attention to an exhaustive little memoir by Mrs. Zelia Nuttall on "The Atlatl or Spear-Thrower of the Ancient Mexicans," published this summer in the third number of the first volume of the "Ethnographical and Ethnological Papers of the Peabody Museum" (Cambridge, Mass., 1891). In this paper, which is illustrated with eighty figures of different kinds of atlatl, the author completely establishes the existence and practical use in warfare of the wooden spear-thrower or atlatl by the Mexicans at the time of the Spanish conquest, although some doubt had been expressed in the matter by such well-known authorities as Prof. E. B. Tylor and Mr. A. Bandelier, while Mr. H. H. Bancroft even stated that "he had not found any description of its form or the manner of using it." Mrs. Nuttall, however, reproduces numerous illustrations of the many varied forms of the atlatl from different *codices*, accompanied by several descriptions of the manner of hurling the weapon, cited from old Spanish writers. Perhaps at this moment the most *à propos* is that from the ancient chronicles of Tezozomoc, who, in describing the drill of the soldiers, relates "how their chiefs ordered them out in canoes to practise throwing spears at flying ducks before engaging the enemy in warfare." Mrs. Nuttall was enabled to trace, by means of a careful study of a MS. edition of "Sahagun's Historia," preserved in the National Library at Florence, the complete evolution of the atlatl from the simple form used by the native hunter to launch the harpoon with two or three barbs at the fish or water-fowl of the lagoons. This had a cord attached to retrieve the game. "Minus the cord, the spear-thrower became part of the necessary equipment of every soldier of a certain grade," and was used with fatal effect, as Bernal Diaz most distinctly states, in opposing the advance of the Spanish adventurers. Elaborately decorated forms first became the emblem of chieftainship, and ultimately symbolic of the Aztec deities, and were borne aloft by the chief-priestly warrior and representative of the gods in ceremonial processions. The maximum of development was attained in the symbolic "blue atlatl" or "blue serpents," inlaid with gold and richly decorated with feather-work, described as "bishops' crosiers" by Cortes, who sent specimens presented to him by Montezuma II. to the Court of Spain. Some examples are still preserved in the Ethnographical Museums of Berlin and Vienna, and in the British Museum.

It was in the course of these researches that Mrs. Nuttall made the important identification of the atlatl "as the hitherto unrecognized weapon" grasped by the warriors sculptured on the "so-called sacrificial stone of Mexico," and also by the warriors depicted in Stephens's illustrations of the *bas-reliefs* adorning the ruins at Chichen-Itza in Yucatan. The different myths relating the invention or origin of the *atlatl* are collected and explained, and the following very practical philological derivation of the name *atlatl* is offered by her as a suggestion supported by a series of careful analyses:—

"The Aztec word *atlatl*, or *atlatli*, is intimately connected with the verb *tlaça* = to aim, to throw, or cast. From this verb a whole series of words is formed, as *tlatlacaliztli* = the act of throwing, &c.; *tlatlaxtli* = the object thrown; *tlatlçani* = thrower. The name atlacatl—a synthesis of *âtl*, water, *tlacatl*, men—was applied to the fishermen, the original users of the atlatl; and it is suggested that the word atlatl may primarily have been a synthesis formed with the verbal noun *tlatlaçani* = thrower, and *âtl*, water, which would give the word *atlatlaçani*, meaning water-thrower, not an unfit name for the harpoon-thrower of the watermen" (p. 12).

This interpretation is certainly not weakened by the recent discovery that the primitive form of atlatl is still in use in the lake regions of Mexico. In other respects Mrs. Nuttall's paper well repays perusal by all interested in Mexican antiquities.

A word with reference to Prof. Otis Mason's remark "that the problem now is to connect Alaska with Mexico." Given hungry aboriginal man in the foreground, and fat wild ducks in what artists term "the middle distance," it does not seem wholly irrational to surmise that the *atlatl*, or spear-thrower, was independently evolved in suitable environments. Does not the average nineteenth-century boy still betray a strong innate tendency to throw or sling stones at every bird he sees? Perhaps this is but accumulated inherited instinct, not yet eradicated by civilization. It is at all events certain that the *atlatl* was widely used by the aboriginal inhabitants of the American continents, as Prof. Max Uhle's researches testify abundantly.

Brighton, November 21. AGNES CRANE.

The Chromosphere Line Ångström 6676·9.

WITH regard to Prof. Young's observations as to the non-coincidence of the bright chromosphere line (NATURE, November 12, p. 28) with the corresponding dark line 6676·9 of Ångström's scale, it may be interesting to note that Profs. Liveing and Dewar have observed a barium line at 6677, which is therefore slightly less refrangible than the dark solar line. In his catalogue Prof. Young also gives a barium line at 6018·0, which is identified with Kirchhoff 933·8. In the course of the observations of sun-spot spectra taken at Stonyhurst with a twelve-prism spectroscope, no dark solar line has been noted in this position except in two uncertain instances over spots. It would be an important fact should two barium lines be found in the chromosphere without corresponding dark lines.

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In the period of maximum solar activity the bright line 6676·9 was on several occasions seen in the spectroscope, while the height of the chromosphere was being measured at Stonyhurst on the C line of hydrogen. At these times C was always very bright, and generally displaced in the prominences in which 6676·9 was seen. The latter line was not seen in the observations taken between March 9, 1886, and September 10, 1891. Although both Young and Thollon attribute the line to iron, no iron line is given in this position by either Ångström or the catalogues of the British Association. Dunér, quoted by Thollon, considers the line variable with the state of solar activity, but Ångström seems to have made an error in drawing it as a fine thin line, as Kirchhoff, Burton, Fievez, Smyth, Thollon, and Higgs give it as a strong dark line. Finally, Young, Burton, and the Stonyhurst observers identify it with Kirchhoff's ray 654·3, and Thollon with 641, which latter is a calcium line. There would, then, appear to be some differences of opinion with regard to this important line (cf. *Monthly Notices R.A.S.*, vol. li., No. 1, p. 22.) A. L. CORTIE.

St. Beuno's College, St. Asaph, November 19.

Peculiar Eyes.

I LABOUR under the peculiar inconvenience of having a right eye of normal power and a short-sighted left eye. The numerals on the face of a clock ⅛ of an inch high are visible to the right eye at 12 feet distant; but in order to discern them as clearly with my left eye I require to bring that organ of vision as near to the figures as 8 inches. On looking at my gold chain hanging on my breast in daylight and with both eyes, the chain, coloured yellow and towards the left, is perceived by the right eye, while a steely blue chain, another, yet the same, is perceived about an inch to the right and a little higher up. By artificial light the same phenomenon presents itself, but the difference of colour is not so apparent; the yellow to the right is only dimmer. Again, when a page of NATURE is being read with the short-sighted eye, there appears, about an inch to the left, part of the same column, small, and the black, under artificial light, like weak purple. The right-hand side of this ghost-like column is lost to the right eye, being commingled with the larger, darker letters seen by the short-sighted left, which cover it like the more recent writing on a palimpsest. Middle life was reached before the discovery was made. These experiences must be gone through with intent, for objects generally being perceived altogether with the right eye, all that the left seems good for is to supply a little more light. The perception of the difference of colour is as good with the one eye as the other, and the short-sighted eye can read smaller type.

As the inferior animals, so far as I know, have no habit of peeping or looking with one eye shut and the other open, it occurred to me that this ability might be a limited one. I tried the experiment with school children, and to my surprise found that a few were quite unable to keep one eye shut and the other open at the same time, and a few did it with an effort, making in all about a fourth of the number. Adults were likewise under similar limits, but to a less extent. This may be the reason why the discovery of inequality of vision, as Sir John Herschel remarks, is often made late in life. Indeed, he mentions an elderly person who made the unpleasant discovery that he was altogether blind of an eye. JAS. SHAW.

Tynron, Dumfriesshire.

Zoological Regions.

THE last number of the *Archiv für Naturgeschichte*, lvii., which has just appeared, contains (pp. 277–291, pl. x.) an article by Prof. Möbius, dealing with the zoological regions of the earth, chiefly with a cartographical and "museological" object, in which a set of regions is proposed differing in some respects from that most generally in use. The number of land regions is raised to twelve instead of the usual five or six, and the marine world is likewise subdivided into a number of regions. A part of what may appear innovations is in fact nothing but a reversion to the zoological subdivisions of the world proposed by Schmarda ("Geographische Verbreitung der Thiere") in 1853. It seems extraordinary that, although alluding to the works of the principal authorities who have dealt with zoogeography since Schmarda, Prof. Möbius should not have referred to that author otherwise than in a second-hand quotation. For not only did Schmarda lay down the basis on which zoological regions have since been elaborated, but his attempt is, everything considered, in many respects superior to that of his immediate successors in the same field.

It will be seen, on comparing Schmarda's and Möbius's maps, or the table annexed to this note, that several of the regions independently proposed by these authors coincide in their limits, the principal difference being that Schmarda divided the world into a greater number of "Reiche," some of which are merely amalgamated in Möbius's "Gebiete."

G. A. BOULENGER.

SCHMARDA, 1853.		MÖBIUS, 1891.
A. *Festland.*	=	A. *Landgebiete.*
I. Arctisches Reich ..	=	I. Nordpolar Gebiet.
II. Mittel-Europa	=	II. Europäisch-Sibirisches G. (+ part of Schmarda's I. R.)
V. Mittelmeer Reich ...		
III. Kaspische Steppenländer	=	III. Mittelmeer G.
IX. Wüste		
IV. Centralasiatische Steppen		
VI. China	=	IV. Chinesisches G.
VII. Japan		
VIII. Nordamerica	=	X. Nordamerikanisches G.
X. Westafrika	=	VI. Afrikanisches G.
XI. Hochafrika		
XII Madagascar	=	VII. Madagassiches G.
XIII. Indien	=	V. Indisches G.
XIV. Sunda-Welt		
XV. Australisches Reich ..	=	VIII. Australisches G.
XVI. American. Mittelreich		
XVII. Brasilien		
XVIII. Andо-peruan.-chil. R.	=	XI. Südamerikanisches G.
XIX. Pampas		
XX. Patagonien		
XXI. Polinesien	=	VIII. (Part). IX. Neuseeländisches G.
B. *Meere.*	=	*B. Meergebiete.*
XXII. Arctisches M.	=	I. Nordpolar M.
XXIII. Antarctisches M. ...		
XXX. Südl. Atlant. Oc. ...	=	VIII. Süd-M.
XXXI. Südl. Stiller Oc ...		
XXIV. Nördl. Atlant. Oc. ...	=	II. Nordatlant. M.
XXV. S. Eur. Mittel-M. ...	=	III. Mittel-M.
XXVI. Nördl. Stiller Oc. ...	=	VII. Nordpacifisches M.
XXVII. Trop. Atlant. Oc. ...	=	IV. Südatlantisches M.
XXVIII. Indischer Oc.	=	V. Indisch-Polynesisches M.
XXIX. Trop. Stiller Oc. ...		VI. Peruanisches M.

Scientific Nomenclature.

A propos of Prof. Parker's interesting article on scientific nomenclature in your issue of the 19th inst. (p. 68), I should like to call attention to the misuse of the term involucre in regard to the Anemone, &c. The so-called involucre of the Anemone is really, morphologically, a calyx, and until the flower-bud has grown to the height of an inch or two from the ground, it to a certain extent performs the ordinary functions of a calyx. Then an internode is developed between the calyx and corolla. But the presence of this internode, long as it is, should no more prevent our assigning to the calyx its proper name, than does the slight internode existing between the calyx and corolla of *Lychnis diurna*.

Great Malvern. H. ST. A. ALDER.

"The Darwinian Society."

I WOULD call Mr. White's attention to the fact that the name of this Society is not "The Darwinian Society," but "The Edinburgh University Darwinian Society"—a name which, considering Darwin's connection with the University and with a similar Society here, I think we are quite entitled to assume.

JOHN S. FLETT,
Secretary.

University of Edinburgh,
November 24.

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SOME NOTES ON THE FRANKFORT INTERNATIONAL ELECTRICAL EXHIBITION.[1]

VI.

The Frankfort Motor and the Lauffen Dynamo.

IN all the motors described in Part V. the magnetism of the stationary iron ring is being rapidly reversed, while that of the moving interior varies at a rate which is equal to only the difference between the speeds of the rotatory magnetic field and the rotating central portion of the motor. This difference is always comparatively small in a well-designed multiphase motor, even when loaded, and becomes practically nought for light loads. Hence we may regard a multiphase motor as roughly one in which the magnetism of the rotating iron interior remains unchanged relatively to the iron itself, while that of the stationary iron ring varies rapidly.

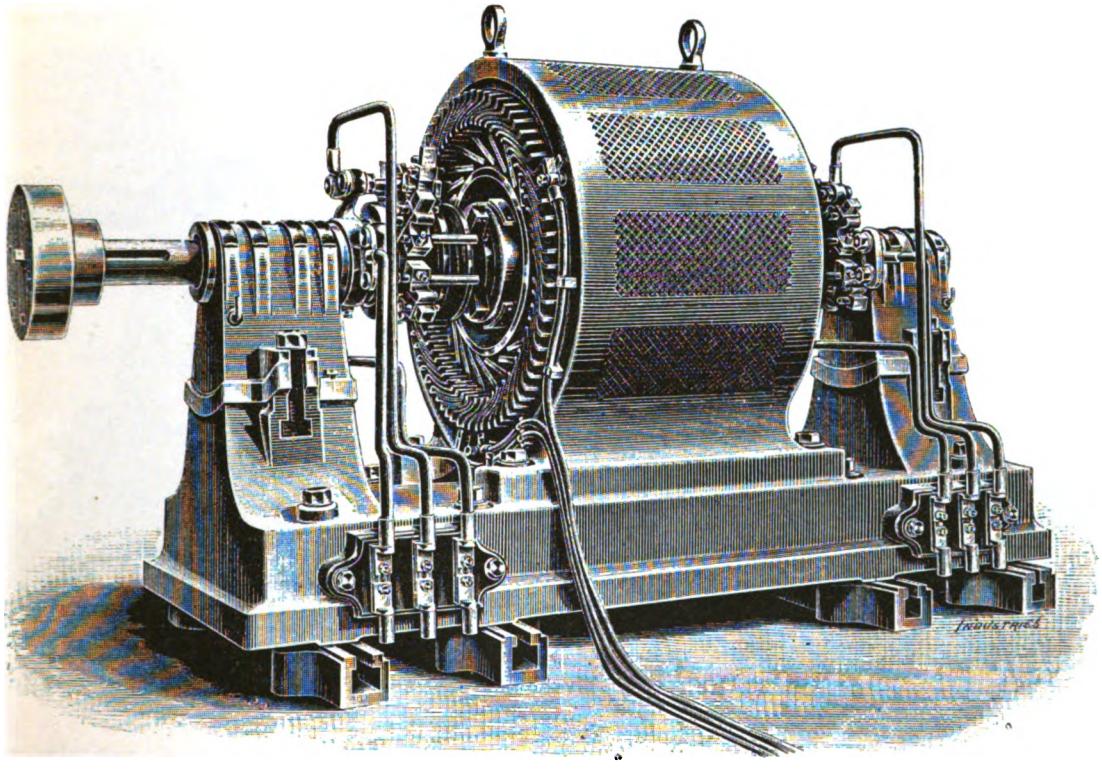

FIG. 32.—The Frankfort 100 horse-power rotatory current motor.

Now whenever the magnetism of iron is being rapidly reversed, there is loss of power; the magnitude of the loss, for a given rate of reversal and quality of iron, depending on the intensity of the magnetization and the mass of the iron acted on. And, as the length of the lines of force in the outer stationary iron ring of the multiphase motor is necessarily longer than in the interior rotating portion, Mr. Dobrowolski concluded that less power would be wasted if he inverted the functions of the stationary and rotating parts, sending the multiphase alternate currents round the interior rotating laminated iron drum, and attaching the short-circuited conductors, in which currents are induced by the rotating magnetic field, to the *inside* of the stationary laminated iron ring, so as to form a kind of short-circuited Siemens armature turned inside out.

The preceding conclusion is undoubtedly correct if it be certain that *the smaller density of the lines of force in the stationary ring do not more than compensate for the increased length of their path*, a point to which we venture to think Mr. Dobrowolski has not given sufficient attention when coming to this decision to turn the multiphase motor inside out.

However, be that as it may, Fig. 32 shows the multiphase motor of 100 horse-power nominal, which the considerations described in this and the previous article led Mr. Dobrowolski to construct for being worked at Frankfort by a portion of the power generated at Lauffen 109 miles away.

To lead the currents coming along the mains to the rotating interior of the motor, three rubbing contacts must be employed; indeed, six contacts become necessary if we desire to be able to couple up the coils on the motor in open or closed circuits (Figs. 20 and 24, pp. 56 and 57)—an arrangement provided for in the Frankfort motor (Fig. 32). The great simplicity of the original Ferraris' motor is thus abandoned, but, although this would be disadvantageous in the case of small motors, where simplicity and freedom from sparking are all-important, the change is not so serious in a large motor, like that shown in Fig. 32, since high efficiency, and therefore small waste of power, combined with relatively small first cost, are the main things to be aimed at in large motors.

Another reason why the application of rubbing contacts to a large rotatory-field motor is less objectionable than might at first sight appear arises from the fact that, even if the motor were constructed in one of the original forms shown in the preceding article, it would be necessary to employ rubbing contacts for a totally different reason, viz. to introduce resistance, at the moment of starting the motor, into the circuits which carry the currents induced by the rotating magnetic field. Whereas, if these currents be induced in conductors attached to the stationary ring,

[1] Concluded from p. 60.

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such a resistance can be introduced without extra rubbing contacts; and it is for the purpose of introducing this resistance into these stationary circuits that the three wires, trailing on the ground in Fig. 32, are seen attached to the conductors attached inside the stationary part of the motor.

The necessity, at starting the motor, for increasing the resistance of the conductors carrying the induced currents will appear from the following consideration. When the motor (Fig. 32) is running at full speed under a light load, the interior part rotates at such a rate—relatively to the frequency of alternation of the currents in the main wires—that the magnetic field is practically stationary, just as it is in an ordinary direct current motor. But at the start, the interior laminated iron drum is only moving slowly, while the currents flowing in the conductors attached to it are alternating rapidly, hence the magnetic field is rotating rapidly, and powerful currents are induced in the stationary conductors, so powerful, in fact, as to produce a magnetic field which seriously distorts that produced by the main alternating currents. In fact, there is the same antagonism of magnetic fields that occurs with a direct current motor, if the armature field be very powerful in comparison with that of the field magnet, and if the lead of the brushes be adjusted so as to cause the fields to oppose one another; and it is to avoid this result that M. Dobrowolski introduces a liquid resistance into the circuits of the stationary conductors of his large motors while the motor is getting up speed.

We have hitherto spoken of the conductors on the rotating part as being wound on the outside of a laminated iron drum, and those on the stationary part as being wound on the inner surface of a laminated iron ring; but, as a matter of fact, in the large Frankfort motor both sets are composed of copper rods, insulated in asbestos tubes, and slipped into holes punched out of the iron close to the periphery. This burying of the copper bars to a small depth inside the iron has been adopted because it has been found that the generation of Foucault currents in the thick bars can in this way be more effectively prevented than by following the method usually adopted with bar armatures, which consists in moulding each conductor out of stranded copper wire with the various wires composing the strand partially insulated from one another.

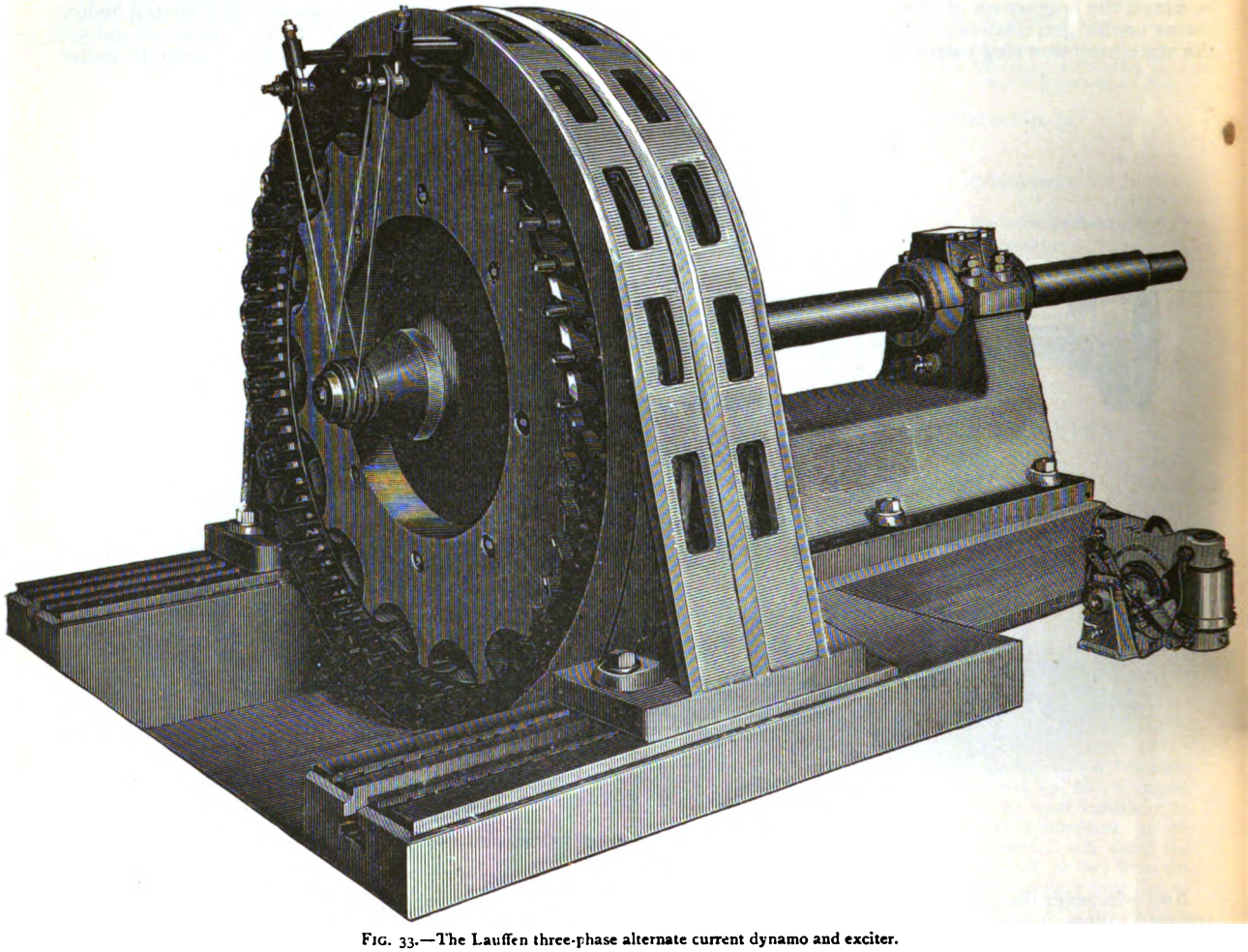

FIG. 33.—The Lauffen three-phase alternate current dynamo and exciter.

No tests have yet been published of the power and efficiency of this machine, but the smoothness with which it ran, pumping up water for the artificial waterfall in the Frankfort Exhibition, and the absence of the roar audible with some alternate current machines, and even of the rhythmical hum noticeable with the best alternate current motors, were very striking.

In the last article it was proved that if three harmonic alternating currents of the same periodic time and maximum amplitude, but differing by 120° in phase, flowed in

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three wires, A, B, C (Figs. 20, 21, 22), each current was at any moment algebraically equal to the sum of the other two. To test, therefore, whether the currents flowing in the three parallel wires between Lauffen and Frankfort fulfilled this condition, we had merely to find out whether any current was induced in a neighbouring telegraph wire which was sufficiently far away as to be practically at the same distance from each of the Lauffen-Frankfort wires.

Between Frankfort and Hanau the power wires are carried on one side of a broad railway, and for some eight or nine miles the telegraph wires run on the other side; the telegraph wires for the remainder of the distance between Frankfort and Hanau following quite a different route. If one of these telegraph wires were put to earth at Frankfort and at Hanau, and if a telephone were placed in the circuit, a confused chattering of telegraph instruments was always heard in this telephone, due to induction from the telegraph lines on the same posts. But during the hours that power was being transmitted

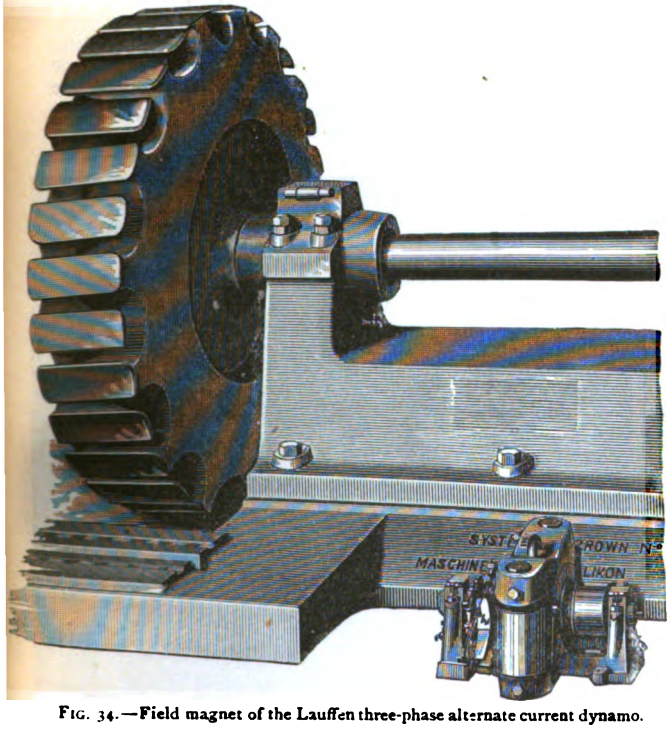

FIG. 34.—Field magnet of the Lauffen three-phase alternate current dynamo.

through the wires on the other side of the broad railway a rhythmical hum could be detected superimposed on the confused babel of telegraph signals, proving that the three alternate currents were either not truly sine currents, or that their phase difference was not accurately 120°.

To generate the three-phase current at Lauffen, the extremely compact dynamo shown in Fig. 33 was designed by Mr. Brown, and constructed at the Oerlikon Works, near Zurich. The armature is wound with three distinct circuits, each arranged to give 1400 amperes at a potential difference of 50 volts, so that the dynamo can develop 300 horse-power. To avoid, as far as possible, rubbing contacts, the armature remains stationary and the field magnet revolves; while by the employment of 32 poles a frequency of 40 complete alternations per second can be obtained in each circuit when the field magnet only makes 150 revolutions per minute.

For examining the interior, the armature, which forms the outside shell of the machine, can be withdrawn sideways, leaving the field magnet in position, as seen in Fig. 34. Each of the 32 flat-looking plates round the circumference of the field-magnet is a magnetic pole, the poles being alternately north and south. This result is attained by constructing the field magnet in the ingenious manner shown in Fig. 35, the coil which carries the direct current to magnetize this field magnet being wound in the circumferential channel seen in section in Fig. 35.

The armature bars, 96 in number, are constructed of copper rods 29 mm. in diameter, insulated in asbestos tubes, and slipped through holes (parallel to the axis of rotation) punched out of the laminated iron ring which composes the armature core; this burying of the con-

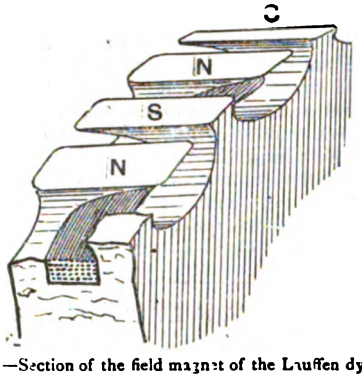

FIG. 35.—Section of the field magnet of the Lauffen dynamo.

ductors to a small depth in the iron being, as already explained in the case of the Dobrowolski motor, for the purpose of avoiding Foucault currents being induced in the thick copper bars.

A portion of the three separate windings, *a a a a*, *b b b b*, *c c c c*, on the armature is shown in Fig. 36, which represents a bit of the circumferential part laid out flat; the dotted rectangles indicate the poles, and to avoid confusion the armature bars, parallel to the length of the poles, are drawn longer in proportion than they really are.

In order that the electromotive forces induced in all the up and down bars of any one of the windings *a a a a* in Fig. 36 should help one another, the distance between

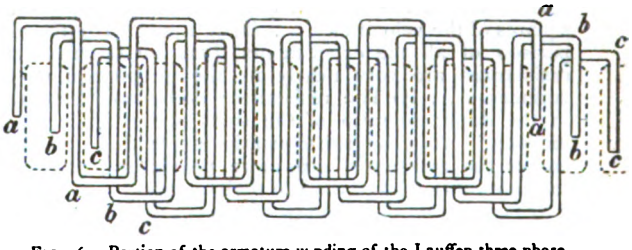

FIG. 36.—Portion of the armature-winding of the Lauffen three-phase alternate current dynamo.

any up and the adjacent down conductor of the same winding must be equal to the distance between two adjacent poles—that is, to 1/32 of the circumference of the armature; and in order that the electromotive force generated by the winding *b b b b* should differ in phase by 120° from the electromotive force generated in the winding *a a a a*, the distance between an up bar of the winding *a a a a* and the following up bar of the winding *b b b b* must be two-thirds of the distance between the centres of two adjacent poles—that is, must be 1/48 of the circumference of the armature. Similarly, an up bar of the winding *c c c c* must be behind the preceding up bar of the winding *c c c c* by 1/48 of the circumference of the armature.

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The exciting current is led into the field magnet by the novel employment of the two endless metallic cords seen to the left of Fig. 33, which saves the necessity of using a standard to carry contact brushes, and the smallness of the power spent in exciting the field magnet, compared with the power developed by the machine, is seen from the dwarf-like character of the direct current exciting dynamo in Figs. 33 and 34.[1]

This three-phase alternate current dynamo of Mr. Brown's, on account of the simplicity and solidity of its design, the slow speed of its rotation, and the entire absence of the experimental makeshifts which are supposed to be characteristic of an electrician, but which are in reality evidences of the rapid development of his tools, appeals especially to the mechanical engineer. It is therefore probable that the employment of so well constructed a dynamo at Lauffen, and so smoothly running a motor at Frankfort, will bring home to the mechanical engineer that he can now avail himself, for the practical transmission of power, of that silent carrier electricity—a carrier which, while it can communicate a great force almost instantaneously to a vast distance through a thin wire, travels itself so leisurely that, in its steady flow, it experiences no extra difficulty whether it goes up hill or down dale, overhead or underground, in a straight line or round a succession of sharp corners.

EXPERIMENTS IN AËRODYNAMICS.[2]

THE subject of this memoir is of especial interest at the present time, when the skill of a distinguished inventor is understood to be engaged in attacking the many practical difficulties which lie in the way of artificial flight upon a large scale. For a long time the resistance of fluids formed an unsatisfactory chapter in our treatises on hydrodynamics. According to the early suggestions of Newton, the resistances are (1) proportional to the surfaces of the solid bodies acted upon, to the densities of the fluids, and to the squares of the velocities; while (2) "the direct impulse of a fluid on a plane surface is to its absolute oblique impulse on the same surface as the square of the radius to the square of the sine of the angle of incidence." The author of the work[3] from which these words are quoted, in comparing the above statements with the experimental results available in his time (1822), remarks:—"(1) It is very consonant to experiment that the resistances are proportional to the squares of the velocities. . . . (2) It appears from a comparison of all the experiments, that the impulses and resistances are very nearly in the proportion of the surfaces. . . . (3) The resistances do by no means vary in the duplicate ratio of the sines of the angle of incidence." And he subsequently states that for small angles the resistances are more nearly proportional to the sines of incidence than to their squares.

It is probable that the law of velocity tended to support in men's minds the law of the square of the sine. For, if both be admitted, it follows that the resistance, normal to the surface, experienced by a plane when immersed in a stream of fluid, depends only upon the *component* of the velocity perpendicular to the surface. That the effect should be independent of the component parallel to the plane seems plausible, inasmuch as this component, *if it existed alone*, would exercise no pressure; but that such a view is entirely erroneous has been long recognized by practical men, especially by those concerned in navigation.

From the law of the simple sine, enunciated by Robison, it follows at once that the pressure upon a lamina exposed perpendicularly to a stream may be increased *to any extent* by imparting to the lamina a sufficiently high velocity *in its own plane*. The immense importance of this principle was clearly recognized by Mr. Wenham in his valuable paper upon flight;[1] and a few years later the whole subject was discussed by the greatest authority upon such matters, the late Mr. W. Froude, with characteristic insight and lucidity.[2]

The theoretical problem of determining the resistance from the first principles of hydrodynamics is not free from difficulty, even in the case of two dimensions, where a long rectangular lamina is exposed obliquely to a stream whose direction is perpendicular to the longer sides. The formula[3] resulting from the theory of Kirchhoff, viz.

$$\frac{\pi \sin \alpha}{4 + \pi \sin \alpha} \rho V^2, \quad \ldots \ldots \ldots (1)$$

where ρ is the density of the fluid, and V is the total velocity of the stream flowing at the angle α with the plane of the lamina, shows that when α is small the resistance is nearly proportional to sin α. Moreover, (1) agrees with the experiments of Vince.[4]

It will be seen that the laws of resistance were fairly well established many years ago, at least in their main outlines. Nevertheless, there was ample room for the systematic and highly elaborate experiments recorded in the memoir whose title stands at the head of this article.

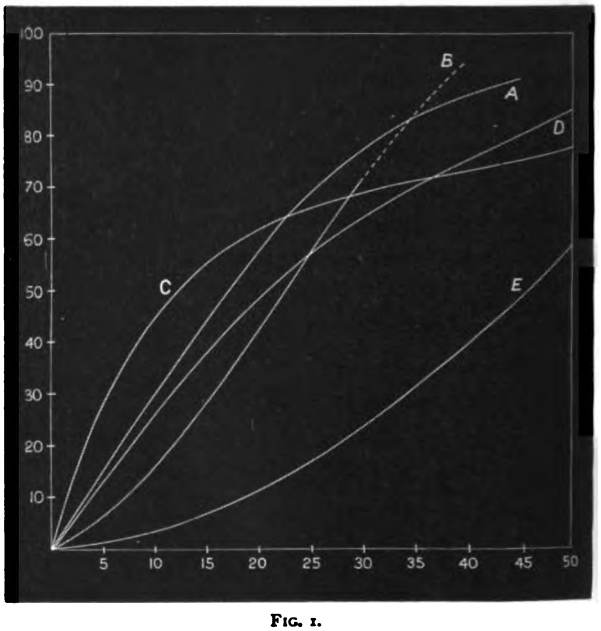

FIG. 1.

The work appears to have been executed with the skill and thoroughness which would naturally be expected of the author, and will doubtless prove of great service to those engaged upon these matters. The scanty reference to previous knowledge, which Prof. Langley holds out some promise of extending in subsequent publications, makes it rather difficult to pick out the points of greatest novelty. The main problem is, of course, the law of obliquity, and this is attacked with two distinct forms of apparatus. The general character of the results, exhibited graphically on p. 62, will be made apparent from the accompanying reproduction, in which are added a curve D,

1 We are indebted to *Industries* and the *Electrician* for some of the illustrations used in this article.

2 "Experiments in Aërodynamics." By S. P. Langley. "Smithsonian Contributions to Knowledge." (Washington, 1891.)

3 "System of Mechanical Philosophy," by John Robison, vol. ii., 1822.

1 Report of Aëronautical Society for 1866.

2 Proc. Inst. Civ. Eng., 1871 (discussion upon a paper by Sir F. Knowles).

3 See *Phil. Mag.*, December 1876. Also Basset's "Hydrodynamics," vol. i. p. 131.

4 Phil. Trans., 1798.

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corresponding to (1), and E, representing the law of $\sin^2 a$. In each case the abscissa is the angle a, and the ordinate is the normal pressure, expressed as a percentage of that experienced when $a = 90°$. Of Prof. Langley's curves, A relates to a *square* plane 12 inches × 12 inches, B to a rectangle 6 × 24 inches, and C to a rectangle 30 × 4·8 inches, the leading edge (perpendicular to the stream) being in each case specified first, so that the theoretical curve D corresponds most nearly to C. It will be seen at a glance that at small angles the pressure is enormously greater than according to the law of $\sin^2 a$. The differences between A, B, C, anticipated in a general manner by Wenham and Froude, are of great interest. They demonstrate that in proportion to area a long narrow wing is more efficient as a support than a short wide one, and that in a very marked degree.

Up to a certain point there is no difficulty in giving a theoretical account of these features. When a rectangular lamina is exposed perpendicularly, there is one point, *i.e.* the centre, at which the velocity of the stream is annulled. At this point the pressure attains the full amount, $\frac{1}{2}\rho V^2$, due to the velocity of the stream, while at every other point the pressure is less, and falls to zero at the boundary. If the lamina is sloped to the stream, as in B and C, there is still a median plane of symmetry; and at one point in this plane, but now in advance of the centre, the full pressure is experienced. In strictness, there is only one point of maximum pressure, whatever may be the proportions of the lamina. But if the rectangle be very elongated, there is practically a great difference in this respect according to the manner of presentation, although the small angle a be preserved unchanged. For when the long edges are perpendicular to the stream (C), the motion is nearly in two dimensions, and the region of nearly maximum pressure extends over most of the length. But the case is obviously quite different when it is the short dimension that is perpendicular to the stream, for then along the greater part of the length there is rapid flow, and consequently small pressure.

It will naturally be asked whether any explanation can be offered of the divergence of C from the theoretical curve D. This is a point well worthy of further experiment. It seems probable that the cause lies in the suction operative, as the result of friction, at the back of the lamina. That the suction is a reality may be proved without much difficulty by using a hollow lamina, AB (Fig. 2), whose interior is connected with a manometer.

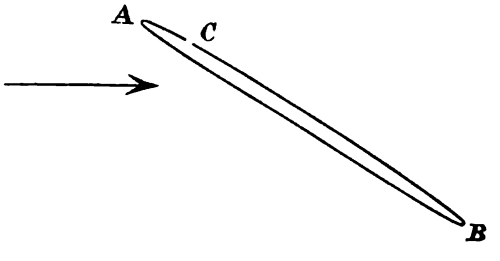

FIG. 2.

If there be a small perforation at any point C, the manometer indicates the pressure, positive or negative, exercised at this point, when the apparatus is exposed to a blast of air.

When once the law of obliquities is known, the problem of aërial maintenance presents no further theoretical difficulty. It was successfully treated many years ago by Penaud,[1] and somewhat later by Froude, whose interesting letters, written shortly before his death, have recently been published.[2] In perhaps the simplest form of the question the level is supposed to be maintained with the aid, *e.g.*, of screw propulsion, the necessary maintenance being secured by an aëroplane slightly tilted (a) upwards in front. The work required to be expended in order to maintain a given weight depends upon the area of plane, the inclination, and the speed. Penaud's results show that, *if skin friction could be neglected*, the necessary work might be diminished indefinitely, even with a given area of wing. For this purpose, it would only be necessary to increase the speed and correspondingly to diminish a. But when skin friction is taken into account, the work can only be reduced to a minimum, and to do this with with a given area of wing requires a definite (large) velocity, and a definite (small) inclination. The accurate determination of the tangential, as well as of the normal, force experienced by an inclined plane is thus of essential importance in the question of flight.

The work of Penaud seems to be so little known that it has been thought desirable to recapitulate some of his theoretical conclusions. But we owe to Penaud not merely sound theory, but the actual construction of a successful flying machine, in which horizontal flight is maintained by a screw propeller. In these models the energy is stored by means of stretched india-rubber, a method available only upon a small scale. It is probable that the principle of the rocket might be employed with advantage; and even upon a large scale the abolition of all machinery would allow of considerable extravagance in the use of explosive material. This method is especially adapted to the very high speeds which on other grounds are most suitable.

In the chapter on "The Plane Dropper," some striking experiments are described, illustrating the effect of a forward movement in retarding the fall of a horizontal plane. Prof. Langley seems hardly to recognize that there is nothing really distinctive in this arrangement when he says:—

"It is, of course, an entirely familiar observation that we can support an inclined plane by moving it laterally, deriving our support in this case from the upward component of pressure derived from the wind of advance; but, so far as I am aware, this problem of the velocity of fall of a horizontal plane moving horizontally in the air has never been worked out theoretically or determined experimentally, and I believe that the experimental investigation whose results I am now to present is new."

But, apart from the complications which attend the establishment of a uniform *régime*, there is no essential difference between the two cases. The hydrodynamical forces depend only upon the magnitude of the relative velocity and upon the inclination of this relative velocity to the plane. All else is a question merely of ordinary elementary mechanics.

It is interesting to note that Prof. Langley's experience has led him to take a favourable view of the practicability of flight upon a large scale Such was also the opinion of Penaud, who (in 1876) expresses his conviction "that, in the future more or less distant, science will construct a light motor that will enable us to solve the problem of aviation." But sufficient maintaining power is not the only requisite; and it is probable that difficulties connected with stability, and with safe alighting at the termination of the adventure, will exercise to the utmost the skill of our inventors.

RAYLEIGH.

[1] See Report of Aëronautical Society for 1876.
[2] Edinburgh Proceedings, R. E. Froude, 1891.

PRELIMINARY NOTICE OF A NEW BRANCHIATE OLIGOCHÆTE.

THE term "Annélides abranches sétigères," applied by Cuvier to the group which included the terrestrial and fresh-water Annelids, now known as the Oligochæta, is no longer applicable to that group. Several Oligochæta have been described as possessing

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gills, which, though for the most part differing in structure from the gills of the Polychæta, must be branchial in function. The most remarkable instance hitherto known is *Alma nilotica*, lately redescribed by Levinsen (*Vidensk. Meddel. naturh. For. Kjöbnhavn*, 1889) under the name of *Digitibranchus niloticus*. The posterior segments of this Annelid possess four to five branchial processes on each side of the dorsal middle line of the body. It cannot yet be regarded as an absolute certainty that this species belongs to the Oligochæta at all; but in any case processes of the body-wall, containing each a capillary loop, and therefore probably branchial in function, have been recently described by Prof. A. G. Bourne (*Quart. Journ. Micr. Sci.*, vol. xxxi.) in a new genus of Naids—*Chætobranchus*. These processes, though doubtless branchial in function, are rather suggestive of the parapodia of marine Annelids, since they inclose, partially or entirely, the dorsal setæ. I have lately had the opportunity of examining this Annelid, through the kindness of Mr. Sowerby. The "*Victoria regia* tank" at the Botanical Society's Gardens, which produced the celebrated "Fresh-water Medusa" and other remarkable forms, furnished me with *Chætobranchus*, and with a new and interesting form of branchiate Oligochæte, which I propose to call *Branchiura Sowerbii*.

In its general aspect this worm recalls a *Tubifex*; the setæ, in their shape, and in their arrangement, resemble those of *Tubifex*. But here the resemblance ends. The last sixty segments or so of the body (there are from 130–170 segments altogether) are provided with a paired series of long tentacle-like processes—a pair to each segment—lying the middle ventral and dorsal lines; towards the middle of the series these processes exceed in length the diameter of the body; anteriorly and posteriorly they diminish, and finally become mere wart-like protuberances. The processes in question are supplied with blood from the main vascular trunks. They are in continual movement, each branchia moving quite independently by means of the contraction of simple muscular fibres. The writhing movements, as well as the structure of these organs, is much like that of the tentacles and cirri of certain Polychæta. Apart from the individual contractions of these branchiæ, the tail end of the worm perpetually jerks from side to side, particularly when the creature is in any way disturbed. I do not know whether the worm usually rests in the mud with the tail protruding and waving about, like many other aquatic Oligochæta; but it is probable, from the limitation of the branchiæ to the tail end, that it does. I found three specimens, which were slowly crawling about.

FRANK E. BEDDARD.

THE ANNIVERSARY OF THE ROYAL SOCIETY.

MONDAY being St. Andrew's Day, the anniversary meeting of the Royal Society was held in their apartments in Burlington House. The report of the auditors of the Treasurer's accounts having been read, and the Secretary having read the list of those Fellows who have been elected and those who have died since the last anniversary, the President, Sir William Thomson, delivered the anniversary address. After an account of the scientific work of those Fellows who had died within the year, the President proceeded:—

"The Royal Society, since the last anniversary meeting, has been, as always, active both in the proceedings of its ordinary meetings, which have been full of scientific interest, and in the conduct of the important affairs committed to its Council. During the past year nineteen memoirs have been published in the Philosophical Transactions, containing a total of 1020 pages and 60 plates. Of the Proceedings, six numbers have been issued, containing 893 pages. Of the large number of papers which have been published in the Proceedings two-thirds are on the physics and dynamics of dead matter and one-third on biological subjects.

"As stated by Sir George Stokes in his Presidential Address at the last anniversary meeting, a revision of the whole body of the Statutes of the Royal Society had been entered upon, a Committee had recently reported to the Council, and its report had been left to the new Council then entering on office to take such action in the matter as might be judged proper. The Council now concluding its term of office has accordingly given much time to the subject, and has completed the work of re-enacting the Statutes with such amendments as have seemed desirable. The only questions upon which there was effective difference of opinion were those connected with the election of Fellows, which were referred to by Sir George Stokes as having elicited considerable difference of opinion in the reporting Committee. The Council, after much anxious consideration, resolved to make no change of the existing Statutes in this respect.

"There have been no changes during the past session in the constitution of the staff employed in the Offices and Library; but in the Catalogue Department, two lady assistants and two copyists have been engaged to work under the superintendence of Miss Chambers, who succeeded in July of last year to the post rendered vacant by the death of the late Mr. Holt, and who continues to give every satisfaction in the discharge of her duties.

"In January of the present year a communication was received from our Fellow, Prof. G. S. Brady, intimating that his brother, the late Mr. Henry Bowman Brady, whose decease I have already mentioned, had bequeathed to the Society all his books and papers relating to the Protozoa, with the recommendation that they should be kept together as a distinct collection. In case this recommendation should be adopted, a further bequest of £300 was made, the interest or principal or both to be applied, at the discretion of the Council, to the purchase of works on the same or kindred subjects, to be added to the collection. The Council have accepted both these bequests, and a case marked with an engraved plate has been set aside in the Library for the accommodation of the Brady collection.

"His Excellency Robert Halliday Gunning, M.D., LL.D., F.R.S.E., who in 1887 founded certain scholarships and prizes for the promotion of original scientific work and proficiency in scientific education in connection with the Royal Society of Edinburgh, the University of Edinburgh, and other institutions in that city, called the Victoria Jubilee Prizes, desires to institute foundations of a similar kind in London. He has accordingly given to the Royal Society a sum of £1000, to be ultimately invested in such manner as the President and Council, in their absolute and uncontrolled discretion, may think fit, and to be held in trust always for the purpose of forming a fund the annual income of which shall be applied triennially towards the promotion of physical science and biology in such manner as to the President and Council of the Royal Society may appear most desirable. The President and Council, for the time being, are given full power to make such rules and regulations as they think fit with regard to the application of the income of the fund, which shall always be kept distinct from and not in any way immixed with the general funds of the Royal Society.

"A very important resolution for the advancement of natural knowledge has been adopted during the past year by the Royal Commissioners of the Exhibition of 1851, in the institution of the Exhibition Science Scholarships, to which, after the first year, an expenditure to the extent of £5000 a year is to be devoted. Sixteen appointments have already been made to scholarships of £150, to be held for two years, with possible renewal for a third year. The Commissioners require of each candidate for an appointment satisfactory evidence of proficiency in a three years' course of University or high class College study, and of capacity for experimental work. To the tenure of each scholarship the duty is assigned of advancing science by experimental work in physics, mechanics, chemistry, or any application of science tending to benefit our national industries.

"A Committee of the British Association, appointed for the purpose of reporting on the best means of comparing and reducing observations on terrestrial magnetism, has strongly recommended the re-establishment of a magnetic Observatory at the Cape of Good Hope. A conference on the subject was held between the Committee and Dr. Gill, the Astronomer-Royal of the Cape of Good Hope, last June, during his recent visit to

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England, which has resulted in an application to the Admiralty to carry this recommendation into practical effect in connection with the astronomical Observatory of the Cape of Good Hope (belonging to the Admiralty). This application is at present under the consideration of the Admiralty.

"A fundamental investigation in astronomy, of great importance in respect to the primary observational work of astronomical Observatories, and of exceeding interest in connection with tidal, meteorological, and geological observations and speculations, has been definitively entered upon during the past year, and has already given substantial results of a most promising character. The International Geodetic Union, at its last meeting in the autumn of 1890, on the motion of Prof. Foerster, of Berlin, resolved to send an astronomical expedition to Honolulu, which is within 9° of the opposite meridian to Berlin (171° west from Berlin), for the purpose of making a twelve months' series of observations on latitude corresponding to twelve months' analogous observations to be made in the Royal Observatory, Berlin. Accordingly, Dr. Marcuse went from Berlin, and, along with Mr. Preston, sent by the Coast and Geodetic Survey Department of the United States, began making latitude observations in Honolulu about the beginning of June. In a letter from Prof. Foerster, received a few weeks ago, he tells me that he has already received from Honolulu a first instalment of several hundred determinations of latitude, made during a first three months of the proposed year of observations; and that, in comparing these results with the corresponding results of the Berlin Observatory, he finds beyond doubt that in these three months the latitude increased in Berlin by one-third of a second, and decreased in Honolulu by almost exactly the same amount. Thus, we have decisive demonstration that motion, relatively to the earth, of the earth's instantaneous axis of rotation is the cause of variations of latitude which had been observed in Berlin, Greenwich, and other great Observatories, and which could not be wholly attributed to errors of observation. This, Prof. Foerster remarks, gives observational proof of a dynamical conclusion contained in my Presidential Address to Section A of the British Association at Glasgow, in 1876, to the effect that irregular movements of the earth's axis to the extent of half a second may be produced by the temporary changes of sea-level due to meteorological causes.

"It is proposed that four permanent stations for regular and continued observations of latitude, at places of approximately equal latitude, and on meridians approximately 90° apart, should be established under the auspices of the International Geodetic Union. The reason for this is that a change in the instantaneous axis of rotation in the direction perpendicular to the meridian of any one place would not alter its latitude, but would alter the latitude of a place 90° from it in longitude by an amount equal to the angular change of the position of the axis. Thus two stations in meridians differing by 90° would theoretically suffice, by observations of latitude, to determine the changes in the position of the instantaneous axis; but differential results, such as those already obtained between Berlin and Honolulu, differing by approximately 180° in longitude, are necessary for eliminating errors of observation sufficiently to give satisfactory and useful results. It is to be hoped that England, and all other great nations in which science is cultivated, will co-operate with the International Geodetic Union in this important work."

The celebration of the hundredth anniversary of the birth of Faraday, recorded in our columns at the time, was next referred to.

"A matter of great importance in respect to the health of the community was submitted to the Royal Society by the London County Council, in a letter of date May 1, 1891, asking for information and suggesting investigation regarding the vitality of microscopic pathogenic organisms in large bodies of water, such as rivers which are sources of water-supply and which are exposed to contamination. After some correspondence, it was agreed, between the County Council and the Council of the Royal Society, to enter upon an investigation, the expense of which was to be defrayed partly by the London County Council and partly by the Royal Society out of the Government Grant for Scientific Research. When we consider how much of disease and death is due to contaminated water, we must feel that it is scarcely possible to over-estimate the vital importance of the proposed investigation. Let us hope that the alliance between the London County Council and the Royal Society, for this great work, may be successful in bringing out practically useful results.

Prof. Stanislao Cannizzaro (Copley Medal).

"Stanislao Cannizzaro, Senator of Italy, and Professor of Chemistry in the University of Rome, has rendered invaluable service to the philosophy of modern chemical science. The work of Avogadro, in 1811, and afterwards that of Ampère, had already thrown much light on the relative weights of the molecules of elementary bodies, and on the proportion in which those weights enter into chemical combination. But it is to Cannizzaro that we owe the completion of what they had left unfinished He pointed out the all-important difference, hitherto overlooked, between molecular and atomic weights, and showed (1) how the atomic weights of the elements contained in a volatile compound can be deduced from the molecular weights of such compounds; (2) how the atomic weights of the elements the vapour-densities of whose compounds were unknown can be ascertained by help of their specific heats. By these investigations the series of atomic weights of the elements, the most important of all chemical constants, and the relation which these weights bear to the molecular weights of the elements, have been placed on the firm basis on which they have ever since rested. It is to Cannizzaro that science is indebted for this fundamental discovery, and it is this which it is proposed to recognize by the award of the Copley Medal.

Prof. Charles Lapworth, F.R.S. (Royal Medal).

"Prof. Lapworth is the author of some of the most original and suggestive papers which have appeared in the geological literature of this country for the last twenty years. Special reference may be made to his researches on graptolites, and to his patient investigation by these means of the exceedingly complicated structure of the Silurian uplands of the south of Scotland. He has been able not only to supply the key which has given the solution of the stratigraphical difficulties of that region, but also to furnish theoretical geology with an array of new facts from which to philosophize as to the mechanism of mountain-making. Of not less importance are his detailed studies of the structure of the North-west Highlands, and his demonstration of the true order of stratigraphical sequence in that region of complex disturbance As a stratigraphist he has attained the highest rank, and he has likewise made himself a chief palæontological authority on the structure and distribution of the Graptolitidæ. For some years past he has been engaged in a laborious study of the Silurian and Cambrian rocks of the middle of England, the detailed publication of which is awaited with much interest by geologists.

Prof. Rücker, F.R.S. (Royal Medal).

"In conjunction with Prof. Reinold, Prof. Rücker carried out an important series of researches (extending over ten years) on the electric resistance and other physical properties of liquid films, in the course of which the fact was established that the black part of a soap film in equilibrium has a uniform or nearly uniform thickness of 11 or 12 micromillimetres, and that there is an abrupt augmentation across its border to a thickness of about 30 or 40 micromillimetres in passing to the coloured portions. This, considered in connection with the well-known sudden opening out of the little black areas in an ordinary soap-bubble, proves a minimum of surface-tension for some thickness between 10 and 50 micromillimetres, which in the ordinary soap-bubble, unmodified by Reinold and Rücker's electric current, is temporarily balanced in virtue of the abrupt change of thickness, a proposition of fundamental importance in the molecular theory, implying the existence of molecular heterogeneousness.

"In theoretical calculations connected with the compounding of dynamos and motors to produce constant potential difference, constant current, or constant speed, electricians did not see their way to obtain results of a sufficiently simple character to be of use in practice, if they employed a function of the current which fairly represented the magnetism. They were, therefore, compelled to assume in such calculations that the magnetism was a linear function of the current, although it was well known that this was very far from being true when the current was large. Prof. Rücker, however, developed a simple method of attacking such problems, and showed how the magnetic saturation of the iron might be taken into account, and a comprehensive solution of the general problem of compounding dynamos and motors obtained in a workable form. Prof. Rücker's paper containing

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his investigation, and which will be found in the Proceedings of the Physical Society, is a most valuable contribution to the theory of direct-current dynamos and motors.

"Prof. Rücker has, with the co-operation of Prof. Thorpe, completed a magnetic survey of the British Isles (1884-89), which, independently of its great value in investigations of the distribution of the earth's magnetism, and the changes to which it is subject, is specially remarkable for the exhaustive discussion of the observations in reference to regions of local magnetic disturbance, and their relation to the geological constitution of the earth's crust in the neighbourhood. Prof. Rücker has followed up this discussion by a paper on 'The Relation between the Magnetic Permeability of Rocks and Regional Magnetic Disturbances,' read before the Royal Society. The high estimate that has been formed of the value of this magnetic survey is perhaps most easily appreciated from the very large sums that the Government Grant Committee have recommended should be contributed to aid in the completion of this work of international importance.

Prof. Victor Meyer (Davy Medal).

"Prof. Victor Meyer, formerly the successor of Wöhler at Göttingen, and who now occupies the chair of Bunsen at Heidelberg, is eminent as an original worker and discoverer in almost every branch of chemical science. His methods of determining the vapour-densities of substances have been of the greatest service to chemists, not only as convenient and generally applicable modes of ascertaining atomic and molecular weights, but also as serving to throw light on the molecular constitution of elements and compounds under varying conditions of temperature and pressure. A striking example of the value of these methods is seen in their application by their author to the study of the molecular dissociation of the element iodine—one of the most masterly investigations of recent years, and which is universally recognized as of the very highest significance and importance. Not less noteworthy are Victor Meyer's services to organic chemistry. His work on the nitroso-bodies, and his brilliant discovery of thiophene, the initial member of a class of substances hitherto unknown, his subsequent synthetical formation of it, and the remarkable series of researches on its derivatives, in part carried out with the aid of his pupils, stamp him as an investigator of exceptional power and distinction."

The Society next proceeded to elect the Officers and Council for the ensuing year. The following is a list of those elected:—President: Sir William Thomson. Treasurer: John Evans. Secretaries: Prof. Michael Foster, The Lord Rayleigh. Foreign Secretary: Sir Archibald Geikie. Other Members of the Council: Captain William de Wiveleslie Abney, William Thomas Blanford, Prof. Alexander Crum Brown, Prof. George Carey Foster, James Whitbread Lee Glaisher, Frederick Ducane Godman, John Hopkinson, Prof. George Downing Liveing, Prof. Joseph Norman Lockyer, Prof. Arthur Milnes Marshall, Philip Henry Pye-Smith, William Chandler Roberts-Austen, Prof. Edward Albert Schäfer, Sir George Gabriel Stokes, Bart., Prof. Sydney Howard Vines, General James Thomas Walker.

In the evening the Fellows and their friends dined together at the Whitehall Rooms, Hôtel Métropole. The company numbered over 230. The chair was occupied by the President.

After the loyal toasts, Dr. John Evans proposed "Her Majesty's Ministers and the Members of the Legislature," a toast to which Sir J. Fergusson responded.

In response to "The Royal Society," proposed by Mr. Forwood, M.P. (who referred to the fact that Sir William Thomson's discoveries "had rendered it possible to steer vessels on our fog-bound coast with an accuracy never before attained to"), the President said that the Royal Society had always been distinguished for the promotion of investigations leading to such results as Mr. Forwood had named. In illustrating this, he spoke of the history of the construction of the sextant and the development of the dynamical theory of the trade winds. A curious interest attached to some of the earlier Transactions of the Society, such as a paper which attributed the trade winds to the breathing of a certain plant, which turned to the sun and blew its breath after it. The earlier pages of the Transactions were full of chronometers and of the work leading up to the invention which gained the reward of £10,000. Excellent work was done with the grant of £4000 administered by this and allied Societies; and he believed its future achievements would at least equal those of the past. The next fifty years would probably produce, in the science of dead matter, and in the science of living matter too, discoveries compared with which those of the last 300 years would ultimately appear to be small indeed.

The President proposed the health of "The Medallists," and spoke in eulogistic terms of the services in respect of which the medals had been awarded.

The Italian Ambassador briefly responded in the name of Prof. Stanislao Cannizzaro.

Prof. Rücker, in responding for the other medallists, said:—

Islanders as we were, the Royal Society prided itself on the fact that some of its medals could be awarded to distinguished scientific workers outside these islands. This year no less than four foreign Fellowships and two medals testified to our respect and esteem for colleagues abroad. We respected them for many things—for the thoroughness with which they grasped all that the scientific movement meant and involved; for the foresight and courage with which—beginning at the beginning—they had provided for their students laboratories and workshops such as no English lad could enter at home. We respected them for the sound educational methods which had led them to use these appliances, so as to point the student to the research laboratory rather than to the examination room as the goal of his ambition. We respected them because these methods have produced their natural results, and year by year a crop of new scientific facts was reaped not only from the laboratories of their Colleges, but from the workshops of their manufacturers. We respected and esteemed most of all the men who had thus led or who were thus leading their countrymen aright—veterans, such as Cannizzaro, who, amid the turmoil in which the foundations of modern Italy were laid, found time to lay the foundations of chemistry anew; investigators, such as Victor Meyer, who, when Bunsen retired from the laboratory where so many English chemists learnt or perfected their art, was judged by all to be a worthy successor to Bunsen himself. While fully admitting that we had something to learn from the work and methods of our foreign colleagues, we might claim that our progress had lately quickened where at one time we notoriously lagged behind. In the multiplication of centres of scientific work, Scotland was formerly the only part of these islands which compared with Germany. This was no longer so. Every large town in England and Wales and the chief towns of Ireland had now University Colleges. Their scale was modest indeed when compared with what a paternal Government was providing for Strasburg, or a democracy for Zürich; but they were full of intellectual energy and of scientific work. Hardly a month passed without the publication of papers on researches conducted in the laboratories of some of them. Almost every year they were represented in the list of Fellows newly elected into the Society. Out of the last eight recipients of the Royal Medals, five had, either as learners or as teachers, or, in turn, in both capacities, spent many years within the walls of one or other of our provincial Colleges. But he must not be understood as claiming for English science only that it was making good confessed educational deficiencies. There were sciences which, either in their origin or their development, were peculiarly our own. One of these was geology. Crowded up between our four seas was an epitome of the past history of the world such as he believed no other country possessed in an equally small area. Thus geologists were a natural product of our soil. But there was one particular in which he thought the President, more than most, would appreciate Prof. Lapworth's audacity and success. Though a Southerner, he had made a foray into Scotland, and had returned laden with spoil. It was true that he, too, had crossed the border, and he deeply regretted that he must confess that his track was marked by disturbances: but speaking for Prof. Lapworth and on his behalf—though without consulting him—he must admit that his offences were venial and that he was most to blame. He turned the most fundamental institution

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of Scotland—its geological strata—upside down. There were only two ways of meeting an invader or an innovator such as this—with steel or with gold. They must confute him with the pen or reward him with a medal. The Council had chosen the better part of valour, and he was sure the Society did not question their discretion. With regard to himself, there was one remark that he must make. In some of the principal researches in which he had been engaged he had worked with colleagues. While, therefore, thanking the Society for the honour they had been pleased to confer upon him, he was, perhaps, not wrong in thinking that Dr. Thorpe and Prof. Reinold, who had received many marks of appreciation from the Royal, the Physical, and the Chemical Societies, were receiving further, though less direct, recognition from the Royal Society to-day. Apart from all minor questions, the distinguishing characteristic of this meeting was the bringing together of men who were working at different branches of science. These gatherings, and those which in the summer take place during the meetings of the British Association, were, he thought, good for all of them. They checked that scientific particularism which in the cultivation of a subject of study ignored the culture of the student. They reminded them that they were all co-operating to one common end—the promotion of natural knowledge. The very speech that he was making bore testimony to this fact, for were it otherwise the President would not have called upon an Englishman to reply for our absent foreign medallists, or a physicist to return thanks for honours bestowed on experts in geology and chemistry. It was only because he himself believed that there was between scientific men a similarity of aim and object, and a community of ideas, which underlay all superficial differences, that he ventured to undertake the task of expressing the thanks in which, he was sure, one and all of the medallists most heartily joined.

Prof. Dyer proposed "The Visitors," associating with the toast the name of the Greek Minister. He said:—

The association appeared to him a peculiarly happy one. The other day he came across a striking statement of Sir Henry Maine's—"Except the blind forces of Nature, nothing moves in this world which is not Greek in its origin." The former influence they could in this Society give some account of. But the latter he regarded with a certain scientific scepticism. Yet he was not disposed to dispute its validity. We still commenced our often arduous mathematical studies with Greek geometry, and he could not gainsay those who thought that the influence of the counsels of Plato, and of the precepts of Aristotle, was unexhausted. In art Greece remained unsurpassed and unsurpassable. Some might say that if scientific men had their way they would extinguish Greek studies. This was far from the truth. In this Society they rejoiced in those exact studies which recreated the literature and life of the past.

The Greek Minister, in replying, said :—

He had always been of opinion that those who were intrusted with the duty of representing their respective Governments in this country, need confine their watchfulness and activity neither to political nor to social circles alone. They had before them a wide and unrivalled field in which to study the benefits accruing to a whole community—to the Government itself—from the efforts of private individuals, when guided by public zeal and devotion to science; and he thought no more striking example of such benefits could be instanced than the results of the labours of this, the most ancient and most illustrious of learned Societies. It might be said to have been born with the first dawn of scientific research in England; it had remained its stronghold in times of political trouble and change; it numbered in its long muster roll all those names which had bequeathed an undying fame to British science; it had worked out and solved, for the benefit of the State, scientific questions which were elsewhere delegated to official departments alone; its catalogue of scientific papers was a monument of the world-wide grasp of its subjects. That the achievements of this Society should have been continuous and ever increasing in importance for close upon 250 years was characteristic of British public zeal and tenacity of purpose. But what was especially instructive was the ardour with which such work was prosecuted, not only by those whose pursuit was science, but by those especially who, like the illustrious statesman at the head of Her Majesty's Government, being independent by fortune and already great by birth and political achievements, yet contributed powerfully to the advancement of science. It was at symposia such as this that the philosophers of ancient Greece laid down those great truths of science which had found amongst this Society such ardent apostles and such illustrious expounders. The guests on whose behalf he responded, and he himself, expressed sincere acknowledgments for the honour they had done them that night.

The company then separated.

NOTES.

A MEETING of the honorary council of advice in connection with the Crystal Palace Electrical Exhibition, which is to be opened on January 1 next, was held last week at the Mansion House. The Lord Mayor presided. Mr. Gardner, the secretary of the Crystal Palace Company, read the report of the directors, in which they referred to the Electrical Exhibition at the Palace in 1881, and to the enormous strides which had since been made in the industry. The Exhibition of 1881 was recognized as the pioneer of electrical engineering in this country, and it was confidently believed that the Exhibition of 1892 would be remembered in history "as showing that the infant Electra has grown to years of maturity, and is capable of further aiding science, commerce, and the world at large." The space available had been over-applied for, and every section of the industry would be well represented. Invitations would be issued to public bodies throughout the United Kingdom to visit the Exhibition, where the various systems of electric lighting would be on view, and in this direction alone very great saving of expense to the authorities would be effected, and other advantages must, the directors believed, also accrue. On the motion of Mr. W. H. Preece, the following gentlemen were appointed to act as a committee of experts in connection with the exhibits: Profs. W. Grylls Adams, W. E. Ayrton, W. Crookes, D. E. Hughes, A. B. W. Kennedy, J. Perry, and Silvanus Thompson, Major P. Cardew, Sir J. N. Douglass, Mr. W. B. Esson, Mr. Gisbert Kapp, and Mr. Preece.

ON Friday last a portrait of Sir William Thomson, by Mr. Herkomer, was presented to the University of Glasgow. A number of friends subscribed for it, to signalize Sir William's election to the office of President of the Royal Society. The presentation was made by Mr. Balfour, the Lord Rector of the University, who spoke eloquently of Sir William Thomson's great career as a man of science and an inventor. A replica of the portrait was presented to Lady Thomson.

THE Egyptian Government has asked the Caisse de la Dette for £50,000 from the general reserve fund on behalf of the Antiquities Department. The Cairo correspondent of the *Times* says that before granting so large a sum the Caisse will probably require the appointment of a Commission to study the purposes for which it is to be used. It is hoped that searching investigation will be made into the management of the department generally.

IT is expected that Australia will be well represented at the Chicago Exposition. Exhibits connected with education, minerals, forestry, and especially wool are to be sent. About fifty wool growers and wool brokers met lately at Sydney, and decided to despatch a very extensive collective exhibit of wools.

WE have to note a change in the form of the publications issued by the Meteorological Department of India. From January 1 last, the Annual Reports on the Meteorology of India, which have hitherto been issued about fourteen months after the termination of the year to which they referred, have been replaced by a Monthly Weather Review, the first four parts

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of which have been received. It is hoped that, when the arrangements are complete, these reports will be published from six to eight weeks after current date. The materials used are the morning observations taken at 136 stations, and afternoon observations taken at 82 stations; and eventually, a monthly summary of rainfall observations will be given for about 2500 stations. The text contains full discussions of the chief features of the weather, under each of the principal elements, illustrated by maps showing the mean distribution of the conditions for the month, and the variations from the mean. The report for January also contains a brief review of the meteorology of the year 1890, in which it is stated that conditions were very abnormal in Upper India, and very favourable for a severe winter in the hill districts, and for abundant rain in the plain districts, while the snowfall in the Kashmir Himalayas and Afghanistan during November and December was abnormally heavy. Temperature was steadily below the average in Northern India, but was excessive in the peninsula.

THE *Abhandlungen* of the Royal Prussian Meteorological Institute (Bd. i., No. 4, 1891) contain the first part of a treatise on the climate of Berlin, referring to rainfall and thunderstorms. Berlin possesses a long series of observations, commencing with the beginning of the eighteenth century, but in this investigation some of the earlier observations have not been used. The subjects treated of are:—(1) The amount of rainfall, the annual mean being given as 23 inches. The extreme values varied from 14·26 inches in 1887 to 30 inches in 1882. The wettest months were June and July, yielding together 24 per cent. of the annual amount. (2) Rain frequency. The average number of days on which more than 0·08 inch fell was 152. The months of greatest rainfall frequency were November and December. (3) Hail and soft hail (*Graupel*). The former occurred on 2 to 3 days and the latter on 3 to 4 days in each year, and mostly in the months May, June, and July. (4) Snow. A Berlin winter numbers on an average 33 snowy days. The distribution according to months is very curious: snow does not occur most frequently in the coldest months; it falls as often in March as in December. It lies on the ground 49 days on an average. (5) Intensity of rainfall. Daily falls of more than 2 inches are quite exceptional, and of 1½ inches are not frequent. The greatest fall was 1·86 inches in 1½ hours. (6) Wet and dry periods. Attention is more particularly given to periods of short duration; wet periods of five or more days are fewer than dry periods of similar length; the former average 7·5 and the latter 13·2 per year. (7) Thunderstorms. Berlin enjoys comparative immunity from thunderstorms, as they occur on an average on only 15 days in the year, about half of them being in June and July. This valuable discussion has been carried out by Prof. G. Hellmann.

THE common type of cyclone weather is sometimes materially altered by orographical conditions. This is the case, *e.g.*, at Turin, as recently shown by Signor Rizzo (in a paper to the Academy there). He cites thirty-three cases in the last twenty-five years, which indicate the general course of the weather when a cyclone passes over Northern or Central Europe. After fall of the barometer, with strong west wind, the sky clears, the temperature rises considerably, and the moisture of the air diminishes. This is explained by the influence of the Alps. The strong west wind is forced up the mountain-range, so that its aqueous vapour is condensed, and falls as rain and snow on the western slopes and summit. After crossing the ridge, it descends, and, having parted with its moisture, appears as a warm dry wind (thus forming an unusual feature in cyclones).

THE temperature of the rivers of Central Europe has been recently investigated by Herr Forster, of the Society of Geographers at Vienna University; the monthly and annual means being obtained from thirty-one stations. He distinguishes (with reference to river and air temperature) the following types:—(*a*) Glacier rivers. These are always warmer than the air in winter, and much cooler in summer; on the average of the year, they are about 1° colder. (*b*) Glacier rivers modified by lakes, and rivers from lakes in general. These are, except in spring, warmer than the air, therefore warmer on the annual average. (*c*) Mountain rivers. Like glacier rivers, these are warmer in winter and cooler in summer than the air, but the difference, especially in summer, is not nearly so great; so that, on the average of the year, it is approximately 0°. (*d*) Flat country rivers. Their temperature is, throughout the year, higher than that of the air; and the annual average difference is over 1°. Sometimes a different relation between river and air temperature is found in the upper part of a river and in the lower, and transition-types occur between those above indicated.

THE Bahama Islands are soon to be connected with the general telegraphic system of Great Britain and the world. A submarine cable about 200 miles long will be laid from a point about five miles from Nassau, New Providence, to a point about the same distance from Jupiter Inlet, on the south-east coast of Florida. The cable has been designed for the Government of the colony by Mr. W. H. Preece. It will be insulated with gutta-percha, and is being manufactured by Messrs. W. T. Henley and Co. It will be laid in January or February next by the steamer *Westmeath*, belonging to that firm. As the Western Union Telegraphic Company's Floridan lines do not at present run so far south as Jupiter Inlet, the station at the American terminus of the cable will be in charge of the officers of the United States Weather Bureau, who will transmit the messages to the Western Union Company's system over their private line. Traffic between England and the Bahamas will thus pass through the Atlantic cables.

AT a meeting held at Aylesbury on Saturday it was resolved, on the motion of Sir Harry Verney, that it was desirable to establish a County Museum for Bucks, and that an executive committee should be appointed to take the necessary measures. Letters from various eminent men of science were read at the meeting. Prof. Flower, writing from the British Museum, pointed out that a good County Museum, well arranged, neat, and attractive, might be the means of conveying instruction and giving interest and pleasure to thousands, and that money, time, knowledge, and sympathetic care must be expended upon it. Prof. Alfred Newton, of Magdalene College, Cambridge, said that the proposal to establish a Museum for Buckinghamshire had his best wishes. He advocated the founding of a maintenance fund, which should be vested in trustees. Prof. Green, of the University Museum, Oxford, contended that in the proposed Museum care should be taken for the proper selection of objects, the primary end being to illustrate the district in which it existed.

DR. ERMLING contributes to the current number of *Globus* an interesting paper on the Nurhagi of Sardinia. There are said to be more than 3000 of these prehistoric buildings in the island. They are almost all in fertile districts, and are built in groups which are separated from one another by wide and generally barren spaces. According to many archæologists, the Nurhagi were tombs; but the late Canon Spano, in his "Memoria sopra i Nurhagi di Sardegna," published in 1854, contended that they were dwellings and places of refuge, and this view is accepted by Dr. Ermling. In a trench closed with asphalte, under the ruins of a Nurhage near Teti, various bronze statuettes, swords, spear-heads, and axes were discovered lately by shepherds. These treasures are now in the museum of M. Gouin, a Frenchman, in Cagliari. Some of the objects have been analyzed, and it has been found that the chemical composition of the bronze statuettes is not the

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same as that of the axes. The statuettes consist of copper 90·3, tin 7·4, iron 2·1; the axes, of copper 87·4, tin 12·0, lead 0·5, with traces of iron.

IN the new number of *Petermann's Mitteilungen* Prof. Vambéry has a valuable paper on the geographical nomenclature of Central Asia. He gives a list of names, his spelling of which may safely be accepted as authoritative. The list is to be extended on some future occasion.

AT a recent meeting of the Field Naturalists' Club, Victoria, Mr. C. G. W. Officer read a paper on supposed human footprints on Æolian rocks at Warrnambool. In introducing the subject, Mr. Officer described in detail the formation and nature of the sand dunes, and their connection with the underlying strata, as shown by the similarity of the stone now being quarried there. From an analysis of the stone made by Mr. Avery, of Queen's College, it appears that it contains about 94 per cent. of carbonate of lime. Last December a slab was discovered in one of the quarries bearing impressions which suggested that they were made by human beings. This slab was secured by Mr. Archibald, and placed in the Warrnambool Museum. The determination of the age of the rocks is of importance, and from the evidence of subsidence and elevation which have probably taken place since the impressions were made, Mr. Officer is of opinion that a considerable lapse of time has occurred since the rocks were laid down, and he suggests that the impressions were made by two individuals sitting close together and somewhat obliquely to each other. Mr. J. Dennant, discussing the paper, pointed out that it was necessary to be very guarded in accepting any but the strongest evidence on such questions as those relating to the supposed footprints. Amongst limestone rocks it was well known that mimetic forms were common. In the Æolian rocks of Cape Bridgewater occurred the so-called fossil forest, which the casual observer could hardly be persuaded to believe was an accidental resemblance, and nothing more. At the same time Mr. Dennant congratulated Mr. Officer on having produced an interesting and highly suggestive paper. The rocks were well described, and whether his conclusions concerning the impressions were accepted or not, he had succeeded in drawing renewed attention to one of the most striking formations in Victoria.

MR. J. B. TYRRELL, Ottawa, of the Canadian Geological Survey, has spent the last two summers in examining the shores of Lake Winnipeg, Winnipegosis, and Manitoba; and he has issued a few notes on his observations, in advance of a more detailed report to the Survey. Speaking of striation, Mr. Tyrrell refers to many distinct and characteristic glacial striæ which show that during the Ice Age a great glacier, or lobe of the Laurentide glacier, moved south-south-eastward across the lacustral plains of Manitoba, along the valley of Red River to the height of land, and onward to near Des Moines, Iowa, sending off branches up the valleys of Swan and Red Deer rivers. The total length of this glacier or lobe, from the north end of Lake Winnipeg to its extreme southern limit in Iowa, would be about 850 miles. With reference to moraines, Mr. Tyrrell says the highest at present known in Northern Manitoba are those capping the summits of portions of the Duck and Riding Mountains, with altitudes of 2500 to 2700 feet above the sea, or 1800 to 2000 feet above the surface of Lake Winnipeg. On the shores and islands of Lake Winnipeg a distinct moraine has lately been recognized. In a section on shore lines Mr. Tyrrell describes Kettle Hill, on the south side of Swan Lake, as one of the most interesting monuments of ancient shore phenomena in the whole district. Swan Lake has an estimated elevation of 27 feet above Lake Winnipegosis, or 855 feet above the sea; and the hill, which appears to have been largely composed of Dakota sandstone, rises 275 feet above it. On the face of this hill are five distinct terraces, representing six different shore lines, at elevations of 920, 955, 995, 1015, 1070 feet above the sea, those at 955, 995, and 1070 being most strongly marked, the last being the most distinct.

MR. D. MORRIS, Assistant Director of the Royal Gardens, Kew, lately sent to the *Entomologist's Monthly Magazine* for identification specimens of a Coccid, supposed by him to be *Icerya Purchasi*, received from St. Helena. They were found there on some rose bushes which had been imported from the Cape of Good Hope. In a note in the new number of the *Entomologist's Monthly Magazine*, Mr. J. W. Douglas says there is not the least doubt that the specimens received are females of *Icerya Purchasi*; and he adds that if the brood of which they are samples be not extirpated at once by burning all the plants on which they exist, so as to destroy all eggs and young larvæ, they will form the beginning of a pest that must be intensely serious in such a small island. The probability is that they were introduced as eggs or larvæ, and so escaped observation.

THE fourth volume of the entomological publication issued by the Russian Grand-Duke Nicholas, under the title of "Mémoires sur les Lépidoptères, rédigés par N. M. Romanoff," contains a very valuable work by M. Gr. Grum-Grshimailo—"Le Pamir et sa Faune lépidoptérologique," with twenty-one coloured plates and a map of the Pamir. Besides its special entomological part, the work contains some interesting deductions concerning the geological history of the Pamir. The author came to the conclusion, confirmed afterwards on geological grounds by Prof. Mushketoff, that during the Miocene period the Pamir plateau and Tibet formed a continent which rose isolated above the great Tertiary sea. It was separated at that time from the Tian-Shan Mountains, but seems to have been connected with the Altai Mountains, probably through the Bei-Shan highlands. The hypothesis seems probable on orographical grounds as well—the Pamir and the Altai Mountains belonging to the great plateau of Asia of which the Great Altai is one of the border ridges, while the Tian-Shan belongs to the series of ridges parallel to the border ridges, and is separated from them by deep valleys, which must have been filled by the waters of a Tertiary sea. The same structure may be observed in East Siberia also.

LISTS of the Macro-Lepidoptera and birds of Winchester and the vicinity have been compiled by members of the Winchester College Natural History Society, and have now been published together in the form of a pamphlet. The compilers have evidently taken great care to be accurate, and their work cannot fail to be of service to students of natural history in the locality. Mr. A. W. S. Fisher, who signs the preface to the list of Lepidoptera, points out that it contains 425 species, which have all occurred within six miles of Winchester College. Mr. S. A. Davies, in the list of birds, indicates by an asterisk the cases in which the birds recorded have been bred within a radius of a quarter of a mile of the College. In most cases, Mr. Davies himself has found the nest within the last three years.

ACCORDING to Hering's views, the optical stimulation-value, or "valence," of a coloured radiation, is made up of one white and one or two colour valences (the greater the former, the less the saturation). And he has sought to measure the white valences; one useful means lying in the fact that to an eye kept long in the dark all coloured rays of a certain low intensity seem colourless, but of very different brightness. Hering has lately had an opportunity of taking measurements on a person having sight, but totally blind to colours (a very rare case). This was a music-teacher, twenty years of age. The experiments (described in *Pflüger's Archiv*) brought out the fact that

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the spectrum of the totally colour-blind is considerably shortened; in this case it began about 665 μ, and ceased about 420 μ. The greatest intensity was in the green. Further, it appeared that all coloured radiations had the same relations of brightness to each other for the adapted normal eye as for the eye of the colour-blind person. With any two spectral lights, again, an equality of sensation could be produced in this person, when a suitable ratio of intensities was established; and when the two different colours, which seemed equal to the colour-blind, were examined with the normal eye (adapted to darkness), it was found that these two colours had equal white valences. In general, the brightness-curve of the spectrum of the colour-blind had the same course as the curve of white valences for the normal eye. These facts are regarded as a strong confirmation of the author's views.

THE Morgue in Paris now has a medico-legal institute attached to it, with courses of lectures, &c. The need of frigorific apparatus has been long felt, and in a recent competition for the supply of it, the arrangement proposed by MM. Mignon and Rouart (Carré's system) has been selected by a Committee, and will be worked out. According to the Report (*Bulletin de la Soc. d'Encouragement*), Prof. Brouardel imposed three conditions: (1) to submit bodies, on arrival, to a temperature of $-15°$ to $-20°$ C. (this on account of bad conductivity and slowness of freezing internally, also the advanced state of decomposition often met with); (2) to take them into a room with temperature varying between $-4°$ and $-1°$; and (3) to keep ten bodies at a temperature of $-4°$. Further, vibration was to be avoided, and the air kept still. The method of Carré, it is known, depends on changes in an aqueous solution of ammoniacal gas, the gas being driven off by heat, liquefied by its pressure, vaporized, and absorbed by water. Chloride of calcium is used to transmit the cold; this liquid passing through pipes in the wooden walls of a freezing cell, into which the body is pushed on a carriage. Ten hours is enough for the largest body: it becomes hard as wood. The after-process is easier. Bodies can be kept thus more than eight months, though decomposition had begun before freezing. When an autopsy is to be made, the body is put into a case which is heated with gas burners, and afterwards it may be relegated to the frozen state to be kept longer. To keep bodies at $-2°$ in a hall, for exhibition to the public, presented special difficulties. How these were overcome may be learned from the above-mentioned Report.

THE new number of the Journal of the Marine Biological Association of the United Kingdom (new series, vol. ii., No. 2) opens with the Council's report for 1890–91 and the Director's report. The weather was extremely unfavourable for continuous and systematic dredging; nevertheless the boats of the Laboratory were constantly employed on every suitable day, and a considerable amount of material was collected. The preservation of specimens has been much more carefully attended to than formerly. One man now devotes almost his entire time to this work. The following are the other contents of the present number:—The egg and larva of *Callionymus lyra*, by J. T. Cunningham (with plate v.); experiments on the production of artificial baits, by Frank Hughes; the rate of growth of some sea fishes and their distribution at different ages, by J. T. Cunningham; on some Ascidians from the Isle of Wight—a study in variation and nomenclature, by Walter Garstang (with plates vi. and vii.); on the development of *Palinurus vulgaris*, the rock lobster or sea crayfish, by J. T. Cunningham (with plates viii. and ix.); the reproduction and growth of the pilchard, by J. T. Cunningham (with plate x.); the distribution of *Crystallogobius nilssonii*, by J. T. Cunningham; physical investigations, preliminary paper, by H. N. Dickson (with plate xi.); notes on meteorological observations at Plymouth, by H. N. Dickson; notes on the herring, long-line, and pilchard fisheries of Plymouth (continued), by William Roach, Associate Member; note on a British Cephalopod—*Illex eblanæ* (Ball), by William E. Hoyle; notes and memoranda.

PROF. KAUFMANN, of Liége, has issued a useful "Student" Guide" to the Liége School of Mines and Engineering, the Montefiore Electro-technical Institute, and the principal engineering firms in Liége and the environs. He quotes from an official report by Mr. Vice-Consul Menzies a statement to the effect that the advantages offered by Liége from an educational point of view do not seem to be duly appreciated in the United Kingdom. While the youth of almost all the other European nations are fairly, and in some instances largely, represented at the Liége University, the British students rarely number more than five or six at a time, and sometimes not even that.

A "Handy List of Books on Mines and Mining" has been compiled and published by Mr. H. E. Haferkorn, of the Milwaukee Public Library. He describes it as an alphabetical reference catalogue, arranged under authors and subjects, and including analytical references to the contents of important works.

MESSRS. WHITTAKER AND CO. have issued the fourth edition of the "Working and Management of an English Railway," by George Finlay. In June 1890 the author read a paper at the Royal United Service Institution, on the transport of troops by rail within the United Kingdom. The substance of this paper he has embodied in the chapter on railways as a means of defence. To the present edition he has also added, as an appendix, a lecture (with emendations) delivered at the Society of Arts, on modern improvements of facilities in railway travelling.

MESSRS. WHITTAKER AND CO. have in the press a second edition of Dr. A. B. Griffiths's "Treatise on Manures." It is a little more than two years since the work appeared. Fifty pages of new matter have been added.

THE third edition of "Electricity, treated Experimentally for the Use of Schools and Students," by Linnæus Cumming, has been published by Messrs. Longmans, Green, and Co. The author has made such additions and alterations as seemed necessary to bring the book up to date.

A NEW edition of Prof. A. Humboldt Sexton's "Elementary Inorganic Chemistry" (Blackie and Son) has been issued. To meet the alterations in the syllabus of the Science and Art Department, the author has recast the part dealing with qualitative analysis.

THE American Association for the Advancement of Science has just issued the Proceedings of its meeting (the thirty-ninth) held at Indianapolis, Indiana, in August 1890.

PART 38 of Cassell's "New Popular Educator" has been published. Besides the illustrations in the text, it includes a good map of Spain and Portugal.

THE second series of lectures given by the Sunday Lecture Society begins on Sunday afternoon, December 6, in St. George's Hall, Langham Place, at 4 p.m., when Mr. Eric S. Bruce will lecture on "Fogs and their Prevention." Lectures will subsequently be given by Prof. J. F. Blake, Prof. Vivian B. Lewes, Prof. Percy Frankland, F.R.S., Dr. Benjamin W. Richardson, F.R.S., Mr. Whitworth Wallis, and Mr. Willmott Dixon.

THE additions to the Zoological Society's Gardens during the past week include a Barbary Mouse (*Mus barbarus*) from Barbary, a Chinese Blue Magpie (*Cyanopolius cyanus*) from China, two Brown Thrushes (*Turdus leucomelas*) from South America, purchased; a Vulpine Phalanger (*Phalangista vulpina*), born in the Gardens.

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OUR ASTRONOMICAL COLUMN.

MOTION OF STARS IN THE LINE OF SIGHT.—In a paper read before the Royal Society in January 1890, Prof. Lockyer described a new method of observing spectra of stars and nebulæ which did away with errors due to the collimator of the spectroscope not being exactly in the optic axis of the telescope owing to the flexure of the telescope tube. It consisted in using a siderostat to reflect the light of the body under observation to a vertical object-glass, whence it was converged on the slit of a fixed spectroscope. By this means perfect stability can be secured. This method has been utilized by M. Deslandres, of Paris Observatory, for the photographic determination of the displacements of lines in stellar spectra due to motion in the line of sight (*Comptes rendus*, November 23). Comparison spark spectra are taken above and below the spectrum of the star, and the difference of position of the lines common to the star and these spectra afterwards measured. The elements used for comparison are iron, calcium, and hydrogen, and the best results have been obtained with the first of the three. The lines in a spectrum of Sirius, taken on March 3, 1891, in this manner, exhibited a displacement which corresponded to a velocity of recession relative to the earth of 19 kilometres per second. But as the earth's motion towards Sirius at the time of observation was 20·2 kilometres per second, the approach of the star to the sun was 1·2 kilometres per second. The results indicate that considerable advantage is to be gained by the use of the siderostat in the study of the radial motions of stars.

THE VARIATION OF LATITUDE.—Some determinations of the latitude of Cambridge, U.S., made in 1884–85 exhibited a progressive variation, from which, however, no inference was drawn at the time. The stars observed were contained between $-5°$ and $+5°$ of declination, but a subsequent discussion based on more northerly stars ($+5°$ to $+50°$) gave an exactly corresponding variation in latitude. Mr. S. C. Chandler, in the *Astronomical Journal*, No. 248, gives the results of a recent examination of his values, and from the curve connecting the residuals finds the minimum latitude to have been on September 1, 1884, and the maximum latitude on May 1, 1885, with a range of about 0″·7.

PHOTOGRAPHY OF THE ECLIPSED MOON.—During the lunar eclipse of November 15, M. Courty, of Bordeaux Observatory, took four photographs of the moon after it had entered the earth's shadow. The exposure given was about two minutes, and the disk of the moon could be easily traced on the negatives, and on some positives presented with a note by M. Rayet to the Paris Academy on November 23. M. Janssen remarked that by photographing the eclipsed moon and the full moon on the same plate, and determining the times of exposure necessary to obtain both images of equal density, a good idea of the relation of the light intensity in the two cases may be obtained.

PROPOSALS FOR A SCHEME OF CO-OPERATIVE OBSERVATION OF THE SO-CALLED LUMINOUS CLOUDS.

SINCE 1885 curious cloud formations have been seen on summer nights in both the northern and southern hemispheres, in evident connection with those phenomena which followed the great volcanic eruption at Krakatáo. The intense brightness of these formations, considering the position of the sun, denoted that they were situated very far above the earth's surface. Probably these clouds consisted of erupted particles thrown to a very great height and there illuminated on summer nights by the sun.

These cloud-like formations, commonly called luminous clouds, are extremely interesting, both on account of the extraordinary height at which they have for years been moving above the surface of the earth (more than 80 kilometres) and of the movements themselves. A very important point about these clouds is that they are—so far as we yet know—visible in each hemisphere only in the summer. It is the more important that these phenomena should be carefully and widely observed, since it is believed that they are gradually breaking up, so that probably in a very few years no distinct traces of them may remain (see also O. Jesse on so-called luminous clouds, in the journal *Himmel und Erde*, vol. i. p. 263).

Photographic results of the researches of O. Jesse are given in Part xl. of the Transactions of the Berlin Academy of Science for 1890, and Part xxvi. for 1891. It is very desirable that such photographs should be taken in as many different localities as possible, because from them we get the surest basis for consideration of the situation and movements of the clouds. But valuable aid may be given by the co-operation of numerous observers in various regions of the earth without the aid of any apparatus.

The principal points upon which stress is to be laid in this inquiry are:—

(1) By what method can the so-called luminous clouds be most surely distinguished from others, especially from the ordinary cirrus cloud?

Clouds or cloud-like formations which after sunset and before sunrise stand out brightly from the dark ground of the heavens, no earthly or unearthly sources of light being present on the horizon, can only produce this effect by means of their own light or else by light which they receive directly or indirectly from the sun or moon below the horizon.

Cloud-like formations which shine at night by their own light have doubtless been formerly observed above the surface of the earth. To these formations belong not only thunder and lightning clouds, but also some polar light and meteoric phenomena.

But the so-called luminous clouds do not belong to the various species of self-luminous clouds, for finer measurements of their light are wanting, besides which the fact that they are only seen within the zone of twilight proves that the sun below the horizon is the principal source of their light.

It is well known that there are clouds within this twilight zone which resemble high mountain peaks, and which in the first stages of twilight shine in the light of the sun, though the latter is below the horizon of the observer. It is easy to determine the relation between the position of the sun below the horizon, and the height of those layers of atmosphere which receive the sun's light and reflect it.

But the laws which govern the whole course of twilight are modified when the distribution of the sunlight-reflecting particles in the atmosphere is altered to any great extent. If, for instance, numerous minute atoms produced by volcanic eruption or by the breaking up of meteoric bodies find their way into those heights above the earth's surface in which usually the gaseous elements of the atmosphere are present in a very scattered form, it may happen that such a layer, which reflects the sunlight very strongly, may curiously alter the course of the twilight.

So long after sunset as the masses of air beneath such a layer receive direct light from the sun and reflect it, the observer will not distinguish any deviation from the usual course of twilight. But as soon as the further sinking of the setting sun gradually deprives the lower layers of air of the direct light, the higher layer of dust still receiving light from the sun stands out in astonishing brightness, the particles of dust having strong reflecting power, thus giving to the close of twilight the curious effect of the sudden appearance of shining clouds on the broad surface of the heavens.

The phenomena of the luminous clouds corresponded when first perceived to the above description. At present they are no longer so strong or so extensive, but only form thin whitish-blue shining veils, similar in form to the so-called cirrus or feather clouds, occupying but a comparatively small part of the floor of the heavens inside the twilight segment, and in our zone mostly near the horizon. Probably, the layers are now so thin that very near and exactly above us they can no longer be seen.

From the above considerations it is clear in what way these clouds differ from those situated nearer to us, and especially from the cirrus clouds floating scarcely more than 13 kilometres above the earth's surface. All these lower clouds appear in the later twilight grey and shadowy on a light ground, because the layers of atmosphere above them are the chief source of the remaining twilight. The luminous clouds differ too in shape and structure from the other kinds of clouds.

We must guard, however, against the error of mistaking cirrus for luminous clouds, when, in exceptional cases, the former look very bright, in consequence of receiving light either directly or indirectly from the moon or other sources. In this case, the question is decided by the relatively high degree of stability in position and form of the very high and distant luminous clouds, as ordinary clouds lie lower and nearer, and show much more rapid changes of position.

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(2) When convinced of beholding so-called luminous clouds, to what points shall attention be especially directed, and what simple measurements of place, time, form, &c., shall be carried out in order to aid most usefully in the inquiry?

In answering this question we will first consider those methods of research in which the observer can obtain no instrumental aid, except only a watch, which should be a sufficiently good timekeeper to estimate the time of observation to one minute, when compared with the correct time within eight to twelve hours after the observation.

Such simple observations are the more useful, since it frequently happens that in the well fitted up and prepared stations, observation of the phenomena is prevented by bad weather, or else that the phenomena stretch over too large an extent of the earth's surface to be included in an organized series of observations. The farther the stations are apart, the more valuable are the most simple methods. For instance, in order to get corresponding photographic observations from two stations, 35 kilometres apart, such as Berlin and Nauen, the most rigid exactness, both as to time and place, must be observed.

If, however, observations are taken in East Prussia and in the Rhine province respectively, a from twenty to thirty times larger margin of difference as to time and place can be allowed than in the foregoing case, without in any way lessening the value of the result.

So, if without preparation and instruments to hand an observer believes he beholds luminous clouds, he must not imagine that he can render no service to science by examining them closely, for very possibly the most simple method may, taken in conjunction with other similar observations, prove to be of the greatest service.

It is desirable, too, to look out for luminous clouds at all seasons of the year, though, so far, they have only been seen in summer. In the northern hemisphere they have only been seen from the end of May to the beginning of August, with greatest frequency and brightness in the month of July.

During these weeks, usually two stars are seen simultaneously with the luminous clouds, a star of the first magnitude, Capella, and a star of the same constellation, of the second magnitude, β Aurigæ.

The brighter of the two stars, which is characteristic of summer nights, in the northern horizon, sets towards the end of June soon after eleven, and towards the middle of July before ten, on account of the northerly direction of the meridian, and, in North Germany, at a distance from the horizon of 10 to 12 diameters of the full moon. At almost as great a distance from this bright star, and at a not very different distance from the horizon, the second magnitude star follows towards the west.

By estimating the distances and directions of these two stars, an excellent means is afforded of determining the outlines of a group of luminous clouds. It is only necessary to determine how great the distance of a certain part of the outline of the cloud group is from one or the other star, and in what direction this line lies with regard to one or the other star, or how far the line in question is above or below the prolongation of the connecting line of the two stars. A simple drawing of the course of the outlines and their situation with regard to the two stars is useful, even when it cannot be completed on the spot but must be finished from memory. The time at which the drawing was made should be noted within one half-minute.

If the group of clouds should be so far from the above-mentioned two stars as to make the determinations inexact, it is advisable to determine the outlines of the clouds for a certain time in the following way. Take up a position from which the outlines of houses, trees, &c., can be seen close to the position of the clouds, and fix thus the relative position of these earthly objects to the position of the clouds by a simple drawing, describing the spot from which the observation is made in such a manner that the place occupied by the head of the observer can be found again. The lines drawn from the position of the observer to the outlines of the earthly objects, and the resulting localization of the outline of the clouds in the heavens can then be determined at once by means of simple instruments for measuring angles, or on succeeding nights by the aid of a good star chart.

It is necessary to verify the exact point of time of these observations by comparison of the watch used with the time at a telegraph office, and correction of any errors should be made to the fraction of a minute.

In communicating these observations, the exact place at which they have been made must be accurately described.

Should a complete observation be impossible, owing to the time during which the luminous clouds are visible being too short for careful measurements and drawings or to any other cause, the observer should nevertheless communicate briefly to the Society of Friends of Astronomy and Cosmic Physics that he has seen what he believes from the foregoing considerations to be luminous clouds from a certain place, in a certain direction in the heavens, and within a certain quarter-hour.

The peculiar movements hitherto observed of the clouds in question lead to the suggestion that perhaps a period consisting of several days exists, within which one and the same group of clouds is visible at the same hour from the same place, other conditions of the heavens being favourable. Every communication as to these phenomena will be valuable in the decision of this important point, which it has hitherto been impossible to settle, owing to the uncertainty of the weather and the fewness of the observers.

Those co-operating in our branch of research who are in possession of astronomical, photographic, or other physical apparatus, will of course be able to give more exact details as to place, movement, and constitution of the luminous clouds.

Suggestions for these observations cannot be given so briefly and simply; but for the sake of full and complete agreement between different observers, especially as to the point of time selected for taking photographs and measurements, members of the Society of Friends of Astronomy and Cosmic Physics are invited to communicate with O. Jesse, Steglitz bei Berlin, Albrechtsstrasse 30. This course would also be advisable in the close optical examination of the clouds with regard to the peculiar changes in strength of light and the degree and kind of self-luminosity which they perhaps send out together with the reflected sunlight.

In the night from June 25–26 of this year the summer reappearance of the luminous clouds was observed very brightly from Berlin and the neighbourhood.

More detailed particulars on the whole subject of inquiry are contained in a small paper by W. Foerster, which has been sent to all the members of the Society of Friends of Astronomy and Cosmic Physics.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—Mr. G. C. Inge, Magdalen College, has been appointed to the Studentship offered to the University by the Managing Committee of the British School of Athens, from the Newton Testimonial Fund.

The death is announced of Dr. Evan Evans, Master of Pembroke College, who filled the office of Vice-Chancellor of the University from 1878–82.

Convocation has granted £25 towards the cost of the antiquarian researches at Chester, which are throwing great light upon the obscure period of the military occupation of Britain in the time of Agricola. Prof. Mommsen has appreciated the value of these researches.

At a meeting of the Junior Scientific Club, Mr. A. Colefax, Christ Church, read a paper on the investigation of the change taking place in acidified solutions of sodium thiosulphate. The subject of hypnotism was treated by Mr. E. L. Collis, of Keble; and P. C. Mitchell had an exhibit, and offered some remarks concerning primitive man in the Torquay caves.

The University has published the official Calendar for 1892. The arrangement and information contained differ little from former years. We learn that the number of undergraduate members of the University has increased from 3110 to 3212. The number of matriculations in 1890 were 771, as compared with 787 in the preceding year. The number of B.A. and M.A. degrees is very nearly the same as in 1889.

SCIENTIFIC SERIALS.

American Journal of Science, November 1891.—The solution of vulcanized india-rubber, by Carl Barus. Experiments have been made by the author on the solubility of india-rubber in different solvents at different temperatures. Elastic sheet india-rubber, such as is used for rubber bands and tubing, is not fully soluble in CS_2 at 100° or 160°, but is quite soluble at 185°, and extremely soluble at 210°. It is also easily dissolved by liquids of the paraffin series at 200°. Various other substances

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have been used as solvents, and many remarkable results obtained. The importance of the paper may be gathered from the fact that in it is described "a method by which vulcanized india-rubber of any quality or character whatever, as well as the undecomposed or reclaimable part of rubber-waste, may be dissolved or liquefied in a reasonably short time, the solutions possessing any desirable degree of viscosity or diluteness, from which india-rubber may be regained on evaporation of the solvents."—Report of the examination by means of the microscope of specimens of infusorial earths of the Pacific coast of the United States, by Dr. Arthur M. Edwards. Seven new fluviatile fossiliferous deposits from Oregon, California, and Washington are described.—The Tonganoxie meteorite, by E. H. S. Bailey. An analysis of the meteorite gave the percentage composition: Fe 91·18, Ni 7·93, Co 0·39, P 0·10, and a trace of copper. The weight is 23¼ lbs., specific gravity 7·45, shape an irregular triangular pyramid 9½ inches long by 6½ inches wide by 4½ inches deep. A fine figure showing numerous pittings on the surface of the meteorite accompanies the paper.—Proposed form of mercurial barometer, by W. J. Waggener.—Colour photography by Lippmann's process, by Charles B. Thwing. The results obtained seem to indicate—(1) that mixed colours may be reproduced with a fair degree of accuracy; (2) that an exposure sufficiently long to give a clear image of the red is quite certain to obliterate the blue by over-exposure; and (3) that an over-exposure may completely reverse the colours, causing the original colours to appear on the reverse, and the complementary colours on the film side of the plate.—New analyses of uraninite, by W. F. Hillebrand. From the analyses it appears that the species may be broadly divided into two groups, one characterized by the presence of rare earths and the almost invariable presence of nitrogen, the other containing little or no nitrogen and no rare earths. Varieties of the former group occur in more or less well defined crystals, whilst members of the latter group are usually devoid of crystalline form.—The Tertiary silicified woods of Eastern Arkansas, by R. Ellsworth Call. The investigation has led to the following conclusions:—(1) The silicified woods of Eastern Arkansas are all of Tertiary age. (2) They are derived from the beds of Eocene clays that underlie the sands and gravels in which they commonly occur. (3) They are silicified lignite; the process of silicification has occurred either while they were still in clays, or most often after they were removed and buried in the sands or gravels. (4) They possess as yet no taxonomic value in determining the relative ages of the members of the Tertiary series.—Occurrence of sulphur, orpiment, axd realgar in the Yellowstone National Park, by Walter H. Weed and Louis V. Pirsson.—Mineralogical notes, by L. V. Pirsson. Some specimens of cerussite, hæmatite, and cassiterite, gypsum, and pennine are described.—Peridot dykes in the Portage sandstones, near Ithaca, N.Y., by J. F. Kemp.—A new locality of meteoric iron, with a preliminary notice of the discovery of diamonds in the iron, by A. E. Foote. The existence of black and white diamonds in the meteorite appears to be established by indifference to chemical agents and hardness. Carbon in the form of an iron carbide also occurs with the diamonds. The meteorite was found in Cañon Diablo, Arizona. Three figures accompany the paper.—The South Trap Range of the Keweenawan series, by M. E. Wadsworth.—Geological facts noted on Grand River, Labrador, by Austin Cary.

SOCIETIES AND ACADEMIES.

LONDON.

Mathematical Society, November 12.—Prof. Greenhill, F.R.S., President, in the chair.—The President announced the recent decease of Mr. H. M. Jeffery, F.R.S., who was elected January 14, 1875.—The following gentlemen were elected to serve on the Council for the ensuing session: Prof. Greenhill, F.R.S., President; Dr. J. Larmor, Major P. A. MacMahon, F.R.S., and J. J. Walker, F.R.S., Vice-Presidents; A. B. Kempe, F.R.S., Treasurer; M. Jenkins and R. Tucker, Hon. Secs.; other members, Messrs. A. R. Basset, F.R.S., E. B. Elliott, F.R., S. J. Hammond, C. Leudesdorf, A. E. H. Love, S. Roberts, F.R.S., Drs. A. R. Forsyth, F.R.S., J. W. L. Glaisher, F.R.S., and M. J. M. Hill.—The following communications were made:—On selective and metallic reflection, by A. B. Basset, F.R.S. It is well known that most transparent substances, which produce anomalous dispersion, exercise a strong selective absorption, and at the same time strongly reflect rays of the same periods as those which they absorb. Thus in fuchsine the order of the colours going up the spectrum is blue, indigo, violet; then there is an absorption band, followed by red, orange, yellow. The experimental laws relating to substances of this class may be summarized as follows: (1) the rays which are most strongly absorbed, when light is transmitted through the substance, are most strongly reflected; (2) when the incident light is plane polarized in any azimuth, the reflected light is elliptically polarized; (3) when sunlight is reflected, the colour of the reflected light, when viewed through a Nicol's prism whose principal section is parallel to the plane of incidence, is different from what it is when viewed by the naked eye. The phenomena of absorption, anomalous dispersion, and the like, have formed the subject of numerous theoretical investigations by German mathematicians. It is not the object of the present paper to propose any new theory upon the subject, but to discuss and extend the theory of von Helmholtz. The theory of von Helmholtz is an elastic-solid theory, which is based upon certain assumptions respecting the mutual reaction of ether and matter. The potential energy of the system may be conceived to consist of three distinct portions, viz. W_1, W_2, W_3, of which W_1 is the ordinary expression for the potential energy of an isotropic elastic solid; W_2 is a homogeneous quadratic function of the displacements of the matter; and W_3 is a similar function of the relative displacements of ether and matter, and is supposed to arise from the mutual reaction of ether and matter. Having obtained the expression for the energy of the system, the equations of motion can be at once written down; and it will be found, on integrating them, that the index of refraction, μ, of light of period τ, is given by the equation—

$$\mu^2 = \frac{\rho}{\rho_0} - \frac{a^2\tau^2}{4\pi^2\rho_0}\left\{1 + \frac{a^2k^2\tau^2}{4\pi^2\rho_1(k^2-\tau^2) - a^2k^2\tau^2}\right\} \quad . \quad (1)$$

In this equation ρ is the density of the ether when loaded with matter, ρ_0 is the density of the ether *in vacuo*, and ρ_1 is the density of the matter; k is the free period of the matter vibrations, and a is a constant depending on the mutual reaction of ether and matter. If we suppose that the value τ_1 of τ, which makes the denominator vanish, corresponds to the double sodium line D of the spectrum, whilst a value τ_2, which makes $\mu = 0$, corresponds to the hydrogen line F, μ^2 will be negative when τ lies between D and F, and (1) accordingly represents a transparent medium (such as fuchsine) which has a single absorption band in that portion of the spectrum. Moreover, the dispersion is anomalous, since the value of μ when τ is a little greater than τ_1, is much greater than its value when τ is a little less than τ_1. To explain selective reflection, I have provisionally adopted Sir W. Thomson's hypothesis, that the ether is to be treated as an elastic medium, whose resistance to compression is a negative quantity, whose numerical value is slightly less than ⅓rds of its rigidity. Under these circumstances, the amplitudes of the reflected light will be given by Fresnel's sine and tangent formulæ, according as the incident light is polarized in or perpendicularly to the plane of incidence. When μ^2 is a negative quantity, these formulæ become complex quantities of the form $\epsilon^{-2i\pi f/\lambda}$ and $\epsilon^{2i\pi f_1/\lambda}$, and this indicates that reflection is total, and is accompanied by a change of phase; moreover, since the changes of phase, f, f_1, are different, according as the incident light is polarized in or perpendicularly to the plane of incidence, it follows that if the former is polarized in any azimuth the reflected light will be elliptically polarized. From these results it appears that the colour of the reflected light is of a greenish yellow when viewed by the naked eye; but when it is viewed through a Nicol, whose principal section lies in the plane of incidence, a considerable portion of the yellow rays are refused transmission by the Nicol, and the light under these circumstances is of a much richer green. Cauchy's formulæ for metallic reflection may be obtained from Fresnel's sine and tangent formulæ, by assuming that μ ($= \sin i/\sin r$) is a complex quantity of the form $Re^{i\alpha}$; but the experiments of Jamin, and the calculations of Eisenlohr, show that the real part of μ^2 must be *negative*, which requires that α should lie between 45° and 90°. In fact, for silver, Eisenlohr finds that $\alpha = 83°$. Lord Rayleigh, on the other hand, has shown that, if we attempt to explain metallic reflection by introducing a viscous term into the ordinary equations of motion of an elastic solid, physical considerations require that the real part of μ^2 should be *positive*; he has also shown that a similar objection lies against attempting to explain metallic reflection on the electro-magnetic theory,

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by taking into account the conductivity. If, however, we start with von Helmholtz's theory, and introduce a viscous term into the *equations of motion of the matter*, it will be found possible for the real part of μ^2 to be negative, provided the free period of the matter vibrations lies between certain limits. We are thus able to construct a mechanical model of a medium which represents the action of metals upon ethereal waves, and which leads to the same formulæ for the amplitudes of the reflected waves as those given by Cauchy.—The contacts of systems of circles, by A. Larmor.—On a class of automorphic functions, by Prof. W. Burnside.—Note on the identity $4(x^6-1)/(x\ 1) = Y^3 \pm pZ^3$, by Prof. G. B. Mathews.—On the classification of binodal quartic curves, by H. M. Jeffery, F.R.S.—Researches in the calculus of variations; discriminating conditions in isoperimetrical problems, by E. P. Culverwell.—Note on Clifford's paper "On syzygetic relations among the powers of linear quantics," by Prof. Cayley, F.R.S.—Note on finding the G points of a given circle with respect to a given triangle of reference, by J. Griffiths.

Linnean Society, November 19.—Prof. Stewart, President, in the chair.—Mr. S. Jennings exhibited a collection of wild flowers made by him during a recent tour through the Rocky Mountains, California, and Mexico.—Prof. G. B. Howes exhibited some dissections of fish crania made by his pupil Mr. R. H. Burne, in which the parts of the skeleton were so displayed that they might be studied in relation to the rest of the head and to the leading cranial nerves.—Mr. E. F. Cooper exhibited specimens of a new variety of *Potamogeton* from Loughborough, lately described and figured by Mr. Alfred Fryer (*Journ. Bot.*, October 1891).—Mr. A. W. Bennett exhibited and made remarks upon some specimens of *Hydrodictyon utriculatum*, Roth. (*H. reticulatum*, De Toni), and some drawings of anomalous *Cypripedium* and *Disa*.—Mr. W. Carruthers, F.R.S., gave a graphic account of a recent visit to Sweden in search of original portraits of Linnæus, and detailed the results of his inquiries. His remarks were illustrated by an exhibition of engravings and photographs.—A paper was then read by Mr. Thomas Hick, on a new fossil plant from the Lower Coal-measures. An interesting discussion followed, in which Mr. Carruthers, Mr. G. Murray, Prof. F. O. Bower, Prof. Marshall Ward, and others took part.

PARIS.

Academy of Sciences, November 23.—M. Duchartre in the chair.—On some manuscripts with figures of historical interest relating to artillery and mechanical arts towards the end of the Middle Ages, by M. Berthelot. Some manuscripts from libraries at Munich, Venice, and Paris have been examined, and appear to be of interest as marking a stage in the development of applied sciences. A few of the mediæval figures are reproduced: one represents a diver in his costume; two others show primitive cannon, and one a small-arm used in the fifteenth century.—Preparation and properties of the phosphides of boron, by M. Henri Moissan. By the use of boron phosphoiodide, two boron phosphides may be obtained. The compound PB combines with HNO_3, H_2O with incandescence, and inflames in an atmosphere of chlorine in the cold. The compound P_3B_5 is much more stable, and is not acted upon in the cold by these two reagents.—On some variations of the glycolytic power of the blood, and on a new method of experimental production of diabetes, MM. R. Lépine and Barral.—M. A. Potier was elected a member of the Academy in the place of the late M. Edmond Becquerel.—*Résumé* of a verbal report on a note by Prince Tourquistanoff, entitled "Le Calendrier vérificateur," by M. Wolf.—*Résumé* of a verbal report on a note by M. de Cohorne, entitled "Le Régleur solaire," by M. Wolf.—Observations of the total eclipse of the moon of November 15, made at Bordeaux Observatory, by M. G. Rayet.—Remarks *à propos* of the observation of M. Rayet as to the possibility of photographing the moon during a total eclipse, by M. A. Gautier.—Remarks on M. Rayet's communication, by M. J. Janssen.—Researches on the motions of stars in the line of sight made with the Paris Observatory siderostat, by M. Deslandres. (For the last four communications, see Our Astronomical Column.)—Remark on a communication by M. Markoff relative to linear differential equations, by M. Painlevé.—On the flow of liquids in capillary tubes, by M. Albert Colson. The influence of temperature on the rate of flow of viscous liquids is seen from the following comparison of the times in which 5 c.c. of glycerine passed through the tube:—

Temperatures	... 21°	... 100°	... 150°	... 250°	... 265°
Duration of flow	... 8h.	... 360s.	... 114s.	... 40·5s.	... 33s.

The same tube passed 5 c.c. of water at 20° in 34 seconds. The author divides the liquids he has experimented upon into two classes, distinguished by being perfectly and imperfectly mobile. Ethers and aldehydes are representatives of the former class, for they appear to obey Graham's law that the duration of flow, or rather the rate of diffusion, is inversely proportional to the square root of the density. The values found for $\frac{t}{\sqrt{D}}$ in this class of liquids is practically constant. On the other hand, the liquids *imparfaitement mobiles*, such as alcohols and benzines, furnish irregular values.—Mechanical determination of the position of the atoms of hydrogen in organic compounds, by M. G. Hinrichs.—Aniline black in dyeing by the dry method, by M. A. Gautier.—On a codeïne violet, by M. P. Cazeneuve.—On the distribution of saccharine matters in the different parts of the edible *Cèpe* (*Boletus edulis*, Bull.), by M. Em. Bourquelot.—On the existence of veins of leucite in a Mont Dore basalt, by M. A. Lacroix. Leucite has not before been recognized in any of the volcanic rocks of the central plateau of France. The author fully describes the petrological characters of the specimens he has discovered, and also their peculiar mode of occurrence.—Earthquakes, submarine eruption, and elevation of land at Pantellaria, by M. A. Riccò. Earthquake shocks were felt at Pantellaria on October 14. On October 18, the sea to the west-north-west of the island, at a distance of about 5 kilometres, was seen in violent commotion, and a band of land about 1 kilometre long appeared, from which were ejected masses of heated rock and vapour. On approaching the place of eruption, a large number of dead fishes were found, and it was seen that the band was composed of an immense number of black floating masses of rock colliding together with great noise, and vaporizing the water over which they passed. On October 23 the position of the erupted island was determined as lat. 36° 50′ N., long. from Paris 9° 33′ E. The island was then about 200 metres long by 50 metres wide. The interior of some of the masses of rock was still hot enough to melt zinc.

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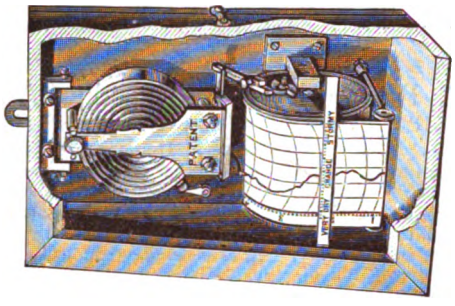

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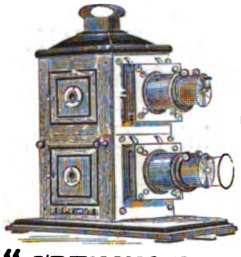

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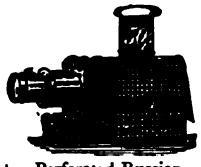

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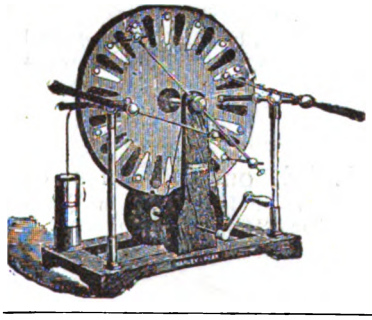

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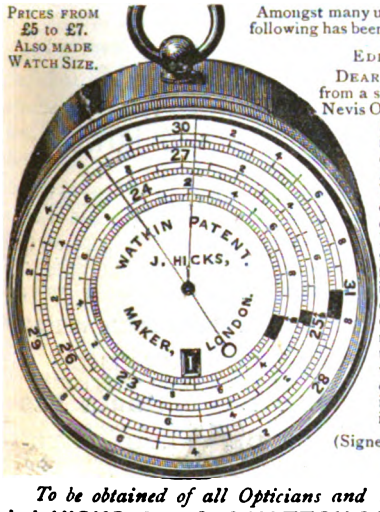

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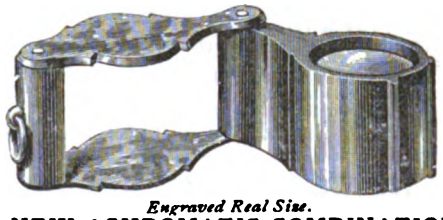

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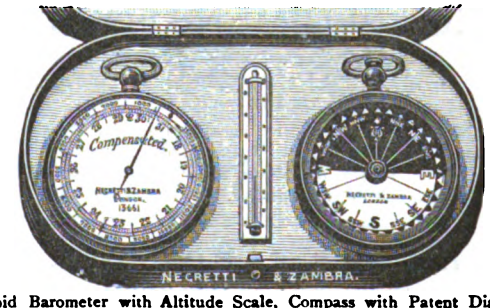

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THURSDAY, DECEMBER 10, 1891.

GERMAN TECHNOLOGY FOR ENGLISH MANUFACTURERS.

A Theoretical and Practical Treatise on the Manufacture of Sulphuric Acid and Alkali. By George Lunge, Ph.D. Second Edition. Vol. I. Sulphuric Acid. (London : Gurney and Jackson, 1891.)

The Alkali-maker's Hand-book. By George Lunge, Ph.D., and F. Hurter, Ph.D. Second Edition. (London : Whittaker and Co., 1891.)

SULPHURIC acid plays a part, directly or indirectly, in every manufacturing industry of civilized countries. It has been said, indeed, that one could gauge the civilization of a country by the amount of oil of vitriol it consumes. It is satisfactory, therefore, to know that Great Britain produces annually nearly a million tons of this civilizing agent, or an amount but slightly less than that made by all the rest of the world.

It is, however, a remarkable and not very creditable fact that no English or Scotch man has been at any particular pains to give his fellow-men an adequate description of the details of manufacture of this civilizing agent. Of course a certain number of books of a kind have appeared ; but it has been left to a German Professor to give us the first complete monograph on the subject. No one is more competent than Prof. Lunge to write authoritatively concerning the manufacture of sulphuric acid. For eleven years previous to his election to the Professorship of Technical Chemistry at the magnificently equipped Polytechnic School which the forethought and patriotism of the Swiss Government have caused to be erected at Zürich, Dr. Lunge was the manager of a large alkali-works in the north of England; and he has added to the experience thus gained by numerous visits to the other alkali manufacturing districts of Great Britain, and to those of Belgium, France, Germany, and Austria. His work, however, is not wholly based on the results of personal observation ; it reflects, in fact, the existing state of chemical literature on the subject, for practically every important memoir or communication, wherever published, bearing on the manufacture, properties, or uses of sulphuric acid, is referred to and judiciously criticized. Moreover, the author's position, as director of one of the most modern and in many respects one of the best-appointed laboratories in the world, gives him unique advantages in the compilation of such a work ; for, surrounded as he is by a band of earnest and enthusiastic workers, eager to aid him in elucidating the theory of established chemical manufactures or in investigating the validity of new processes, he is able to throw light on many obscure reactions by the systematic researches which he initiates, and of which the results, so far as they relate to sulphuric acid or collateral matters, are set forth in this book. It has been frequently observed that, although iron and oil of vitriol are among the most important of our staple products, we know comparatively little of the many chemical reactions which are concerned in their formation. The remark has, however, lost much of its force within recent years, and more especially in the case of sulphuric acid. During the last few years the various changes occurring within the leaden chamber have been more carefully traced, and much definite information has been gained as to the natur of the interactions which result in the production of oil of vitriol. For not a little of this information we are indebted to Prof. Lunge and his pupils.

The present edition of this work differs in many respects from its predecessor. The ten years which have elapsed since the appearance of the first edition have seen many important changes in the manufacture of acid and alkali ; and hence, with a view of bringing his treatise within reasonable compass, Prof. Lunge has been obliged to curtail much of the historical or merely retrospective portion of the work, and to omit matter which deals with views and theories which may now be considered obsolete. In spite of all this, the book has greatly increased in size, and nearly half the illustrations are new. A comparison of the two editions shows that every page has been carefully overhauled, and much fresh information is given, even on points which appeared to be settled and accepted. The present edition is remarkably free from press errors. We have only detected two : on p. 108, calcium "bisulphate," should read "bisulphite" ; and on p. 899 we read that SO_3 is formed when *limestone* is burnt in oxygen, especially at increased pressures ; of course "limestone" is a *lapsus calami* for "brimstone."

"The Alkali-maker's Hand-book," as it is now called, has an accepted position in the laboratory literature of the chemical works of this country. The book owes its origin to a suggestion, made by Mr. Stroof, manager of the Griesheim Alkali Works, to the German Society of Alkali-makers, that a standard manual should be published, with a view of securing uniformity in analytical methods, tables of specific gravity, &c., to be employed by buyers and sellers for the valuation of chemicals, and by manufacturers for controlling and superintending their various processes, in order to avoid disagreements, and to secure exact comparison of results. A small committee of the Society was appointed, and Prof. Lunge was commissioned to collect and sift the materials for such a manual. The present work is the outcome of this action. The great danger of a book of this kind is that it is apt to get stereotyped, in the extended and figurative sense of that word ; and that, owing to the natural conservatism of manufacturers, who are loth to disturb arrangements which are found to satisfy commercial necessities, there is the possibility that it may fail to reflect the state of quantitative analysis of the time. So long, however, as the work remains under the direction of Dr. Lunge and Dr. Hurter, there is very little chance of such a fate overtaking it. The present edition gives abundant evidence that care is being taken to make the book a faithful record of the condition of contemporary quantitative analysis. The work is conveniently arranged and well printed. We would take exception, however, to the character of the illustrations : these compare most unfavourably with those in Dr. Lunge's larger work. Simple outline drawings, like that of the nitrometer on p. 113, would be far preferable to the ill-drawn, ill-cut, and ill-printed designs which disfigure the book. Much of the value of the book depends, of course, on the care and accuracy with which the tables of chemical contents are compiled. We do not regard,

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however, the value 122 as the atomic weight of antimony, and we strongly protest against the continued use of 197·18 as the atomic weight of platinum. We are aware of the reasons which have led to the adoption of this value, but not even a "Potash Convention" has the right to play fast and loose with a stoichiometric constant to the extent of nearly 3 units from the truth. Similar tricks were played in the old days with the atomic weights of sodium and manganese for commercial purposes. Let the Potash Convention agree among themselves to adopt any correction they please on their analytical results with a view of rendering them more accurate; but they have no right to tamper with an atomic weight in order to compensate for the imperfections of their quantitative methods. For table 22, showing the volumes of water at different temperatures, we should have preferred to use the more accurate table of Rossetti, which gives the mean results of the observations of Kopp, Pierre, Despretz, Hagen, Matthiessen, Weidner, and Kremers, and thereby tends to eliminate errors due to the employment of a special method, such, for example, as the dilatometrical method. We may also point out that the table of solubilities of certain gases in water is not based on the most modern data. Bunsen's values for oxygen have been superseded by the more accurate numbers of Winckler, Dittmar, and Roscoe and Lunt, Sondén and Petterson; and the original statement, based apparently on the work of Carius, that hydrogen is equally soluble in water at all temperatures between 0° and 20°, has been shown to be erroneous by Bohr and Bock and Timofejew.

There is a passage in the preface of Prof. Lunge's larger work which may serve to indicate the difference with which the scientific aspect of his business is regarded by the English and Continental chemical manufacturer. In presenting his book to English manufacturers, Dr. Lunge ventures to express the hope that they will not think it too "scientific." He counsels them not to despise the purely chemical detail which they will find in it. There is, it may be thought, a certain element of humour in these remarks. But Prof. Lunge is very much in earnest. He very well knows that we are still ruled by the rule of thumb in these things; the "practical man" still dominates, and nothing but the inevitable adverse dividend will move him out of the way. To what consequences the neglect of a scientific treatment of a practical subject leads, Prof. Lunge illustrates from his personal experience. He tells us that he left his native country for Great Britain rather more than five-and-twenty years ago, because industrial chemistry was but little developed in Germany:—

"The manufacture of sulphuric acid, soda-ash, and bleaching-powder was at that time quite insignificant in Germany, and not very considerable in France as compared with Great Britain; nor could the technical appliances, the yields, or even the purity of the products in the two former countries vie with those of the latter."

How different matters are now is notorious:—

"The manufacture of chemicals has made enormous strides forward, both in quantity and quality, in France, and even more so in Germany. Many of the chemicals of these countries outstrip those of English works in purity; and their plant and their processes are frequently superior to those used in the majority of English works. Everybody knows how this has come about. The foreign chemists and manufacturers have looked all round, not merely in their own countries, but wherever they could find improved methods and apparatus; and upon the practical knowledge thus gained they have brought to bear the scientific training they had received at their Universities and Polytechnic schools. Thus they have already, in many fields formerly remunerative to British manufacturers, distanced the latter, immensely aided though these be by their long occupation of the ground, and by permanent natural advantages, such as cheapness of coal and freight, and their superior command of capital, &c.; and this is likely to go on to an increasing extent if many British chemical manufacturers decline to profit from a scientific study of their respective branches."

No one who has had the opportunity of comparing German chemical works with those of this country can be blind to the truth of these remarks. In certain branches of manufacture we are now absolutely distanced by the Germans, and in branches, too, which by priority of start and by every natural advantage ought to have been our exclusive possession. In the case of some of these we can hardly hope to recover our lost ground. Attempts have not been wanting, but it has to be admitted that British pluck and British capital have been hopelessly beaten by German energy and German capital *plus* German foresight and enlightenment. In some of these industries we may still hope to have a part, but it can only be a secondary one; and if things go on as hitherto, we must be content to be as the hewers of wood and the drawers of water. As we sow so shall we reap; and as we have sown little, it is but little that we may expect to garner.

The development of industrial chemistry in Germany during the last twenty years, and especially in those branches which depend upon the higher and more recondite branches of the science, has been amazing. In the manufacture of organic products the Germans and Swiss practically command the markets of the world; nor is there the least indication that their monopoly in the case of such products as demand scientific training and skill will be or can be assailed by us at present.

Some months ago the writer was required to inspect and report upon the best examples of modern chemical laboratories to be met with on the Continent, with special reference to the work of instruction in chemical research, and in the higher branches of chemical teaching. The advice he received was most significant, and illustrates very strikingly the attitude of the German chemical manufacturer towards the science of his business "If you want to see how organic chemistry should be worked at," said Prof. Kekulé, "go to some of our manufactories: they show us the way now. The men whom we have trained in our academic laboratories have bettered their instruction and teach their teachers." Precisely the same counsel was given by Prof. Victor Meyer: "Do you wish to see how chemical research can be organized? Then go to Ludwigshafen." And to Ludwigshafen we went. The mental impression of that spectacle of "organized research" on the banks of the Rhine will not soon be effaced. The sight, indeed, would constitute a useful object-lesson to the legislators who have sought to grapple with the subject of secondary education by handing it over to the country gentlemen. The very existence of

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such gigantic concerns as Meister, Lucius, and Brüning, and the Badische Anilin und Soda Fabrik, with their thousands of workmen, their splendidly equipped laboratories, and their scores of well-trained investigators, the product of the most advanced chemical instruction that the most eminent teachers in the world can impart, is a sufficient indication of what "the scientific treatment of a practical subject" leads to.

Some days after his visit to Ludwigshafen, the writer met Prof. von Baeyer at Munich, and the talk was of Höchst and Ludwigshafen, and the influence which these and many such places must have on the industrial position of Germany. "And do you know to whom we owe all this?" asked Baeyer. There was but one answer: "To Liebig." "You are right. It is to Liebig and the Giessen laboratory." What the Augustinian cell at Wittenberg was to German theology, the little University laboratory was to German chemical science.

"The foundation of this school," says Hofmann, who was himself one of its products, "forms an epoch in the history of chemical science. It was here that experimental instruction, such as now prevails in our laboratories, received its earliest form and fashion; and if at the present moment we are proud of the magnificent temples raised to experimental science in all our [German] schools and Universities, let it never be forgotten that they all owe their origin to the prototype set up by Liebig half a century ago."

Bureaucracies, being human institutions, have occasionally been known to err, but that bureaucrat who—by recalling the two Prussian students who had dared to seek the instruction in Hesse-Darmstadt which they failed to get in their own State—raised the storm of indignation which found eloquent expression in the famous letters that roused Germany and Austria to a sense of what science could do for their material interests, has deserved a better fate than oblivion. T. E. THORPE.

DIPHTHERIA.

Diphtheria: its Natural History and Prevention. By R. Thorne Thorne, M.B., F.R.S., &c. (London: Macmillan and Co., 1891.)

THE volume before us is a republication of the Milroy Lectures, delivered by Dr. Thorne Thorne before the Royal College of Physicians in London, 1891; and all must heartily congratulate the author on the ability with which he discusses a complex and vastly important subject, and at the same time must be grateful to him for having, by republication in a handsome form, made these lectures accessible to a larger public.

Diphtheria is an infectious disease which was known before the Christian era, and was fully recognized and well described by Bretonneau in 1821. In this country it has of late years undergone, both as to its diffusion and mortality, a remarkable increase. While in former years diphtheria was considered a purely "rural" disease, of late years its repeated occurrence in large towns has raised it to an "urban" disease; so much so that, "while the metropolitan (death) rate (from diphtheria) for 1861–70 was lower than that for the country generally, it exceeded it during the two next periods, and the rise which has taken place in the rate for 1881-89 is far in excess of the corresponding one for England and Wales." "There is thus far evidence that diphtheria as a cause of death is increasing in the country as a whole, and that this increase is very conspicuous in our greatest urban community" (p. 5). It is significant that, "concurrently (pp. 80–81) with the diminution of enteric fever, owing to advance of knowledge in the principles of health, and with the resulting intelligent administration of our sanitary laws, we find that the diphtheria death-rate is increasing in our midst." "But it is, above all, in our large towns and cities that this enlightened sanitary policy has been most marked during the past twenty years; . . . and yet, whereas when, in the past, sanitary defects abounded in our large centres of population diphtheria was essentially a disease of rural districts, that disease is now invading our more cleanly towns and cities to an extent unknown in the annals of their more faulty past."

Now, what is this increase of diphtheria in general, and the "formidable" increase in the London mortality from diphtheria in particular, due to? Although Dr. Thorne abstains from supplying a direct answer to this question, an attentive reader, after perusal of the enormous body of facts which Dr. Thorne produces, will be in a position to draw his own conclusions. This increase is certainly not to be explained by the better recognition and more correct classification of the disease (in the earlier returns of the Registrar-General certain forms of scarlatina, true diphtheria, and certain non-diphtheritic forms of croup are not well distinguished, in the later returns the distinction is carefully carried out), nor can this increase, obviously, be due to any new condition as to soil, water, and air. Dr. Thorne passes in review, and illustrates by numerous examples, collected by the most competent sanitary officers and inspectors, and minutely described in the Reports of the Medical Officer of the Local Government Board, the various conditions that have been, or were suggested as having been, connected with the origin and spread of various diphtheria outbreaks in this country; and a careful perusal of the immense body of facts recorded in this volume must impress the reader, not only with the great caution with which Dr. Thorne draws his conclusions, but with the admirably impartial way in which he tells his story, and in which he pays due regard to every detail, be it for or against. The one fact which above all others stands out prominently, and which it behoves everyone connected with our present system of compulsory school attendance carefully to consider, is the unmistakable influence of "school attendance" on diphtheria. Not the fact that diphtheria spreads from a child affected with diphtheria to another child with which it is brought in contact, either at school or at play or otherwise—a fact only too well known and unfortunately often enough actually illustrated; but the fact that "school influence"—that is, an influence affecting children aggregated in a confined space—has an important bearing on the generation of true diphtheria. This "'school influence' tends to foster, diffuse, and enhance the potency of diphtheria; and this, in part at least, by the aggregation of children suffering from that sore throat which commonly is prevalent antecedent to, and concurrently with, definite diphtheria" (p. 219). Dr. Thorne devotes a considerable portion of chapter iii. to the consideration and discussion of this important subject,

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and brings forward evidence, collected by himself before and after 1878, and by a number of well-known health officers and co-workers (Mr. W. H. Power, Dr. David Page, Dr. Jacob, Dr. Bruce Low, and others), which conclusively proves and confirms Dr. Thorne's proposition, first enunciated by him in 1878. Over and over again has it been shown (chapter iii.) that, in schools frequented by children some of whom were affected with simple sore throat, outbreaks of true diphtheria have occurred, for the explanation of which no antecedent case of diphtheria, nor any of the generally assumed insanitary conditions, could be brought forward. It is on these grounds that Dr. Thorne justly insists on a continued and careful inspection of the throats of the children, and on immediate separation from school of any child affected with sore throat.

The part that milk plays in the dissemination of diphtheria is fully discussed, and illustrated by a number of epidemics that have been recorded in the Reports of the Medical Officer of the Local Government Board; and the important relation of diseases of the lower animals, particularly of cows and cats, is also described and illustrated by epidemics in chapter iv. Last, but not least, Dr. Thorne considers the question of prevention and isolation. By his office as Assistant Medical Officer of the Local Government Board, and from an experience extending over many years, he stands in the unique position of the very best authority, whose conclusions and recommendations deserve carefully to be studied by managers and owners of schools, by hospital authorities, by sanitary officers and Boards of Health, by the owners of dairies, and by all those to whom the health of the community ought to be of paramount importance.

THE NEW YORK MATHEMATICAL SOCIETY.

Bulletin of the New York Mathematical Society, a Historical and Critical Review of Mathematical Science. Vol. I. No. 1, October 1891. (New York: for the Society.)

WE have occasionally wondered that now the study of mathematics is so diligently and successfully prosecuted across the Stream, there was no Society to bring together all such persons as were willing "to encourage and maintain an active interest in mathematical science." The "Organization of the New York Mathematical Society" gives a list of 174 members, mostly Professors of Mathematics or Astronomy. The President is Mr. Emory McClintock, a Vice-President of the Actuarial Society of America, who is also a member of the London Mathematical Society, and a contributor of some excellent memoirs to the *American Journal of Mathematics*. The constitution embraces six articles, and there are ten by-laws. These are apparently founded upon the rules which have been drawn up for other similar Societies. The date of the pamphlet (*i.e.* the "Organization, &c.," cited above) is June 1891. From a circular we gather that the Society has only recently inaugurated the present state of affairs, for this document states:—

"The New York Mathematical Society has consisted in the past of most of the professors and instructors of mathematics at the several Colleges situated in New York and the vicinity, the actuaries of a few of the larger life insurance companies, and a number of other persons interested in higher mathematics. At present an extension of membership is in progress."

Then it goes on to say:—

"The Society is about to undertake the publication of a periodical review of pure and applied mathematics. The idea is not to enter into any competition with the *American Journal of Mathematics*, the *Annals of Mathematics*, or any other similar journal, but it is proposed to publish, primarily, historical and critical articles, accounts of advances in different branches of mathematical science, and reviews of important new publications; also *résumés* of lectures before the Society, short contributions from members and correspondents, and general mathematical news and intelligence. Such a periodical, if circulated extensively, will do much to incite an interest in mathematical studies, and to maintain the interest of those who, having pursued such studies, are now perhaps at a distance from others of like tastes and training. It will appeal to many that our mathematical journals do not reach."

We have allowed the Society to state its aims: these have been sanctioned by Profs. Newcomb, Woolsey Johnson, and Craig (associate editor of the *American Journal of Mathematics*). We wish the Society every success in their endeavour "to promote a long-needed spirit of active co-operation, and to establish a bond of union between American mathematicians."

The *Bulletin* contains an article on "Octonary Numeration," by Prof. Woolsey Johnson. The concluding paragraph is as follows:—

"As there is no doubt that our ancestors originated the decimal system by counting on their fingers, we must, in view of the merits of the octonary system, feel profound regret that they should have perversely counted their thumbs, although Nature had differentiated them from the fingers sufficiently, she might have thought, to save the race from this error."

The rest of the number is taken up with reviews of several books, viz. "The Teaching of Elementary Geometry in German Schools" (review of Schotten's "Inhalt und Methode des planimetrischen Unterrichts," by Prof. Ziwet); Bertrand's "Calcul des Probabilités" (by Prof. Ellery Davis); Fine's "Number-System of Algebra" (by G. Enestrom, of Stockholm); and notices of works on West African longitudes and South American longitudes (by the treasurer, H. Jacoby). There are several short notes and a translation of Picard's demonstration of the general theorem upon the existence of integrals of ordinary differential equations (by the secretary, T. S. Fiske).

From this account it will be seen that there are no mathematical memoirs read before the Society in this part, but that such papers have been communicated we learn from the fact that three are printed in the current number of the *American Journal of Mathematics*, viz. one by C. Steinmetz (February 6, 1891), and two by the President (March 6, 1891).

OUR BOOK SHELF.

Delagoa Bay: its Natives and Natural History. By Rosa Monteiro. With Twenty Original Illustrations after the Author's Sketches and from the Natural Objects by A. B. and C. E. Woodward. (London: George Philip and Son, 1891.)

BOTANISTS and zoologists alike will remember the services rendered to science by the late J. J. Monteiro,

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in conjunction with his wife, both in western and eastern tropical Africa, and his modest volumes on Angola and the River Congo, dedicated to his partner in the pleasures and dangers of life in a tropical climate, and his zealous aid in the collecting of objects of natural history. He was one of the three almost contemporaneous discoverers of that very remarkable plant the *Welwitschia mirabilis*—the others being Welwitsch and Baines; and he sent some of the finest specimens of it in existence to this country.

After the loss of her husband, Mrs. Monteiro returned to Delagoa Bay, and spent five years in solitude, in the cottage built for her under happier circumstances, devoting her time to collecting insects, birds, and other natural objects, and studying the life-history of insects and their relations to plants. The present book is an unpretentious narrative of her life and labours during that period, and a record of her observations and her experiments in breeding insects, illustrated with some of her own discoveries in the animal and vegetable kingdoms.

A Hand-book of Industrial Organic Chemistry. By Samuel P. Sadtler, Ph.D. (Philadelphia: J. B. Lippincott Company, 1891.)

IN this book Prof. Sadtler has attempted to compress into about 500 octavo pages an account of those manufactures which depend upon the applications of organic chemistry. For what particular class of readers such a book is intended is rather difficult to determine. The scientific man is hardly likely to consult it in preference to the numerous special manuals to which he has access; and to the manufacturer the book is practically useless, owing to the comparative absence of all working detail. Considering the volume of literature which is required to give an approximately adequate representation of one industry alone—viz. the tar-colour manufacture—it would seem hopeless to expect anything of value from a chapter on the artificial colouring-matters, which, in well-leaded "roman spaced," attempts to give in 45 pages an account of the production and chemical nature of the numerous artificial and natural organic colouring-matters used in the arts, including their identification, chemical analysis, and detection on dyed fabrics. Certain of the other subjects are. it must be stated in fairness, treated with greater detail; and, as we should expect from Dr. Sadtler's connectionas an expert with the mineral oil industry, his description of the manufacture of petroleum and its associated products is reasonably complete. So also is the account of the cane-sugar industry. But, with the exception of the bibliographical and statistical information which occupies a relatively large share of the space devoted to each article, we see little else to commend. The book, however, is well got up; the paper and printing are all that can be desired, and the illustrations are, as a rule, much better executed than is usual in works of this class.

Progressive Mathematical Exercises. First Series. By A. T. Richardson. (London: Macmillan and Co., 1891.)

THE examples contained in this book are of the most elementary nature, and are intended for the use of those who have got no further than quadratic equations. In this series the exercises only deal with arithmetic and algebra, and are arranged in sets of papers which gradually become more difficult. The examples in arithmetic commence by dealing with the first four rules, simple and compound, and fractions; while those in algebra consist mostly of numerical values, addition and subtraction. Cube root and compound interest in arithmetic, and quadratic equations in algebra, form the highest limit to which these subjects are carried in this series. Throughout the work the author seems to have paid great care to insure accuracy in the answers; and though we have worked out many problems, picked out at random, we failed to find any errors.

We may mention that, in working through the papers, the beginner will occasionally come across examples which appear to be far above the average standard; but these, on trial, will always be found very simple, and are placed there with the intention of encouraging boys to look up methods they have not reached, and so to find that "a little research enables them to do a new sort of question."

Teachers and taught alike should find this book a useful adjunct to the text-book they have in use. W.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Implications of Science.

IT would be a great misfortune if such views as were expressed by Dr. St. George Mivart in a lecture delivered under the ægis of the Royal Institution, and reported at length in your columns (pp. 60 and 82), were allowed to pass unchallenged. In case no abler challenger appears, will you allow me to say a few words about "the implications of science"?

The great objection I take to Dr. Mivart's view is, that he does not appear to recognize any distinction between a real and a verbal truth. He apparently puts our knowledge of "the law of contradiction" into precisely the same category as our knowledge of "our own continuous existence," and draws but a slight distinction between these items of knowledge and such an item as the law of gravitation. Whereas, in fact, the so-called "law of contradiction" is not a *necessary* truth at all, it only expresses a verbal convention. It is not a law, but is of the nature of a definition. On the other hand, our knowledge of our own existence, *in the present*, comes to us by direct apprehension, and really is a "necessary truth" to each of us individually; though, since our knowledge of our existence in the past depends on the accuracy of our memories, this latter may easily be erroneous. That the memory exists is of course indisputable, but it may well be that the fact it professes to recall either took place differently, or even did not take place at all. Our confidence in our memories depends upon induction—ultimately on *inductio per enumerationem simplicem*—in just the same way as our belief in the law of gravitation does, and neither of these items of knowledge can therefore be necessary truths, though we may well hold them with so strong a conviction that the distinction may for practical purposes be ignored.

The "implications of science" which Dr. Mivart insists on are nearly all truisms (that is, purely verbal assertions)—all those to which he ascribes universal validity in any regions of time or space are such. I may repeat here what I have said elsewhere: "*The supposed peculiar certainty of mathematical conclusions is solely due to the fact that they are truisms.*"

For example, the assertion "Two straight lines cannot inclose a space" is certainly not a "necessary truth." Either its terms are defined by connotation, so that its truth depends solely on those definitions, or else its terms are defined by denotation, as representing real things in space, and the truth of the assertion can only be proved by induction from actual experience with those things. In the first case the truth is arbitrary, not necessary; and in the second case it might conceivably be false, as was shown by Helmholtz. It is of course true that the imaginary dwellers on a sphere might still *conceive* what we call "straight lines," but if they chose to reserve that term for geodesics of their space they would be within their rights in doing so. This is practically what Euclid does, and this is why he requires "axioms" which are *not* necessary truths; even though, in fact, they are true as far as we can test them.

So also there is no useful sense in saying that twice two must be equal to four under any conditions of time or space. Doubtless, *if* the inhabitants of the Dog Star defined "twice," "two," and "four" as we do, then "twice two" would to them be "four"; but to say that it was so could only give verbal in-

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formation. And if the people in the Dog Star chose to define four as 1 + 1 + 1, the so-called "necessary truth" would not even be true! Again, we do not "recognize that what we know 'is' cannot at the same time 'not be,'" we *define* it to be so. To know that anything "is," is indeed to possess real knowledge; but in order to conclude that therefore it cannot "not be," we require no further knowledge, except as to the meanings of the words employed in the argument. The "law of contradiction" never tells us whether anything "is" or "is not." It only tells us that the terms "is" and "is not" are not applicable to the same thing. This is part of the definition of the terms. If anyone chooses to say a thing both "is" and "is not," there is no law against his doing so, only if he does so he is not talking the Queen's English. Dr. Mivart is wrong in speaking of the "objective absolute validity of the law of contradiction." Its validity is not only not objective at all, but even subjectively it is not absolute, but depends on the arbitrary meanings assigned to its terms. It is exactly on a par with the assertion that at chess one king cannot give check to another.

EDWARD T. DIXON.

Trinity College, Cambridge, November 29.

The Koh-i-Nur.

ABSENCE from home and pressing business since my return have delayed my sending a reply to Prof. Maskelyne's second article upon the above subject (NATURE, November 5, p. 5). So far as I can discern Prof. Maskelyne's primary object in writing these articles, it is to endeavour to maintain the hypothesis put forward by him many years ago; and with this object in view he has made a number of statements, from which I have culled not a few that may be ranged under either of two heads —firstly, those which I believe can be shown to be distinctly contrary to the evidence; and secondly, those which, if not directly contradicted by the evidence, are quite unsupported by it. In my first reply I gave samples of these statements which afforded perfectly clear issues, and as these have been unanswered, it is useless to refer to others in detail at present.

Some readers of what has already been written have expressed to me their regret that finality has not been attained by this discussion. For my own part I have a feeling of sincere regret at any additional confusion being introduced into the subject. Some of the statements referred to may, unless a warning be given, be quoted in the future, as others have been in the past, by writers who may not have the means or may not be willing to take the trouble to refer to the original authors.

There are several references in Prof. Maskelyne's last article to authors with whose writings I have considered it to be my business and duty to make myself familiar. I possess their works, and of one of them I have recently published a detailed commentary, while of another I have a commentary in course of preparation. Among these authors are Garcia de Orta and Chappuzeau, and Prof. Maskelyne's remarks lead me to conclude that he has not a very intimate acquaintance with their writings and with those of some of their contemporaries. From internal evidence it is practically certain that at the time Garcia wrote his book he had not visited the Mogul's Court, and could not, therefore, have seen his jewels, though, for the sake of argument, Prof. Maskelyne suggests he had. As for the discredited Chappuzeau, whose malicious statements are quoted without their refuta ion, I need only say that Prof. Joret's investigations have cleared Tavernier of the charges of plagiarism, &c., which were made against him, and they have further disclosed the fact that his own original manuscript documents, from which the "Travels" were prepared, are still extant (see preface to the second volume of my edition of the "Travels").

Now, as to the De Boot mistake, to which Prof. Maskelyne again refers as though it had an important bearing on the subject, it is the case that Mr. King, in a footnote, pointed out the error in De Boot's quoting as from Monardes. The footnote does not occur in Mr. King's account of the diamonds, but elsewhere. When I wrote, I had Prof. Maskelyne's quotation (*Edinburgh Review*), as from Mr. King, before me, and thus I was for the moment misled as to the extent of Mr. King's knowledge. Seeing, then, that it was Prof. Maskelyne's misquotation which misled me, his not having accepted my invitation to explain, coupled with his crowing over me for having been misled (by his own words), is one of the most extraordinary features in this controversy. Two years ago I annotated my original paper with the remark that Mr. King *had* noticed the mistake of De Boot about Monardes, but it was then too late to correct the press.

The confusion which has most unfortunately been introduced into this subject by authors has now, it is to be fervently hoped, culminated in the publication by Prof. Maskelyne of a figure of a huge mounted jewel, which, going much further than his previous reference to it might have led one to expect, he labels "The Mogul." What the authority may be for this sketch, we are not clearly informed; all, apparently, that can be said for it is that "it speaks for itself." I cannot understand how Sir John Malcolm can be responsible for it, at least as it is labelled, because I know what he has published about the Shah's jewels, especially the Darya-i-Nur and its companion the Taj-e-mah. Kerr-Porter, Eastwick, and others who have described the Shah's jewels, make no mention of the existence of any such stone as this figure represents.

"It speaks for itself"; and I must venture by two alternatives to hazard an interpretation of what it says. Firstly, the amorphous-looking mass may be intended to represent some uncut stone, possibly a ruby; but why should it be the Mogul's diamond, which is known to have been cut? Secondly, it seems to be more probable that the figure may have been taken from a native sketch which originally professed to represent, but greatly exaggerated the size, and omitted the facets, of the Koh-i-Nur. Prof. Maskelyne says it was accompanied by two other stones in the same mount: so was the Koh-i-nur (see the copies of the original model in the Tower and in several public museums). The character of the mount is somewhat similar to that in the Hon. Miss Eden's sketch of the Koh-i-Nur. This is all that, as it appears to me, can be legitimately deduced from this figure which has been left "to speak for itself."

As to Prof. Maskelyne's own sketch of the Koh-i-Nur, I thank him for it, because I think it may perhaps serve to aid readers who have not seen the original in accepting the hypothesis put forward by me, that it had been mutilated after cutting.

Through the kindness of Mr. L. Fletcher, F.R.S., Keeper of the Minerals in the British Museum, I have recently had an opportunity afforded me of seeing the original plaster model of the Koh-i-Nur, and of comparing it with a glass model similar to the one upon which my remarks as to the mutilation were based, and I find them to be identical in form and all essential details.

V. BALL.

Dublin, November 13.

Pfaff's "Allgemeine Geologie als Exacte Wissenschaft."

IN this work (Leipzig, 1873) there is a speculation (on p. 162) that in early geological times the carbonic anhydride, while yet free on the surface of the earth, was sufficient in quantity to exert a pressure of 356 atmospheres. If this had been the condition of things at any time when the surface temperature was below the critical temperature (30°·9 C.), it follows that abundant liquid carbonic anhydride flowed over the surface of the earth, or floated upon the seas; unless it be supposed, which is not probable, that this quantity could be held in solution in the water. Other very important and interesting effects are also involved. The statement of the 356 atmospheres has been quoted without question by so high an authority as Dr. Irving in his "Metamorphism of Rocks."

Pfaff's result, however, is based on a statement of Bischof's (as quoted by Pfaff), that the calcium carbonate of all formations would suffice to cover the surface of the earth to a depth of 1000 *füsse*. Pfaff takes 44 per cent. of this to be CO_2, and assumes the specific gravity of the rock to be 2·6.

On these data, and taking the *fuss* as = 0·3 metre (as stated elsewhere by Pfaff), the CO_2 would exert a pressure, not of 356 atmospheres, but of 33·2, approximately. It appears, in fact, as if Pfaff's result was, through some oversight, calculated as just ten times too great.

Perhaps there is some other explanation of the discrepancy. But, lest it prove an error, I have thought well that attention should be drawn to it, the statement being made on such high authority.

J. JOLY.

Physical Laboratory, Trinity College, Dublin.

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SEISMOMETRY AND ENGINEERING IN RELATION TO THE RECENT EARTHQUAKE IN JAPAN.

AT 6.38 a.m. on October 28, I was awakened at my house in Tokio by the long swinging motion of an earthquake. There was no noise of creaking timbers, and there were no shocks such as usually accompany earthquakes. It was an easy swing, which produced dizziness and nausea. As recorded by bracket seismographs this continued for ten or twelve minutes. During the interval there was ample time to study the movements of these instruments, and the conclusion that could not be avoided was that rather than acting as steady points these heavy masses were simply being swung from side to side—horizontal displacement was not being measured, but angles of tip were being recorded. That many of our seismographs are useless as recorders of horizontal motion whenever a vertical component of motion is recorded, is a view that I have held for many years, and therefore when these two have been recorded in conjunction I have been inclined to receive the records with caution.

Further, the measurement of vertical motion as recorded by a horizontal lever arrangement can only be trusted if we can assure ourselves that the advance of the waves has been at right angles to the direction of the lever. If this condition is not fulfilled, then the seismograph for vertical motion may also become a tip-recording instrument. As another indication that during this particular earthquake earth tips occurred, I may mention that the water in a tank with perpendicular sides which is about 25 feet deep, 60 feet long, and 30 feet broad, rose quickly, first on one side and then on the other, to a height of 3 or 4 feet—much in the same way that water would rise and fall in a basin that was being tipped from side to side.

Assuming what is said to be correct, it must not be concluded that modern seismographs are useless. For earthquakes where the motion is horizontal, they give records which practically are absolutely correct. When vertical motion occurs, in many cases if not in all, the records must be interpreted in a new light. The so-called horizontal displacements may be employed in determining the maximum slope of a wave, and if from an instrument recording vertical motion we are assured that we have measured the vertical height of a wave, we can at least approximate to the length of the same. The period of the waves being recorded, it follows that the velocity of propagation may be calculated.

Although it seems possible to use our present bracket seismographs as angle measurers, it is evident that there are other types of instruments, where swing due to inertia is minimized, which will act more satisfactorily. To obtain a true measure of vertical displacement, the most evident solution would be to use a number of lever arrangements in different azimuths. Other methods may, however, suggest themselves.

For the present our time is too much occupied with outside observations to attend to instruments or to reduce their records. Up to date it is known that nearly 8000 people have been killed, many having been consumed in the burning ruins where they were entombed. At least 41,000 houses are level with the plain, and engineering structures which have stood both typhoon and flood have been reduced to ruin. In the middle of the stricken district, which is near Gifu and Ōzaki, it is doubtful whether any ordinary building could have resisted the violence of the movement; but outside this, much destruction might have been obviated had attention been given to the ordinary rules of construction, and to the special rules formulated by those who have considered the question of building in earthquake countries. In many places so-called "foreign" buildings of brick and stone—undoubtedly put up in the flimsiest manner—lie as heaps of ruin between Japanese buildings yet standing. Cotton mills have fallen in, whilst their tall brick chimneys have been whipped off at about half their height. Huge cast-iron columns, which, unlike chimneys, are uniform in section, acting as piers for railway bridges, have been cut in two near their base. In some instances these have been snapped into pieces much as we might snap a carrot, and the fragments thrown down upon the shingle beaches of the rivers. The greatest efforts appear to have been exerted where masonry piers carrying 200-feet girders over lengths of 1800 feet have been cut in two, and then danced and twisted over their solid foundations considerable distances from their true positions. These piers have a sectional area of 26 × 10 feet, and are from 30 to 50 feet in height. Embankments have been spread outwards or shot away, brick arches have fallen between their abutments, whilst the railway line itself has been bent into a series of snake-like folds and hummocked into waves. The greatest destruction has taken place on the Ōkazaki-Gifu plain, where we have all the phenomena—like the opening of crevasses, the spurting up of mud and water, the destruction of river banks, &c.—which usually accompany large earthquakes. At Ōkazaki and Nagoya the castles have survived. The reason for this may be partly attributable to the better class of timber employed in their construction, but principally to their pyramidal form and to the fact that they are surrounded by moats. Here and there a temple has escaped destruction, partly, perhaps, on account of the quality of materials employed in its construction, but also in consequence of the multiplicity of joints which come between the roof and the supporting columns. At these joints there has been a basket-like yielding, and the interstice of the roof has not, therefore, acted with its whole force in tending to rupture its supports. On the hills which surround the plain, although the motion has been severe, the destruction is not so great. These hills are granites, palæozoic schists, and other rocks. There is nothing volcanic. In the small cuttings where the railroad passes from the hills out into the plain, no effects of disturbance are observable, the surface motion probably having been discharged at the faces of the inclosing embankments. The general appearance outside the cuttings, however, is as if some giant hand had taken rails and sleepers and rubbed them back and forth until the ballast lying between them was formed into huge bolster-like ridges. Crossing the hills and proceeding to other plains, it is noticeable that there has been more movement on the alluvium than on the rocks.

Earthquakes yet continue, and in the Gifu plain each one is preceded by a boom as if a heavy gun had been fired in some subterranean chamber. Although the survivors, who may number, perhaps, two millions, are, for the most part, destitute, have witnessed the most terrible scenes, and are yet surrounded by the dead and the dying, yet there is no panic. They hear a "boomb," and run laughing to the middle of the street to escape the shock which the unaccountable noises herald. The Japanese have their feelings, but on occasions of this sort there is no helplessness in consequence of hysteria or mental prostration. As to what happens with Europeans under like circumstances, I must leave readers to consult history.

JOHN MILNE.

Tokio, November 7.

FURTHER RESEARCHES UPON AZOIMIDE, N_3H.

THE discovery of this remarkable compound of hydrogen and nitrogen by Prof. Curtius, in the chemical laboratory of the University of Kiel, formed one of the

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most interesting chemical events of last year. The extraordinary nature of the compound—manifested by its fearfully explosive properties, together with its acid character, by virtue of which it forms salts with metals containing only metal and nitrogen—mark out for it a place among the most attractive of hitherto discovered substances. It was first obtained by Prof. Curtius in the form of a gas, by treating with soda a compound containing the organic radicle benzoyl in the place of the hydrogen atom, and subsequently warming the sodium salt thus produced with dilute sulphuric acid. The gas was described as possessing a frightfully penetrating odour, and as being absorbed by water with extreme avidity, forming a solution of strongly acid properties, which liberates hydrogen in contact with metals. So great, indeed, is the affinity of azoimide for water, that in these earlier experiments it was not found possible to collect the gas in the anhydrous state. Shortly after the publication of his first communication (see NATURE, vol. xlii. p. 615), an improved method of preparing the solution in water was devised by Prof. Curtius. It consisted in distilling a soda solution of a derivative containing the radicle of hippuric acid with dilute sulphuric acid. He was thus enabled to obtain a tolerably large quantity of the aqueous acid. By successive fractionation of this solution in water, and finally distilling the last product of the fractionation over fused calcium chloride, pure azoimide itself was eventually isolated, and found to be a volatile liquid, boiling at 37°.

Owing to the terribly explosive nature of both the free acid and its salts, the work has been attended with considerable danger, and has, unfortunately, been delayed by a lamentable accident which befell Prof. Curtius's assistant, Dr. Radenhausen, who was seriously injured by the explosion of a quantity of the anhydrous acid. At length, however, Prof. Curtius is able to publish some further particulars concerning the acid and its salts, and an important communication from him will be found in the current number of the *Berichte* of the German Chemical Society. The following is a brief account of these further researches, together with a *résumé* of the present state of our knowledge of this interesting compound and its derivatives.

Sources of Azoimide and its Derivatives.

Azoimide and its salts have been obtained from two distinct sources, both organic. One source, the first employed by Prof. Curtius, is benzoyl-glycollic acid, $C_6H_5CO—O—CH_2COOH$; the second is hippuric acid, $C_6H_5CO—NH—CH_2COOH$. During the investigation of the reactions of his previously discovered compound of hydrogen and nitrogen, hydrazine, N_2H_4, Prof. Curtius found that both benzoyl-glycollic and hippuric acids reacted with hydrazine hydrate, forming hydrazine derivatives.

Benzoyl-glycollic acid reacts with two molecules of hydrazine hydrate, forming benzoyl hydrazine, $C_6H_5CO—NH—NH_2$, and the hydrazine derivative of acetic acid, $NH_2—NH—CH_2COOH$, with elimination of water. When benzoyl hydrazine is treated with nitrous acid, it is converted into a nitroso derivative, $C_6H_5CO—N(NO)—NH_2$. This nitroso compound is a very unstable substance; it spontaneously parts with water, and becomes converted into the benzoyl derivative of azoimide, benzoyl-azo-imide, $C_6H_5CO—N<\begin{matrix}N\\|\\N\end{matrix}$. This latter compound forms a direct starting-point for the preparation of azoimide. Upon boiling with alkalies an alkaline salt of azoimide is formed, together with benzoate of the alkali. Thus with soda

$$C_6H_5CO—N<\begin{matrix}N\\|\\N\end{matrix} + 2NaOH = C_6H_5COONa + NaN<\begin{matrix}N\\|\\N\end{matrix} + H_2O.$$

When the sodium salt of azoimide is distilled with dilute sulphuric acid, azoimide escapes as a gas, which condenses along with water in the form of an aqueous solution.

Hippuric acid reacts with one molecule of hydrazine with formation of hippuryl hydrazine, $C_6H_5CO—NH—CH_2CONH—NH_2$. When this substance is treated with nitrous acid, a compound is obtained which was at first considered to be a nitroso compound, but is now discovered to be in reality a diazo compound possessing the constitution $C_6H_5CO—NH—CH_2CONH—N=N—OH$. This substance may be isolated in quantity, and yields salts of azoimide directly upon treatment with alkalies. If soda is employed the sodium salt of azoimide is obtained, from which azoimide itself may, as before, be liberated by distilling with dilute sulphuric acid. It is more convenient, however, as will be described later, to employ it directly for the preparation of the ammonium salt of azoimide by saturating its alcoholic solution with ammonia gas; from the ammonia salt, if desired, azoimide itself may be obtained by converting it into the insoluble silver salt, and distilling the latter with sulphuric acid.

Preparation and Properties of the Sodium Salt of Azoimide, $Na—N<\begin{matrix}N\\|\\N\end{matrix}$.

The method of preparing the sodium salt of azoimide, now adopted as most convenient by Prof. Curtius, is somewhat different from the earlier one just described, although based upon the same lines. Instead of benzoyl-glycollic acid, ethyl benzoate, $C_6H_5COOC_2H_5$, is employed. This substance is converted readily into benzoyl hydrazine by treatment with hydrazine hydrate:

$$C_6H_5COOC_2H_5 + N_2H_4 . H_2O = C_6H_5CONH—NH_2 + C_2H_5OH + H_2O.$$

The benzoyl hydrazine is next treated with sodium nitrite and glacial acetic acid, whereby it is quantitatively transformed into benzoyl azoimide, the benzoyl derivative of the new acid:

$$C_6H_5 . CONH—NH_2 + HNO_2 = C_6H_5CO—N_3 + 2H_2O.$$

The benzoyl azoimide thus obtained is finally dissolved in an equal weight of absolute alcohol, and the equivalent of an atom of sodium is also dissolved in a little absolute alcohol, and the two solutions mixed; the mixture is then digested for several hours upon a water-bath, when the sodium replaces the benzoyl radicle, and ethyl benzoate is regenerated:

$$C_6H_5CO—N_3 + C_2H_5ONa = C_6H_5COOC_2H_5 + Na—N_3.$$

Upon cooling, the solution deposits crystals of the sodium salt, and the remainder may be precipitated from the mother-liquor by means of ether. The ethyl benzoate is recovered by distillation with very little loss, and may be employed again for the preparation of a further quantity of the sodium salt of azoimide.

The sodium salt, NaN_3, obtained by this method is substantially pure. It is very soluble in water, but is, strangely enough, not hygroscopic. It is almost insoluble in ether and alcohol. It gives a feebly alkaline reaction, and possesses a briny taste. The crystals do not explode

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by percussion, but do explode when heated to a temperature which is higher than in the case of most other salts of azoimide. The explosion is accompanied by the production of a brilliant yellow flame and a detonation which is less loud than in the case of other salts. The salt is not volatile, and is not changed by evaporation of its aqueous solution.

Preparation and Properties of the Ammonium Salt, $NH_4—N\begin{matrix}N\\ \|\\ N\end{matrix}$.

The ammonium salt, which is by far the most convenient salt to start with for the preparation of the free acid and its metallic salts, is best prepared from the curious diazo compound of the amide of hippuric acid, $C_6H_5CO—NHCH_2CO—NH—N=N—OH$, before mentioned. This substance is readily obtained in calculated quantity by first acting with hydrazine hydrate upon the ethyl ether of hippuric acid, and subsequently treating the hippuryl hydrazine thus produced with sodium nitrite and glacial acetic acid. Diazo-hippuramide appears to be most prolific in its reactions. Prof. Curtius states that it reacts with almost every class of organic and inorganic bodies with which he has brought it in contact, and generally without the application of external heat. Thus, when treated with water, alcohol, haloid ethers (alkyls), aldehydes, free halogens, or hydrazine derivatives of organic acids, it evolves free nitrogen gas, and forms compounds which are derived from hippuramide by replacement of a hydrogen atom in the NH_2 group by the radicle of the reacting substance. On the other hand, when acted upon by alkalies, ammonia or substituted ammonias (amines), or by diamide (hydrazine) and its derivatives, salts of azoimide are formed. Thus last reaction, when ammonia is employed, forms the most convenient mode of obtaining the ammonium salt of azoimide.

About a pound of diazo-hippuramide is placed in a flask of 2 litres capacity, and covered with 600 grams of 85 per cent. alcohol. The flask is then placed in a freezing mixture, and ammonia gas is led in until the liquid is saturated with it. The flask and contents are then allowed to stand twenty-four hours in order to complete the reaction, when the diazo compound is quantitatively converted into hippuramide and the ammonium salt of azoimide :

$$C_6H_5CO—NHCH_2CO—NH—N=N—OH + 2NH_3 = C_6H_5CO—NHCH_2CONH_2 + NH_4—N_3 + H_2O.$$

The liquid is then boiled, the flask being fitted with an upright condenser, until no more ammonia escapes, when the heat is removed, and the solution allowed to cool. After standing another twelve hours, the clear alcoholic solution is decanted from the mass of hippuramide crystals, and treated with four times its volume of ether, when 70 per cent. of the total yield of the ammonium salt is precipitated in the form of a white powder. The remaining 30 per cent. of the azoimide may be recovered by recrystallizing the hippuramide from water, adding the mother-liquor to the ethereal-alcoholic solution after removal of the precipitated ammonium salt, and treating the whole of the liquid with solutions of lead, silver, or mercurous salts, when the azoimide is precipitated in the form of the difficultly soluble lead, silver, or mercurous salts. The hippuramide is readily converted, by boiling with hydrazine hydrate, into hippuryl hydrazine, which may thus be used again for the preparation of more of the diazo compound.

The precipitated ammonium salt is washed with ether and dried in the air. The snow-white crystalline powder thus obtained, consisting of fine anisotropic needles, may be recrystallized from boiling alcohol. It is only sparingly soluble in absolute alcohol, but on boiling for some time in a flask fitted with inverted condenser, the whole passes into solution. Upon cooling, the salt separates out in large colourless crystals, tabular in form, and frequently aggregated in step or fan-shaped forms. These aggregates often resemble those of ammonium chloride very closely, but the crystals do not belong to the cubic system. The crystals are readily soluble in water, and, upon allowing the aqueous solution to evaporate *in vacuo*, large transparent prisms are obtained, which, however, soon become turbid in air.

The ammonium salt of azoimide reacts in a feebly alkaline manner. It is not hygroscopic, although so readily soluble in water. It dissolves easily in 80 per cent. alcohol, but, as above described, with difficulty in absolute alcohol. It is insoluble in ether and benzene. It is distinguished by its great volatility. When the crystals are allowed to lie exposed to air, they gradually disappear, eventually passing away entirely in the form of vapour. Upon gently warming a small quantity of the salt in a test-tube to a temperature very slightly superior to 100°, it sublimes like ammonium chloride, condensing again, however, in brilliant little prisms. This operation requires great care, for if the heating proceeds too rapidly the substance explodes with great violence.

As may be expected, great difficulties were met with in obtaining an analysis of a substance so explosive. Upon attempting to determine its composition by combustion with copper oxide in a stream of dry air, the apparatus was destroyed upon each occasion with a fearful detonation. Only one-tenth of a gram of the salt was employed, placed in a small platinum boat. At first the compound sublimed out of the boat into the cooler portion of the combustion tube ; the little sublimed crystals then commenced to fuse into yellow drops, and immediately this occurred, in each experiment, the tube was shattered to fragments with a frightful report. The platinum boat was in each case torn to fine splinters. Eventually, however, Prof. Curtius succeeded in obtaining a satisfactory analysis by performing the combustion with copper oxide in a stream of carbon dioxide.

The ammonium salt may be readily converted into the sodium salt by evaporation with caustic soda upon a water-bath.

Preparation and Properties of Free Azoimide, $H—N\begin{matrix}N\\ \|\\ N\end{matrix}$.

An aqueous solution of azoimide may be prepared by distilling any of its salts, preferably the sodium or silver salts, with dilute sulphuric acid. It is more conveniently obtained, however, by dissolving the crystals of diazo-hippuramide in dilute caustic soda, warming the solution for a short time upon a water-bath, so as to insure the formation of the sodium salt, and subsequently distilling the liquid with dilute sulphuric acid. The latter is allowed to drop slowly from a dropping funnel upon the soda solution contained in a flask and maintained at the temperature of ebullition. The flask is connected with a condenser, and the azoimide, as it escapes, is carried along with the steam, and condenses in the receiver in the form of an aqueous solution. This solution may be concentrated by precipitating it with silver nitrate, collecting the insoluble silver salt, and distilling it with sulphuric acid diluted with eight times its volume of water.

The aqueous solution of azoimide possesses a most intolerable odour.

Free azoimide itself may be obtained by the fractional distillation of the concentrated aqueous solution thus prepared. The first fraction is collected separately and refractioned. Upon repeating this process with four successive first fractions, an acid containing over 90 per cent. of azoimide is obtained, which distils at about 45°. The last 10 per cent. of water may be completely removed

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by means of fused calcium chloride, after treatment with which pure anhydrous azoimide distils over.

Azoimide is a clear, colourless, mobile liquid, which boils without decomposition at 37°. It is endowed with the same intolerable odour as the solution. Its most characteristic property, however, is its frightful explosiveness. It explodes in a most erratic manner—sometimes, without the least apparent provocation, at the ordinary temperature. Its distillation is an operation attended with great danger; Prof. Curtius and his assistant *have* succeeded, as above described, in isolating it and determining its boiling-point several times; but upon other occasions, under apparently the same conditions, the experiment has ended with a disastrous explosion. When suddenly heated, or touched with a hot body, it always explodes. The explosion is accompanied by an intensely vivid blue flame. The damage wrought by the explosion of very minute quantites is most surprising. The thousandth part of a gram, placed upon an iron plate and touched by a hot glass rod, is sufficient to produce a loud detonation, and considerably distort the iron plate. The twentieth part of a gram was found sufficient to completely pulverize a Hofmann "density" apparatus, when an attempt was made to determine its vapour-density in the Torricellian vacuum at the ordinary temperature. Upon another occasion, seven-tenths of a gram, contained in a closed glass tube, upon removal from the freezing mixture in which it had been immersed exploded with such immense force as to shatter every piece of glass apparatus in the laboratory. It was upon this occasion that Prof. Curtius's assistant was so seriously injured as to cause the temporary abandonment of the work The aqueous solution is almost as explosive as the anhydrous liquid, the explosion of two cubic centimetres of a 27 per cent. solution upon one occasion shattering the glass tube into dust so fine that Prof. Curtius, who was attempting to seal it, escaped uninjured.

The anhydrous liquid readily dissolves in water and alcohol. The aqueous solution is strongly acid to litmus. Magnesium, aluminium, zinc, iron, and even copper are rapidly attacked by it, hydrogen being evolved. Gold and silver are likewise attacked, although not so rapidly. In the case of gold a red solution of the gold salt is formed; the silver salt being insoluble soon prevents further action in the case of silver.

The anhydrous compound appears to be decomposed by *concentrated* sulphuric acid.

Other Metallic Salts of Azoimide.

The silver salt, $Ag—N\langle^{N}_{N}$, is obtained in the form of a white precipitate whenever a soluble silver salt is added to azoimide, its aqueous solution, or solutions of its salts. It resembles silver chloride very closely in appearance, but is not darkened by the action of light. There is very little difficulty in distinguishing between the two salts, however, inasmuch as the silver salt of azoimide partakes of the frightfully explosive properties of the free acid. It is the most dangerous of all the salts to handle. In spite, however, of this explosiveness, Prof. Curtius has *once*, and only once, succeeded in obtaining a determination of its nitrogen content, by combustion in a long layer of copper oxide. The number obtained was 27·65. The value calculated from the formula AgN_3 is 28·00. In every other experiment the tube was shattered into fragments, notwithstanding the finest subdivision and distribution among the copper oxide.

The precipitated silver salt is soluble, like silver chloride, in ammonia. Upon evaporation of the solution, however, instead of obtaining an ammoniacal double compound, the silver salt itself crystallizes out in almost colourless crystals half an inch long. These crystals, as may be imagined, are most delicate objects to handle; they explode even upon breaking the prisms across. They are almost perfectly insoluble in water.

The mercurous salt, HgN_3, is likewise insoluble in water, and may be readily obtained by precipitation of the free acid or its soluble salts with mercurous nitrate. It has the advantage of being more safely handled than the silver salt, and is less sensitive to percussion. It also requires a higher temperature to bring about explosion by heating. It is usually obtained by precipitation in the form of microcrystalline anisotropic needles. It becomes coloured yellow when exposed to light. Like mercurous chloride, it forms a black compound with ammonia.

The lead salt, PbN_6, resembles lead chloride very closely. It is insoluble in cold water, but dissolves somewhat in boiling water, though not to such an extent as lead chloride, about half a gram dissolving in a litre of boiling water. Upon cooling, brilliant colourless needles, attaining sometimes the length of half an inch, separate out. It may be best obtained by precipitating the solution of the sodium or ammonium salt with lead acetate. The precipitate is soluble in excess of lead acetate. The crystals of the lead salt explode with fearful force when slightly warmed. By long boiling with water this lead salt appears to be decomposed, a non-explosive lead compound separating, and free azoimide escaping. Warm acetic acid also dissolves it, with gradual decomposition and liberation of azoimide.

The barium salt, BaN_6, is readily obtained by neutralizing the acid with baryta, and crystallizes from solution in large lustrous crystals. It is likewise a highly explosive salt, and the explosion is accompanied by the production of a brilliant green flame.

Ethereal Salts of Azoimide.

The metallic salts of azoimide may be readily converted into ethereal salts by reacting upon them with haloid ethers. The phenyl ester has been prepared by Prof. Curtius, and is found to be identical in every respect with the long known diazobenzene imide, $C_6H_5N\langle^{N}_{N}$. The aromatic esters are generally remarkably stable substances, and it was consequently found impossible to obtain azoimide by the direct saponification of diazobenzene imide with soda. Within the last few months, however, Drs. Noelting and Grandmougin, of Mülhausen, have succeeded in preparing azoimide indirectly from diazobenzene imide, by first diminishing somewhat the stability of the compound by introducing two nitro groups into it. Upon treating dinitro-diazobenzene imide with alcoholic potash, the potassium salt of azoimide was at once formed. Upon distilling this with dilute sulphuric acid, an aqueous solution of azoimide was obtained, and eventually anhydrous azoimide itself, identical in all respects with that obtained by Prof. Curtius.

Hydrazine Salt of Azoimide, $N_3H \cdot N_2H_4$.

This interesting substance, formed by the union of equal molecules of the two remarkable compounds, hydrazine and azoimide, whose isolation we owe to Prof. Curtius, is a solid substance forming large well-defined crystals. It is obtained when one molecular equivalent of hydrazine hydrate (the preparation and properties of which were described in NATURE, vol. xliii. p. 205) is added to the ammonium salt of azoimide, and the mixture, placed in a shallow dish, is allowed to evaporate in a desiccator. It is curious, also, that by adding a very concentrated aqueous solution of azoimide, obtained by distilling 60 grams of the lead salt with dilute sulphuric acid, to hydrazine hydrate, until litmus is turned strongly blue by the mix-

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ture, one does not obtain, as might be expected, the salt $2N_3H \cdot N_2H_4$; on the contrary, the mono-salt, $N_3H \cdot N_2H_4$, is again formed; and if the mixture is placed in a desiccator containing solid caustic potash and oil of vitriol, crystals of the mono-salt are deposited, and the excess of azoimide escapes and is absorbed by the potash.

The mono-hydrazine salt crystallizes in lustrous, anisotropic prisms an inch long, which melt at 50°. They rapidly deliquesce in air. They are soluble with difficulty in boiling alcohol, and crystallize from the solution, on cooling, in brilliant leafy crystals. The crystals burn quietly with a smoky flame when brought in contact with a flame, leaving no trace of residue. Owing to the strong reducing power of the liberated glowing hydrogen, metallic surfaces upon which the crystals may be ignited become brightly cleaned, as if polished, being freed from the least trace of oxide. When the crystals, however, are rapidly heated, or touched with a white-hot wire, they explode with fearful violence. The explosion may also be brought about by detonating in the neighbourhood a little of one of the metallic salts of azoimide. Even in the deliquesced state the substance retains its fearfully explosive properties.

In conclusion, it may be remarked that Prof. Curtius has further succeeded in preparing a number of derivatives of the as yet unisolated compound of hydrogen and nitrogen, triamide, $NH_2—NH—NH_2$, by the action of the hydrazine compounds of several organic acids upon diazohippuramide. The organic radicles are so rigidly attached in these derivatives that he has not yet been fortunate in displacing them. It appears very probable, however, that before long some means of effecting this object will be discovered, and that Prof. Curtius will further add to his achievements by making us as familiar with triamide as we now are with hydrazine and azoimide.

A. E. TUTTON.

BEAST AND MAN IN INDIA.[1]

THE natives of India have been described from many different points of view, but Mr. Kipling is the only writer to whom it has occurred to give a full account of their relations to animals. The task is one for which he is admirably fitted, and he has fulfilled it in a way that cannot fail to secure for the book an enduring place in the literature relating to Indian subjects. The work has considerable scientific value, not only because the author presents the people of India in a new light, but because he has much that is fresh to say about the animals to which he refers. Animals have a great attraction for him, and his notes on their habits have the brightness and charm that belong only to records of direct personal observation. The volume is enriched with many clever illustrations, the majority of which are from drawings by Mr. Kipling himself. Most of the others are from drawings by native artists. Several of these illustrations we are enabled to reproduce.[2]

There is a general impression in Europe that animals are treated kindly in India. This impression is only in part confirmed by Mr. Kipling. It is true that the Hindus, believing in the transmigration of souls, are reluctant to take animal life; but that, says Mr. Kipling, "does not preserve the ox, the horse, and the ass from being unmercifully beaten, over-driven, over-laden, underfed, and worked with sores under their harness; nor does it save them from abandonment to starvation when unfit for work, and to a lingering death which is made a long torture by birds of prey, whose beaks, powerless to kill outright, inflict undeserved torment." Even the sacred cow is not so well treated as the milch cows of Europe. Mr. Kipling's statements on this subject are extremely unpleasant reading, but it is to be feared that they are only too well founded. With regard to free creatures, however, he is able to give a better report. The village school-boy does not stone frogs, or set dogs at cats, or tie kettles to dogs' tails, or go bird-nesting, or annoy squirrels; and "the sparrow, the crow, the maina, and the hoopoe move from his path without a flutter of fear." The farmer, too, endures with extraordinary patience depredations on his crops by wild animals. The monkey, the nilghai, the black buck, the wild pig, and the parrakeet fatten at his expense, and do not even reward him by killing a caterpillar or a weevil. There are bird-catchers in India, but the popular feeling about them is shown in a scornful proverb on their ragged and disreputable condition.

One of the brightest chapters in the book is on monkeys, which the author has had good opportunities of observing. The monkey most commonly seen both in the hills and plains is the Macaque (*Macacus rhesus*), which is not to be confounded with the tall, long-tailed, white-whiskered Langūr (*Presbytes illiger*). The latter is clad in an overcoat of silver-grey. He is, in his way, a king of the jungle, and is not so often met with in confinement as his brown brother. The Hooluck, or Black Gibbon (*Hylobates hooluck*), is most often found in Bengal and Assam, and, if a pair can be secured, easily reconciles itself to captivity; but it is "a depressing companion." An Assam monkey, known as "the shame-faced one," is "a gentle, bashful, large-eyed creature, with a quaint trick of hiding its face in its hands and hanging its head like a timid child."

The monkey is held in great respect in India, and Hanumān, the monkey-god, is one of the most widely-worshipped of Hindu deities. Sacred as the animal is, the people are thoroughly alive to the faults of its character, and in ordinary talk the monkey is used, as in Europe, "to point morals against wanton mischief, helplessness, and evil behaviour generally." For some months a number of wild monkeys were daily fed by Mr. Kipling and his family; and they soon learned to distinguish by smell between fresh and stale biscuits. Yet—as the natives have noted in proverbs—monkeys are not sharp enough to provide themselves with shelter against the heavy rains of the country. In the Simla region they may be seen in troops sitting shivering for hours within a few yards of covered spaces. The scheme of their life, according to Mr. Kipling, seems to be patriarchial, with a touch of military organization, the patriarch being "at once commander-in-chief and effective fighting force." The natives call him "Maharajah," and the name is well chosen, as he is "the very type and incarnation of savage and sensual despotism." Monkey mothers treat their little ones with a tenderness "that endears them to the child-loving Oriental"; and the young, when the sun shines, often contrive to have a good time. Says Mr. Kipling:—

"They have a game like the English boys' cock of the dung-hill or king of the castle, but instead of pushing each other from the top of a knoll or dust-heap, the castle is a pendent branch of a tree. The game is to keep a place on the bough, which swings with their weight as with a cluster of fruit, while the players struggle to dislodge one another, each, as he drops, running round and climbing up again to begin anew. This sport is kept up for an hour at a time with keen enjoyment, and when one is nimble as a monkey it must be splendid fun."

Mr. Kipling finds in cows and oxen the subject of another capital chapter. They seem to him to be the foremost figures in both the rustic and the urban scenery of the country. The cow is now "firmly enthroned in the Hindu pantheon," and the peculiar sanctity in which

[1] "Beast and Man in India." By John Lockwood Kipling, C.I.E. With Illustrations. (London: Macmillan and Co., 1891.)

[2] Figs. 1, 2, and 4. are by Mr. Kipling. Fig. 3 is by Munshi Sher Muhammad.

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she is held may, he thinks, be partly due to the fact that in early Aryan mythology she was used as a symbol of the clouds attendant on the Sun-god. Mr. Kipling expresses much admiration for Indian oxen, of which he says that it is with them as with the people of India—

FIG. 1.—Young monkeys at play.

"the more you learn about them the more you find to interest you." Fig. 2 represents roughly the range of their size. Even larger beasts than the largest shown sometimes occur. The smallest belongs to a miniature

FIG. 2.—Comparative sizes of the largest and smallest breeds of Indian oxen.

race which is not much bigger than a Newfoundland dog. This little creature is exquisitely finished in every detail of ox form; it is full of life and spirit, and, when harnessed to vehicles of a suitable size, it trots at a great pace.

Mr. Kipling notes the peculiar impression produced on Europeans by the silence of Indian cattle. Students, reading in Gray's Ode of the lowing herd, will say to the Professor, "Sir, what is lowing?" Even the grunting note of the ox and cow is seldom heard, and does not carry. The herdsman is a vocalist, but his song "is always in a minor key, and has falsetto subtleties in it that baffle our methods of notation." He talks a good deal in a loud heavy voice; and "when his women folk walk with him they follow respectfully an ordained number of paces behind, and he flings his conversation over his shoulder."

It would be impossible, of course, to write a book about "Beast and Man in India" without treating of

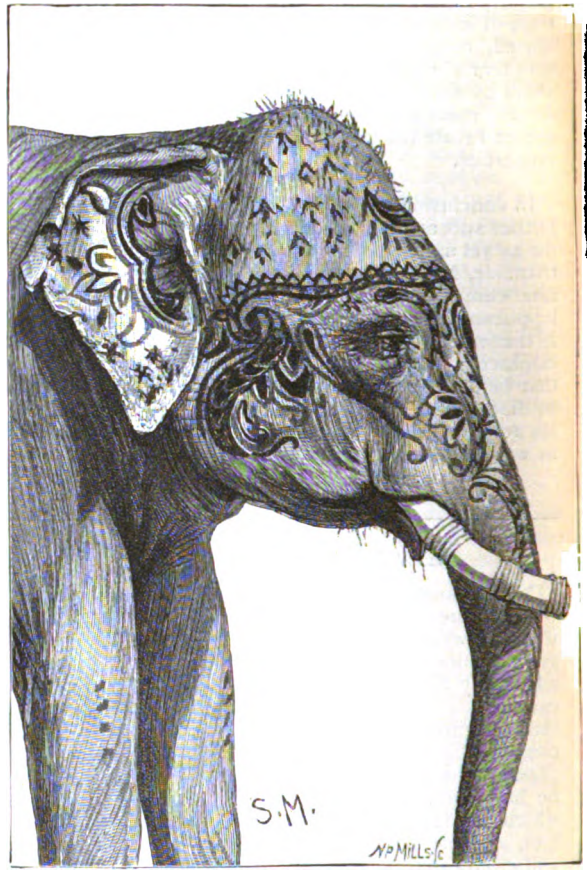

FIG. 3.—A painted elephant.

elephants; and Mr. Kipling discourses on them most pleasantly. The elephant seems to be second only to the cow in Hindu estimation. In Hindu art other animals are treated only in a decorative and conventional style; but in artistic representations of the elephant, whether in sculpture or in painting, there is invariably "a strong feeling for nature." This contrast is seen in most old temples, but especially in the sculptured gates of the Sanchi tope in Central India, "where all kinds of animals are shown, but the elephant alone is carved with co uplete knowledge and unvarying truth of action."

Mr. Kipling is at great pains to convey a true idea of the character of the elephant, the essential quality of which he takes to be gentleness. He also tells many curious legends about the animal, and about Ganésha, the elephant-headed god, who is not less popular than the monkey-god. The dressing of the elephant for parade is also described. This, although an elaborate process,

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is not really much more difficult than the dressing of a child for church. The most remarkable part of the process is the painting of the forehead, trunk, and ears, which follows a thorough washing. "The designs are often good, and the whole serai, excepting always the elephant himself, is deeply interested. His mind and trunk wander; he trifles with the colour-pots; so with each stroke comes an order to stand still. Some mahouts are quite skilful in this pattern work."

In an interesting chapter on the training of animals, Mr. Kipling shows that the skill of the natives of India in this difficult art has often been greatly overrated. The Oriental brings "boundless patience" to the task, but "he has no steadfastness of aim, nor has he sufficient firmness of hand and will to secure confidence and obedience." The cheetah or hunting leopard (*Felis jubata*), when caught and tamed, undergoes so little training in the field that it loses its natural dash, and is often left behind by the antelope. It becomes so mild

FIG. 4.—A restless bedfellow.

that it is frequently allowed to curl itself under the same blanket with its keeper. The keeper, when his bedfellow is restless, "lazily stretches out an arm from his end of the cot, and dangles a tassel over the animal's head, which seems to soothe him." In the early morning Mr. Kipling has seen a cheetah "sitting up on his couch, a red blanket half covering him, his tasselled red hood pushed awry, looking exactly like an elderly gentleman in a nightcap, as he yawned with the irresolute air of one who is in doubt whether he will rise or turn in for yet another nap." This is mentioned as an instance of the curious intimacy that exists in India between animals and those who have charge of them.

Of the remaining chapters we can only say that all of them embody the results of a close study of the animal world and of the Hindu character. We may note as of especial interest the three concluding chapters, on animals in Indian art, on beast fights, and on animals and the supernatural.

ON AN OPTICAL PROOF OF THE EXISTENCE OF SUSPENDED MATTER IN FLAMES.[1]

DEAR PROFESSOR TAIT,—I write to put on paper an account of the observation I mentioned to you to-night, in case you should think it worth communicating to the Royal Society of Edinburgh.

In the course of last summer I was led, in connection with some questions about lighthouses, to pass a beam of sunlight, condensed by a lens, through the flame of a candle. I noticed that where the cone of rays cut the luminous envelope there were two patches of light brighter than the general flame, which were evidently due to sunlight scattered by matter in the envelope which was in a state of suspension. The patches corresponded in area to the intersection of the double cone by the envelope, and their thickness was, I may say, insensibly small. Within the envelope, as well as outside, there was none of this scattering. The patches were made more conspicuous by viewing the whole through a cell with an ammoniacal solution of a salt of copper, or through a blue glass coloured by cobalt. In the former case the light from the flame was more weakened than the scattered light, which was richer in rays of high refrangibility; in the latter case the patches were distinguished by a difference of colour, the patches being blue, while the flame (with a suitable thickness of blue glass) was purplish. The light of the patches exhibited the polarization of light scattered by fine particles—that is to say, when viewed in a direction perpendicular to the incident light it was polarized in a plane passing through the beam and the line of sight.

When the beam was passed through the blue base of the flame there was no scattered light. A luminous gas flame showed the patches indicating scattered light like the flame of a candle, but less copiously. They were not seen in a Bunsen flame or in the flame of alcohol, but were well seen in the luminous flame of ether. When a glass jar was inverted over burning ether, the blue part, which does not show scattered light, extended higher, till, just before the flame went out, the luminous part disappeared altogether. A Bunsen flame, fed with chloride of sodium, did not show the phenomenon, though the flame was fairly luminous.

The phenomenon shows very prettily the separation of carbon (associated, it may be, with some hydrogen) in the flame, and at the same time the extreme thinness of the layer which this forms. It shows, too, the mode of separation of the carbon—namely, that it is due to the action of heat on the volatile hydrocarbon or vapour of ether, as the case may be. At the base, where there is a plentiful supply of oxygen, the molecules are burned at once. Higher up the heated products of combustion have time to decompose the combustible vapour before it gets oxygen enough to burn it. In the ether just going out, for want of fresh air, the previous decomposition does not take place, probably because the heat arising from the combustion is divided between a large quantity of inert gas (nitrogen and products of combustion) and the combustible vapour, so that the portion which goes to the latter is not sufficient to decompose it prior to combustion.

In the Bunsen flame fed with chloride of sodium, the absence of scattered light tallies with the testimony of the prism, that the sodium is in the state of vapour, though I would not insist on this proof, as it is possible that the test of scattering sunlight is not sufficiently delicate to show the presence of so small a quantity of matter in a solid or liquid state.—Yours sincerely,

G. G. STOKES.

P.S.—I fancy the thinness of the stratum of glowing carbon is due to its being attacked on both sides—on the outside by oxygen, on the inside by carbonic acid, which with the glowing carbon would form carbonic oxide.

[When the above was written, I was not acquainted with the previous paper by Mr. Burch, published in vol. xxxi. of NATURE (p. 272), nor did any of the scientific friends to whom I had mentioned the observation seem to be aware

[1] Read before the Royal Society of Edinburgh on June 15, 1891. Reprinted from the Proceedings of the Society.

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of it. Had I known of it, I should not have thought my paper worthy of being presented to the Royal Society of Edinburgh, as Mr. Burch has anticipated me in the fundamental method of observation.

The reaction mentioned in the postscript is to be taken merely as a specimen of the reactions, on the inside of the carbon stratum, by which the carbon may be re-engaged in a gaseous combination. Carbonic oxide is only one of the combustible gases, not originally present, which are formed during the process of combustion, and are found inside the envelope in which the combustion is going on.—G. G. S., *November* 20, 1891.]

NOTES.

THE annual general meeting of the Institution of Electrical Engineers will be held at the Institution of Civil Engineers, 25 Great George Street, Westminster, this evening (Thursday), at 8 o'clock, for the reception of the annual report of the Council, and for the election of Council and officers for the year 1892. The following paper will be read:—"On the Specification of Insulated Conductors for Electric Lighting and other purposes," by W. H. Preece, F.R.S., Past-President.

THE Royal Danish Academy of Sciences at Copenhagen offers two prizes of 400 and 600 kronen respectively, for investigations on the exact nature and proportions of the more important carbo-hydrates present, at different stages of maturity, in the cereals in most general use; and for investigations on the *Phytoptus* galls found in Denmark, with a monograph on the insects producing them. The prizes are to be awarded in October 1893.

AN improved armillary sphere has been patented by Prof. J. S. Slater, of Calcutta University, which differs from other spheres of the same kind in having a latitude circle to which the celestial sphere is hinged, and in being provided with a movable horizon which adjusts itself to the selected latitude. It can be obtained from Messrs. Walsh, Lovett, and Co., Philpot Lane, E.C.

THE next one-man photographic exhibition organized by the Camera Club will consist of pictures by Mr. J. Pattison Gibson, of Hexham. It will be opened in connection with a concert to be held on the first Monday in January, 1892.

WITH the consent of the Sultan of Muscat, the Survey of India is about to establish a tidal observatory at Muscat. This will probably be followed by the establishment of another observatory of the same kind at Bushire in the Persian Gulf.

WE have had some correspondence with Prof. Arnold about our notice of his speech at the recent meeting of the Institution of Mechanical Engineers. Referring to his remarks on Prof. Roberts-Austen's "Report to the Alloys Research Committee," we expressed the opinion that it was rather straining the prerogative of rhetoric to speak of the work done by Prof. Roberts-Austen as "not worth a rush." We did not intend to imply that Prof. Arnold applied the expression "not worth a rush" to the whole of the work on which Prof. Roberts-Austen reported. He wishes us to state that what he said was, that he thought "any analogue obtained from a comparison of simple bodies like gold and lead with a complex body like steel would not be worth a rush."

THE reports of the examiners on the results of the science examinations held in April and May 1891 have been issued. The examinations related to building construction, naval architecture, mathematics, theoretical mechanics, applied mechanics, magnetism and electricity and alternative elementary physics, chemistry, geology, mineralogy, animal physiology, botany, the principles of mining, navigation and nautical astronomy, steam, the principles of agriculture, and hygiene.

MRS. R. M. CRAWSHAY, writing to us from Mentone, on December 2, about the recent eclipse of the moon, refers to the fact that "the Rev. A. Freeman and Mr. R. T. Leslie are not agreed as to the shadow on the moon's disk having colours or not." For some time there were illuminations and fireworks at Monte Carlo on account of the birthday of the Prince of Monaco, and, when these were over, clouds suddenly came up. "It was only," Mrs. Crawshay says, "when the moon was very nearly half obscured that I caught a glimpse of her without any colouring whatever, orange or otherwise. One could only liken it to a painting in Indian ink."

MR. GEORGE T. BETTANY, who was well-known as a popular writer on scientific subjects, died on December 2 in his forty-second year. For some years he lectured on botany at Guy's Hospital. Conjointly with Prof. Parker, he wrote a work on "The Morphology of the Skull." He was also the author of "The World's Inhabitants," and other books. For Messrs. Ward, Lock, and Co., he edited "Science Primers for the People" and "The Minerva Library."

AN interesting paper on aluminium and its application to photography, by Mr. G. L. Addenbrooke, is printed in the December number of the Journal of the Camera Club. Mr. Addenbrooke thinks aluminium ought now to supersede brass for photographic lenses and the metal parts of cameras. By its use the weight of lenses, flanges, and adapture is reduced nearly to one-third. He is also in favour of aluminium being used for the revolving tripod heads fixed in the bare boards of cameras, as these are rather too heavy in brass. "In hand-cameras," he says, "I think the metal should be useful in most places where there are metal parts. I am also not without hopes that dark slides may be made in it altogether, which will be lighter and more compact than the wooden ones. For developing-dishes, also, it is very suitable, as the action of most of the chemicals used in photography is very slight on it, and when there is any, the compounds formed would not be harmful."

IN his latest communication to the *American Journal of Science* (for November), Prof. Goodale describes his visit to the Queensland Museum at Brisbane, under the charge of Mr. De Vis, rich in specimens illustrating the natural history and ethnology of the colony. An account is also given of the well-known Botanic Garden and Laboratories at Buitenzorg in Java, under the directorship of Dr. Treub, and of the annex on a contiguous mountain; of the Botanic Garden and Experimental Garden at Singapore, under the control of Mr. Ridley; and of the new and at present but poorly developed Botanic Garden at Saigon in French China.

POISONING by mussels is a well-known fact. Such poisoning appears in chronic form in Tierra del Fuego, mussels being abundant on the shores, and other kinds of food rare, so that the natives eat large quantities of the former daily, both of bad and of good quality. According to a doctor of the Argentine fleet, M. Segers, the mussels are rarely injurious at their maximum time of growth, which corresponds with full moon, but when the moon wanes, they become poor and often poisonous. The poisonous quality apparently results from the death of a large number at this time, and the putrefaction of their bodies yielding ptomaines which are absorbed by the surviving mollusks. In any case, the Fuegians are often attacked by a liver complaint, consisting in atrophy of the organ, with jaundiced colour of the skin and tendency to hæmorrhage; and M. Segers believes this is due to mussel poisoning. He finds sulphate of atropine an efficacious antidote.

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PRINCIPAL J. L. THOMPSON, of the Hawkesbury Agricultural College, New South Wales, has no doubt that the climate and much of the soil of Australia are well suited for the culture of the olive. All that is needed, he thinks, is an adequate supply of labour. He himself has been very successful in preserving green olives; and in a paper on the subject in the August number of the *Agricultural Gazette* of New South Wales he gives the following account of the system adopted. The olives are very carefully picked from the trees when about full grown, but perfectly green. They should be handled like eggs. If they are bruised in any way, they will become black and decompose. In the green state, olives contain gallic acid, which gives them an acrid taste. To remove this they are first of all steeped in alkaline water, made either of wood ashes, lime water, or washing soda; of the latter, about three or four ounces to the gallon of water. As soon as the lye has penetrated through the pulp, which is usually in from eight to ten hours, they are put into clean water and steeped until all acrid and alkaline taste has been removed. During that time the water is changed every day. They are then put into brine, composed of one pound of salt to each gallon of water, and kept carefully covered with a thick linen cloth, for if exposed to the air they will turn black. They are finally put up in air-tight jars.

THE Meteorological Department of the Government of India has published Part IV. of "Cyclone Memoirs," being an inquiry into the nature and course of storms in the Arabian Sea, and a catalogue and brief history of all recorded cyclones in that sea from 1648 to 1889. The work, which has been prepared by Mr. W. L. Dallas, chiefly for the use of mariners navigating those parts, will no doubt be of considerable use to them, as hitherto there were no track charts of the storms in the Arabian Sea for the different months. For the majority of the storms quoted the available materials are admittedly very scanty; nevertheless, the author has been able to draw some useful conclusions from them, with reference to the general behaviour of the storms. The paper is divided into two parts—the first gives the details of each of fifty-four storms in chronological order, the second treats of their geographical distribution and movements according to months and seasons, and the discussion is followed by charts showing the tracks of the storms in the different months. The cyclones are formed on the northern limits of the south-west monsoon; when the northern limits of the monsoon reach the land, and also when the north-east monsoon extends from Asia to the equator, which is the case from December to March, no cyclones are formed over the Arabian Sea. The barometric fall is gradual and equal on all sides, except near the centre, and a depression of 0·25 inch below the average is indicative of the existence of a cyclone in the neighbourhood. When the storms are in confined waters they may burst with great suddenness, but in other cases strong winds are felt for several hundred miles around the centre. The northern parts of the Arabian Sea are liable, during the prevalence of the north-east monsoon, to be disturbed by small cyclonic storms descending from the highlands of Persia and Beluchistan, but the whole of the south-west of the Arabian Sea, though liable to south-west gales during the summer monsoon, and to strong north-east winds during the winter monsoon, is free from cyclones.

DR. STIRLING'S *Notoryctes typhlops*, the lately discovered Australian animal, to which we have repeatedly called attention, forms the subject of an interesting note in the "Hand-List of Australian Mammals," by J. Douglas Ogilby, an advance copy of a portion of which has been forwarded to us. The conclusion at which Mr. Ogilby has arrived, after an exhaustive study of Dr. Stirling's pamphlet, is that in this animal we have at last obtained a definite connecting link between the Monotremes and the Marsupials. At the present stage of our knowledge it would, he thinks, be presumptuous to class *Notoryctes* among the Monotremes proper, although several leading naturalists incline to the opinion that its affinities are closer to these mammals than to the Marsupials. He prefers for the present to look upon it as an aberrant Polyprotodont.

THE Institute of Jamaica has isued the first number of a Journal which is to contain, among other things, contributions regarding newly discovered flora and fauna of the island, and articles dealing with botany and kindred sciences. Four parts will be published in the year. In this first number there are excellent notes, by Mr. T. D. A. Cockerell, on the transformation of some Jamaica Lepidoptera. He points out that, although many species of butterflies and moths have been described from Jamaica, the transformations of very few are known.

DR. A. H. POST, the well-known anthropologist, describing in this week's *Globus* various marriage customs, refers to a strange sort of symbolical marriage which is supposed to have originated in India. It is a marriage with trees, plants animals, or inanimate objects. If anyone proposes to enter upon a union which is not in accordance with traditional ideas, it is believed that the ill-luck which is sure to follow may be averted by a marriage of this kind, the evil consequences being borne by the object chosen. In various regions a girl must not marry before her elder sisters, but in some parts of Southern India the difficulty is overcome by the eldest daughter marrying the branch of a tree. Then the wedding of the second daughter may safely be celebrated. Dr. Post gives several other instances, which are likely to be new to many students of anthropology.

ACCORDING to an official French Report, the copper mines of French Congo are likely to prove of considerable importance. They lie in the district around the sources of the Ludima-Niadi, about two days' journey south of Stéphanieville. The ore is malachite, which is brought to the surface by about 350 negroes. Their methods of work are extremely simple. They reach the malachite by digging out, with implements of hard wood, holes or shafts three feet wide and twice as deep. The malachite is broken on the ground, and afterwards, when pulverized, put into a furnace on a tray with charcoal, on which bellows are made to play. In due time the tray is removed by means of pieces of bamboo, and the metal is poured into sand moulds. The entire district is said to be rich in copper, and masses of malachite are frequently found in the Ludima.

MR. ERNEST E. THOMPSON, of Toronto, contributes to the new volume of the Proceedings of the U.S. National Museum (vol. xiii.) a valuable study of the birds of Manitoba. He gives an enthusiastic description of the music of prairie larks, large numbers of which, at dawn, may be heard in the spring to "burst all together into a splendid explosion of song, pouring out their rich, strong voices from every little height and perch, singing with all their might." They sing all day, and at night joyously hail the moon. As their notes become more complicated, the most casual observer cannot fail to perceive "that the love-fires are kindling, and that each musician is striving to the utmost of his powers to surpass all rivals and win the lady lark of his choice." "On one occasion," says Mr. Thompson, "as I lay in hiding near a fence, three larks came skimming over the plain. They alighted within a few yards of me, and two of them burst into song, sometimes singing together and sometimes alternately, but the third was silent. When at last they flew up, I noticed that the silent one and one of the singers kept together. I had been witness to a musical tournament, and the victor had won his bride."

ANOTHER of the many birds of Manitoba about which Mr. Thompson has something interesting to say is the crane. The

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first intimation of its advent in the spring is usually a loud trumpeting or croaking that seems to shake the air for miles. But the cranes themselves, generally in pairs, soon begin to be seen. Their food at that season is chiefly rose-pips, in gathering which they stalk over the bare plains. At first little can be noted but their excessive wariness, but as the warmer weather quickens their feeling, they often "so far forget their dignity as to wheel about and dance, flapping their wings and shouting as they 'honour their partners,' and in various ways contrive to exhibit an extraordinary combination of awkwardness and agility." This dance Mr. Thompson has seen only during the pairing season.

REFERRING to the question "whether squirrels are torpid in winter," Mr. C. Fitzgerald writes in the December number of the *Zoologist* that, during many winters passed in the backwoods of North America, he has seen squirrels frisking among the trees in the coldest weather. On bright sunny days especially they delight in chasing each other from tree to tree among the evergreens, and cover the snow with their tracks. The young of the ordinary red squirrel are born early in the spring. The "Chipmunks," or little striped ground squirrels, lay up in the autumn a store of provisions of grains, nuts, &c., for winter, and on fine days may be seen sunning themselves. Mr. Fitzgerald has on several occasions come across their hoards, and once saw two large bucketsful of shelled buckwheat taken from the hollow of an old birch-tree that the woodmen had chopped down on the edge of a clearing which had been cropped the previous summer with that grain.

AT the meeting of the Linnean Society of New South Wales on October 28, the fifth part of Mr. E. Meyrick's "Revision of Australian Lepidoptera" was read. This paper practically concludes the Australian *Geometrina*, except in so far as future discoveries may produce fresh material. One hundred and twelve species are included, of which forty-seven are described as new.

MR. CARL LUMHOLTZ contributes to the current Bulletin of the American Geographical Society a very interesting report on his explorations in Northern Mexico. The most remarkable caves he met with were at the head of the Piedras Verdes River, 6850 feet above sea-level. These caves contain groups of deserted houses or small villages, and the houses are splendidly made of porphyry pulp, and show that the inhabitants had attained a comparatively high culture. The dwellings were sometimes three stories in height, with small windows, and doors made in the form of a cross; and occasionally there were stone staircases. The caves, which number about fifty in a stretch of twenty miles, are from 100 to 200 feet above the bottom of the cañon, and the largest is some fifty feet high. One series of them, on the shady side of the cañon, had been reserved for burial-places. Here, at a depth of three feet, Mr. Lumholtz dug out a number of bodies in a wonderful state of preservation, the saltpetre which is mixed with the disintegrated rock having for centuries preserved them, making them look like mummies. Several had their features, hair, and eyebrows perfect, and these were photographed. The hair is very slightly wavy, and softer than that of the ordinary Indian, almost silky in fact. They were small people, and reminded Mr. Lumholtz strikingly of the present Moqui village Indians. The Moquis, like the Zuñis, have a tradition that they came from the south. The same district abounds in mounds, some of which are very large. Mr. Lumholtz thinks that an explorer might find in these mounds a fine field for investigation. With his own limited force of men he was not able to make as extensive excavations as he wished to make; but still, a good deal of work was done. He unearthed a great many polished stone implements, about 300 jars, most of them decorated, and some in very odd shapes, and several specimens of a big stone wheel, and a stone cylinder fitting into it, probably used for some sort of game. The mounds contain houses, and, as usual, most of the relics are found near the dead bodies, which are always buried under the floor, partly under the wall. These people must have been there before the arrival of the cave and cliff dwellers, but who they were it would not yet be safe to say.

MESSRS. T. COOKE AND SONS, of Buckingham Works, York, have issued a new illustrated catalogue of telescopes, surveying and other optical instruments.

MESSRS. GURNEY AND JACKSON (Mr. Van Voorst's successors) hope to have ready for publication by the end of this year the first volume of "A Synonymic Catalogue of Lepidoptera-Heterocera," which Mr. W. F. Kirby, of the Zoological Department, British Museum, has been for some time engaged upon.

SIR J. D. HOOKER'S well-known book of travels, "Himalayan Journals," has been reprinted in the Minerva Library series (Ward, Lock, Bowden, and Co.). It is reprinted from the first (unabridged) edition, with the omission of some of the appendices, which were only of limited general interest. Mr. Murray has supplied copies of the original woodcuts, many of them from drawings by the author.

A NEW Review, which will be partly scientific, is about to be issued at Rome. It is to be published twice in the month, and will be entitled *Natura ed Arte*.

THE admirable Harveian Oration recently delivered by Dr. W. H. Dickinson has just been published by Messrs. Longmans, Green, and Co.

PART 9 of Cassell and Co.'s "Universal Atlas" has just been issued. It contains a map of the Balkan Peninsula, another of China and Japan, and one of Japan alone, the first occupying a double page.

TWO communications upon phosphides of boron have been published by M. Moissan and M. Besson respectively in the most recent numbers of the *Comptes rendus*. M. Moissan has obtained two compounds of phosphorus and boron of the composition PB and P_3B_5, by the reduction of the new compound PBI_2 recently prepared by him (comp. NATURE, vol. xlv. p. 67) in a current of hydrogen gas. M. Besson, however, in July of this year published a note upon one of these compounds, PB, which he obtained by heating the compound $BBr_3.PH_3$ to the temperature of 300° C, and in the current number of the *Comptes rendus* calls M. Moissan's attention to the fact. These compounds of boron and phosphorus appear to be somewhat remarkable substances, and the following is a brief account of their mode of preparation and properties, as described by Messrs. Moissan and Besson. The curious compound PBI_2 is a substance crystallizing *in vacuo* in beautiful bright red crystals. When these crystals are heated in a current of dry hydrogen to a temperature of 450°–500°, three things happen: a small portion of the compound volatilizes unchanged, and forms an annular red deposit upon the cooler part of the tube; another portion loses iodine and yields a second sublimate, yellow in colour, of the other compound of phosphorus, boron and iodine, $(PBI)_x$, prepared by M. Moissan; the remainder of the PBI_2 becomes converted *in situ* into the normal phosphide of boron, PB. The heating of the PBI_2 is best effected in a U-shaped tube immersed in a bath of fused nitre. After the reduction is completed as far as possible, which is determined by the cessation of the evolution of vapour of hydriodic acid, the U-tube is removed from the bath, and the residual phosphide extracted, powdered rapidly, and again placed in the tube, and the reduction continued for a short time longer, in order to insure the removal of the las

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traces of iodine. The phosphide of boron thus obtained is a brown powder, very light in texture, and insoluble in every solvent which has yet been tried. In contact with oxygen the compound ignites at a temperature about 200°, and burns with a very brilliant flame, forming boric and phosphoric anhydrides. With chlorine gas it inflames at the ordinary temperature, producing boron trichloride and phosphorus pentachloride. Vapour of sulphur converts it into sulphides of boron and phosphorus. When thrown into a little fused nitre instant incandescence and deflagration occur. Its behaviour with nitric acid is characteristic; it immediately becomes incandescent, and moves rapidly to and fro over the surface of the acid, all the while burning with a most dazzling flame. It reduces concentrated sulphuric acid to sulphur dioxide. Fused potash decomposes it with evolution of phosphoretted hydrogen and formation of potassium borate. Sodium or potassium, in an atmosphere of hydrogen, react upon warming with great energy, the mass becoming white-hot. Magnesium, heated with the phosphide to 500°, also reacts with incandescence. Even silver and copper react violently upon heating with phosphide of boron. Vapour of water decomposes it at 400°, with production of boric acid and phosphoretted hydrogen. Heated to 300° in ammonia gas it takes fire, and burns with formation of nitride of boron and deposition of phosphorus.

THE second compound of boron and phosphorus, P_3B_5, was obtained by M. Moissan by heating the compound PB just described in a current of hydrogen to a temperature near 1000°. Under these circumstances a portion of the phosphorus is eliminated, and condenses in drops in the colder part of the tube, leaving the P_3B_5 in the form of a light brown powder, which is distinguished from the normal phosphide BP by its indifference to chlorine and nitric acid. It is much more stable than the normal phosphide, but is, like the latter compound, decomposed with incandescence by fused nitre.

THE additions to the Zoological Society's Gardens during the past week include a Formosan Fruit Bat (*Pteropus formosus*) from Formosa, presented by Mr. Thomas Perkins, F.Z.S.; a Patagonian Cavy (*Dolichotis patachonica*) from Patagonia, presented by Mr. H. H. Sharland, F.Z.S.; a Blotched Genet (*Genetta tigrina*) from South Africa, presented by Mr. Edmund R. Boyle; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Mr. G. F. Hawker; a Little Grebe (*Tachybaptes fluviatilis*), British, presented by Mr. T. E. Gunn; a Tuatera Lizard (*Sphenodon punctatus*) from New Zealand, presented by Mr. W. King; a Brush-tailed Kangaroo (*Petrogale penicillata*) from New South Wales, purchased; three Carpet Snakes (*Morelia variegata*) from New South Wales, received in exchange.

OUR ASTRONOMICAL COLUMN.

THE SECULAR ACCELERATION OF THE MOON AND THE LENGTH OF THE SIDEREAL DAY.—Laplace showed that the secular diminution of the eccentricity of the earth's orbit ought to produce in the longitude of the moon a term proportional to the square of the time, and which he determined as $+ 10''t^2$, where t is expressed in centuries. Adams and Delaunay have reduced this term to $+ 6''{\cdot}11t^2$. From a discussion of eclipses Airy concluded that the coefficient of acceleration is as much as 12" or 13"; and accepting this, the question arises as to the cause, other than that indicated by Laplace, which will account for the difference of $6''t^2$. This forms the subject of a paper by M. Tisserand in *Comptes rendus*, No. 20, 1891. Prof. Darwin found that the tidal action between the earth and the moon was sufficient to furnish an apparent acceleration equivalent to the required complement. The accompanying decrease in the earth's rotational velocity produces an apparent acceleration of $3''{\cdot}8t^2$ in the case of Mercury, an amount which may make the longitude of the planet vary by as much as 15" in a couple of hundred years. Since the observed transits of Mercury extend over more than two centuries, M. Tisserand has discussed them with the idea of determining whether the term $3''{\cdot}8t^2$ is really indicated by them. He finds, however, that the extreme transits are not so well represented with the new term as without it, although the difference is not very great. This result, therefore, is unfavourable to the idea as to the variability of the sidereal day, or at least to a variation sufficient to reconcile the result of Airy's research with the calculations of Adams and Delaunay. This being so, it is concluded that the increase in the length of the day, produced by tidal action, has nearly the same value as the diminution which results from the contraction of the earth caused by secular cooling, and that, on account of the compensating action of the two effects, the length of the sidereal day remains very nearly invariable.

STATE OF SOLAR ACTIVITY.—Prof. Tacchini gives, in *Comptes rendus* for November 30, a *résumé* of solar observations made at the Royal Observatory of the Roman College during July, August, and September of this year. The number of days of observation were 31 in July, 31 in August, and 19 in September, and the results obtained are as follows:—

1891.	Relative frequency of spots.	Relative frequency of days without spots.	Relative magnitude of spots.	Relative magnitude of faculæ.
July	18·65	0·00	76·25	82·03
August	8·84	0·06	49·06	70·81
September ...	17·52	0·00	114·45	61·10

A comparison of these numbers with those determined in the preceding quarter shows that solar activity has sensibly increased, for the spotted surface has twice the area. It will be seen that the minimum magnitude of faculæ occurred at the time of a maximum of spots. The following are the results obtained for prominences:—

1891.	Number of days of observation.	Prominences. Mean number.	Prominences. Mean height.	Prominences. Mean extension.
July	30	8·37	40″·2	1·4
August	30	6·77	41·0	1·9
September	23	9·26	41·4	2·2

The number of prominences recorded is greater than during the preceding three months. The highest prominence (142") was observed in August.

OBSERVATIONS OF μ CEPHEI.—Mr. J. E. Gore made some observations of the variable star μ Cephei, the "garnet star" of Sir William Herschel, between January 1888 and December 1890, which show that the variation of light is very irregular, and that the star sometimes remains for several months with little or no perceptible change of magnitude (Proc. Royal Irish Acad., January 26, 1891).

Astronomische Nachrichten, No. 3067, contains an account of the investigation, carried out by Herr Dr. Walter Wislicenus on the "Influence of Ring and Disk Blinds in Micrometic Measurements," in order to account for the following phenomenon. If one lifts a transit off its pillars and places it so that it does not interfere with the line of sight of the collimators, and then brings the central wires of each collimator exactly in coincidence, it is found that, by putting the meridional circle back again, and placing it in its vertical position with the apertures in the central cube open, coincidence of the wires no longer exists, but a slight displacement is noticed. It may be remembered that this question was raised at Greenwich as early as the year 1868, while in the two following years, from observations made in that interval, a correction of 0"·48 and 0"·58 was found for the difference of reading. In 1874 this discrepancy was accepted as real, and corrections for it were made, but no real origin for it was assigned. Mr. Turner, in the year 1886, also investigated this difference of reading, employing the collimators of the transit circle at Greenwich, and the numerical results obtained were given in vol. xlvi. p. 329, of the *Monthly Notices*. By using a wooden model of the central cube of the transit, he got essentially the same results as those given by the cube in the ordinary manner, but both were in discordance with the readings taken when nothing was interposed. To account for the difference he says: "The discrepancy is due to a real difference between the lines of collimation of the central and eccentric portions of the object-glasses of the collimators."

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In Herr Dr. Wislicenus's experiments, six blinds of varying diameters were employed, and were placed on the cube of the Strassburg meridional circle to represent different central-cone apertures. He measured the difference between the readings taken with and without these blinds on five separate days, in the two positions, horizontal and vertical, of the collimator threads. To still further vary the method, he removed the meridional circle, and placed the blinds on the collimators, making another series of observations, the collimator threads being again in these two positions. From the above measurements he concluded (to state it very briefly): (1) that the differences obtained with the Greenwich circle are of a purely optical nature, and can be easily removed by making the aperture of the central cube somewhat larger than the full aperture of the collimators; (2) and also if the objective of a telescope be screened quite symmetrically by concentric rings or disks, or by such an arrangement as that in the Greenwich instrument, there occurs not only a variation in the focal image as regards sharpness and brightness, but there can also be found the same displacement. In discussing the observations and conclusions arrived at, he mentions that in the best objectives the same colour rays do not combine in a point on the optical axis, but in such a way that one does not obtain a focus but a focal line of unequal brilliancy, from the brightest point of which one deduces the focal plane of the lens; he then goes on to say that since the optical axis of the lens forms therefore an angle with that of the objective, the displacement of the brightest point of the focal line would not fall perpendicular on the focal plane of the lens, but one would have to observe it with the lens somewhat on one side, by this means one would be able to see its projection on the focal plane of the lens. Therefore, "by the existence of a centering-error the displacement of the focal image by the insertion of blinds before the objective would be explained."

THE *Annales* of the University Observatory in Vienna, vol. vii., contains all the observations of planets and comets made in the years 1887–89, with the Fraunhofer's, Clark's, and Grubb's refractors of apertures 16·2 cm., 30·1 cm., and 68·0 cm. respectively, together with the reduced results of the above. In addition to the work mentioned, the Grubb refractor was extensively employed in the study of the nebula in the Pleiades, special attention being given to the Merope nebula, which forms the chief topic of discussion in the interesting report towards the end of the volume; an excellent illustration also of the nebula itself is added, in which are shown all the fundamental stars with many others of smaller magnitude.

Of the other illustrations given, there are three very good pictures of the moon, taken with the same instrument. Plate I. is the result of an exposure of 6 seconds taken on an orthochromatic plate, and for sharpness and clearness is excellent. Plate II., which is an enlargement of a part of Plate I. enlarged four times is also very fine. Plates IV., V., and VI., contain drawings of comets and nebulæ, and are accompanied with descriptions of their peculiarities.

Altogether this volume is of a most interesting nature, and shows the result of a great amount of painstaking and useful work, which will be welcomed by all astronomers.

THE EASTERN TAURUS AND ANTI-TAURUS.

AT the meeting of the Royal Geographical Society, on Monday evening, the paper read was on the Passes of the Eastern Taurus and Anti-Taurus, by Mr. D. G. Hogarth.

The paper described the general characteristics, geographical and ethnographical, of the eastern half of the mountain system of Southern Asia Minor, and is based on experience gained by the author in the course of journeys in 1887, 1890, and 1891, undertaken under the auspices of the Asia Minor Exploration Fund, to which the Royal Geographical Society has been a generous contributor. In 1890, Prof. W. M. Ramsay was the head of the Expedition, and though in the other years the author was not accompanied by him, he followed lines which that great authority on Asia Minor had laid down. Mr. H. A. Brown (author of "A Winter in Albania"), the Rev. A. C. Headlam, of All Souls' College, and Mr. J. A. R. Munro, of Lincoln College, Oxford, took part in the expeditions in different years. The first object of the journeys was archæological, to carry on the brilliant work of Prof. Ramsay commenced in 1881, but the members of the expeditions have always taken geographical notes and observations in traversing the interior of Asia Minor, about many parts of which less is known in modern than was known in ancient times. In following old trade-routes across the mountains, he explorers have traced the modern tracks, for the limits of ancient and modern geography are very often not to be distinguished in Asia Minor. Much of the peninsula is a land of the dead, but much also possesses great interest in the present, and, may be, will acquire an interest of a different kind for England in the near future. It has been explored by many travellers, from Pococke, Hamilton, Leake, and Ainsworth, to the archæologists who have penetrated it in different directions during the past twenty years, and the trained surveyors, led by Sir Charles Wilson, who did so much geographical work in it ten years ago. But Asia Minor is very large, often very difficult to traverse, and of very varied character, as is to be expected in the meeting-place of so many civilizations and faiths, ancient and modern. Much has yet to be done before western geographers can claim even a superficial knowledge of its whole area, and many parts have never been visited by any explorer at all.

The first district described is the wild mountainous region between the beautiful lakes of Egerdir and Beysheher, remarkable for the absence of passes, for the great gorge of Eurymedon, and for the primitive character of the indigenous population who live cut off from the world. Not less noteworthy are the extraordinary ruins of the Pisidian city of Adada, which exist high up among the hill-tops, and are now called *Kara-Bavlo*, a name which recalls that of St. Paul, and probably is derived from a great church dedicated to the Apostle in commemoration of a sojourn on his way from Perga to Antioch in 45 A.D. These ruins preserve the most perfect specimen of an Anatolian city of Roman days. Passing by the sites of Lystra and Derbe, the Low Taurus is reached, a marked depression between the high inter-lacustrine ranges and the Bulgar Dagh, which begins about 70 miles west of the Cilician Gates (Gulek Boghaz). The waterless, arid character of the northern, and beautiful scenery of the southern slopes, especially in the Calycadnus valley, are described in connection with the routes radiating from Karaman. The remarkable ruins of the monastery of Koja Kalessi, which contain a very perfect church of the early fifth century at the latest, and of the city of Coropissos, add archæological interest to this section of the Taurus. The eastern part of this region is a veritable Pompeii, where Roman cities, villages, and roads have been left to decay in a deserted country.

The high Taurus is reached near Eregli. The famous defile known as the "Cilician Gates" has been often described, but not so the important passes further east, from Sis to Hadjin and Gyuksun; from Marash to Gyuksun, Zeitun, and Albistan; and from Adiaman to Besni and Malatia. The Eastern Taurus is a region of great beauty, richly wooded, and traversed by the tremendous cañons of the Samanti, the Saros, and the Jihan, not passable even on foot. Whenever a railway is made from Asia Minor towards the Euphrates, it will take the gorge of the latter river, which in ancient times was rendered possible for a road. The ethnographical and historical interest of this region is very great, as it formed the refuge of the last independent Armenians of Cilicia, whose robber-towns, Hadjin and Zeitun, are described by the author. Of late their exclusive possession has been disputed by Circassians and Kurds, the latter retaining curious traces of their pre-Islamite rites and customs.

Lastly, the principal passes into the Anti-Taurus from the west, and out on the east in the direction of the Euphrates, are briefly noticed. The Anti-Taurus district is one of the most curious in Asia Minor; man deserted it almost entirely from the eleventh century until less than a century ago, when nomadic Avshar and Kurds penetrated to its remote and lofty valleys. Thus has been preserved so much of the great Roman military road to the Euphrates in the valleys of the Saros and Gyuk Su, with a series of milestones recording its many restorations; to the same cause we owe the interesting ruins of Comana, and "Hittite" monuments, recalling very early days, when a great trade-route, afterwards identical with the Royal Persian road, already took this line. Of different but equal interest are the modern inhabitants, nomadic Avshar, and half-troglodyte Kurds, nominally Musulmans, but really worshippers of other gods than that of Islam; and newly-imported Circassians, settled near troublesome Armenian strongholds as a menace and a check. The medley of races in this remote region, for whose control the Turks seem able to make no adequate provision, suggests speculations as to the possible future of race-supremacy in the Ottoman Empire.

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THE ZOOLOGY AND BOTANY OF THE WEST INDIA ISLANDS.[1]

THIS Committee was appointed in 1887, and it has been reappointed each year until the present time.

During the past year Mr. F. DuCane Godman, F.R.S., has continued to employ a collector in the island of St. Vincent, and owing to the valuable assistance thus afforded to the Committee it has been possible to complete the exploration of this island. The collections in zoology are very extensive, and those in botany extend to the whole of the phanerogams and the vascular cryptogams. No expense has been incurred by the Committee in regard to any of these collections in St. Vincent.

The services of Mr. R. V. Sherring, F.L.S., were accepted, as mentioned in the last report, to make botanical collections in the island of Grenada. He left this country in October last, and returned after seven months' absence in June last. Mr. Sherring has forwarded to this country large collections, consisting for the most part of vascular cryptogams, and these are now in course of being determined at Kew. A detailed report on the various collections in zoology and botany received during the past year is given below.

At the present time Mr. Herbert H. Smith, the collector employed by Mr. Godman, is making collections in zoology in the island of Grenada. This is the most southerly of the chain of islands intended to be explored by the Committee. When this island is completed, the Committee will have been engaged in investigating the fauna and flora of most of the islands in the Lesser Antilles lying between Martinique and Trinidad. The islands in which collections have so far been made consist of Dominica, St. Lucia, Barbados, St. Vincent, the Grenadines, and Grenada.

Zoology.—Since the last report collections have continued to be received from St. Vincent by Mr. Godman. The work of sorting and arranging these collections has been begun. The arthropods are already completed, and the larger part of the insects is mounted and prepared for despatch to the specialists who have been engaged to work them out.

For this purpose the Committee have been so fortunate as to obtain the assistance of the following naturalists: Herr Hofrath Brunner von Wattenwyl for the Orthoptera; Prof. Riley for the Rhynchota; Mr. Howard for the parasitic Hymenoptera; Prof. S. W. Williston for the Diptera; Prof. Aug. Forel for the Ants; Lord Walsingham for Lepidoptera, part; F. D. Godman and O. Salvin for Lepidoptera, part; D. Sharp for Coleoptera; M. Simon for Spiders generally; Mr. G. W. Peckham for Attidæ. The Committee have undertaken to procure publication of the memoirs that may be received from these *savants*.

A small collection of specimens made by Dr. H. A. Alford Nicholls, F.L.S., local secretary to the Committee in the island of Dominica, was received in May last. This consisted of nine mammals, one lizard, one snake, five fishes, one *Ascalaphus*, twelve Longicornia, two Passalidæ, and five Lamellicornia. Besides these Dr. Nicholls sent from the island of Tobago four of the peculiar nests of the yellow-tailed bird of that island (*Cassicus cristatus*). These birds usually build their nests depending from isolated branches of the silk-cotton tree (*Eriodendron anfractuosum*), and they look like huge fruits waving in the wind.

A small collection of Lepidoptera was received in November last from Captain Hellard, R.E., local secretary to the Committee in the island of St. Lucia. The mounted specimens in this collection arrived in bad order, owing to the pieces of camphor getting loose in the boxes and breaking the greater part of them, including almost the whole of the *Sphingidæ*.

Mr. John C. Wells, who has devoted attention to the ornithology of Grenada, has kindly consented to act as a local secretary for that island.

Botany.—Of the botanical collections received from St. Vincent the vascular cryptogams have been determined by Mr. J. G. Baker, F.R.S., and an account of them, with two plates, printed in the *Annals of Botany*, vol. v. (April 1891) pp. 163–172. Amongst the ferns the most striking novelty is a new species of *Asplenium*, named *A. Godmani*, Baker (pl. xi.), found in damp forests at the extreme top of Morne à Garou. Other new species are *Hymenophyllum vincentinum*, Baker (pl. x.), and *Acrostichum* (*Elaphoglossum*) *Smithii*, Baker. The total number of vascular cryptogams found recently in St. Vincent amounts to 168 species. Most of these are widely spread through tropical America, and only four are endemic. In addition to the above a new species of *Hepaticæ*, also from St. Vincent (*Kantia vincentina*, C. H. Wright), was described in the *Journal of Botany*, vol. xxix. (April 1891), p. 107.

Of the phanerogams from St. Vincent and some of the Grenadines the work of determining these is being carried on as expeditiously as circumstances permit. The collection is a very large one, and the results so far attained are contained in the following memorandum prepared by Mr. R. A. Rolfe:—

The flowering plants have been determined as far as the end of Rutaceæ. Those from St. Vincent number slightly over a hundred species, of which about thirty, consisting for the most part of common West Indian plants, were not previously recorded from the island. The most interesting plant is a species of *Trigyneia* (apparently new), a small tropical American genus of Anonaceæ not hitherto detected in the West Indies. A *Clusia* and a species of *Tetrapterys*, which cannot be identified, may also prove new. The remainder have been fully determined. The three most interesting of these are *Norantea Jussiæi*, Tr. and Pl., previously known only from Guadaloupe and Dominica; *Zanthoxylon microcarpum*, Griseb., from Dominica and Trinidad; and *Z. spinosum*, Sw., from Dominica, Jamaica, and Cuba. The composition of the flora of the Lesser Grenadines, situated between St. Vincent and Grenada, was previously almost unknown. The plants hitherto determined are as follows:—From the island of Bequia, 34 species; from Mustique, 18; from Canouan, 5; and from Union, the nearest to Grenada, 5. They are, without exception, common West Indian plants, and are all also natives of St. Vincent. From the results hitherto obtained it seems clear that the flora of the Lesser Antilles is tolerably uniform throughout, although the larger islands of Dominica, Martinique, St. Lucia, and possibly St. Vincent, appear to have each a very small endemic element.

The collections made by Mr. Sherring at Grenada consist of nearly 6000 specimens of vascular cryptogams and about 1000 specimens of phanerogams. The number of species of ferns is about 140, and of these two are new, viz. *Alsophila Elliottii*, Baker, and *Acrostichum Sherringii*, Baker. The phanerogams have not yet been worked out. Sixty species of ferns were previously known from Grenada from collections made by Mr. G. R. Murray, F.L.S., and Mr. W. R. Elliott. Mr. Sherring has increased this number to 140. The species of greatest interest, other than those known to be new, are *Asplenium Godmani*, Baker, recently found in St. Vincent; *Polypodium Hartii*, Jenman, first described in 1886, and known only in the mountains of Jamaica and Dominica; and *Acrostichum Aubertii*, widely spread in continental America, but new to the West Indies. Other interesting plants collected by Mr. Sherring are *Schizæa fluminensis*, Miers, new to the West Indies, but believed to be only a shade variety of *S. dichotoma*, and *Danæa polymorpha*, Leprieur, a critical form of which but little is known.

An account of vascular cryptogams collected at Grenada is in course of being prepared for the *Annals of Botany*.

Mr. Sherring has prepared an interesting report on the flora of Grenada, and this will prove of great interest to students of West Indian botany.

A collection of plants was received from Dr. Nicholls at the same time as the specimens in zoology already noticed. These consisted of fifty-six species of vascular cryptogams—all of them, were, however, well-known West Indian plants—and a small number (175 numbers) of phanerogams. The latter have not yet been determined.

The Committee recommend their reappointment, with the following members: Dr. Sclater, Mr. Carruthers, Prof. Newton, Mr. Godman, Dr. Günther, and Dr. Sharp. The Committee also recommend that the grant of £100 placed at their disposal, but not expended during the current year, be renewed.

[1] Fourth Report of the British Association Committee, consisting of Prof. Flower (Chairman), Mr. D. Morris (Secretary), Mr. Carruthers, Dr. Sclater, Mr. Thiselton-Dyer, Dr. Sharp, Mr. F. DuCane Godman, Prof. Newton, Dr. Günther, and Colonel Feilden, appointed for the purpose of reporting on the present state of our knowledge of the Zoology and Botany of the West India Islands, and taking steps to investigate ascertained deficiencies in the Fauna and Flora.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Mr. A. E. Shipley, Demonstrator of Comparative Anatomy, has been appointed Secretary to the Museums Syndicate; and Mr. S. F. Harmer, Demonstrator of Invertebrate Morphology, Superintendent of the Museum of Zoology, in the room of Mr. J. W. Clark.

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Dr. William Ewart and Mr. Frederick Treves have been appointed Additional Examiners for the Third M. B. Examination, for which the number of candidates is unusually large.

Candidates for Lord Walsingham's Gold Medal in Biology and Physiology are requested to send in their essays to Prof. Newton by October 1, 1892.

An interesting report on the course of study pursued in Cambridge by the Local Lecture students during the Long Vacation, 1891, appears in the *Reporter* of December 8. Scientific courses on invertebrate palæontology, chemistry, physics, and physiology were given with success in the University Laboratories; while single lectures, with the object of inspiring interest in other departments, were given by Dr. Hill, Dr. D. MacAlister, and Prof. Darwin. Courses in general literature and art were also arranged, and the result of the whole is deemed by the Syndicate so satisfactory that they propose to regard the Long Vacation scheme as part of their regular work. Forty-seven students took advantage of the facilities offered by the University for acquiring a closer knowledge of the subjects they had commenced under the University Extension Lectures.

It is proposed that two new Syndicates shall be appointed—the first, to be called the Engineering Laboratory Syndicate, is to make arrangements for the further development of the Engineering School in the University, and in particular to endeavour to raise funds for its adequate endowment; the second is to consider the establishment of an Honours Examination in Mechanical Science.

SCIENTIFIC SERIALS.

THE *Quarterly Journal of Microscopical Science* for June 1891 contains:—On the renal organs of certain Decapod Crustacea, by W. F. R. Weldon (plates xxi. and xxii.). It would appear that the nephro-peritoneal sacs of the Decapoda should be regarded rather as enlarged portions of a tubular system, such as that found in Mysis and in the Thalassinidæ, than as persistent remnants of a "cœlomic" body-cavity, into which tubular nephridia open.—On the nephridium of Lumbricus and its blood-supply, with remarks on the nephridia in other Chætopoda, by Dr. W. Blaxland Benham (plates xxiii. to xxv.). In this memoir the author settles the nomenclature of the parts of the nephridium and the course of the various regions; details the structure of these regions in Lumbricus; institutes a comparison with the nephridium in other genera; describes the nephrostome or funnel in *Perichæta malamaniensis*, n.sp., and some other genera; and describes and figures the vascular supply of the nephridium in Lumbricus and in Arenicola.—Notes on the Naidiform Oligochæta, containing a description of new species of the genera Pristina and Pterostylarides, and remarks upon cephalization and gemmation as generic and specific characters in the group, by Dr. A. Gibbs Bourne (plates xxvi. and xxvii.).—On *Pelomyxa viridis*, sp.n., and on the vesicular nature of protoplasm, by Dr. A. Gibbs Bourne (plate xxviii.). This new species of Pelomyxa was found in the mud of a small tank in the neighbourhood of the Madras Presidency College; it would seem to be about the largest known species of the Lobosa, spread out specimens average one-third of an inch in diameter; the vesicles contained chlorophyll, and were numerous; the protoplasm was densely packed with bacteria, the "crystals" of Greef; the pseudopods were coarse and blunt; no reproductive processes were noted; nuclei and nucleoli were present in numbers.—On the medusæ of *Millepora murrayi*, and on the gonophores of Allopora and Distichopora, by Dr. Sydney J. Hickson (plates xxix. and xxx.). From specimens from Torres Straits, preserved by Prof. Haddon, the author has determined that the ampullæ described in the hard parts of this Millepora by Quelch are the cavities containing male medusæ. The medusa of Millepora is a transformed zooid—that is, it is not from the first modified to bear the spermarium, but it is an ordinary zooid of the colony changed into a medusa after the migration of spermospheres into its ectoderm, which are subsequently developed there; these medusæ escape from the ampullæ before the spermatozoa are matured. The male gonophores of Distichopora differ from those of Allopora, but the female gonophores of these genera closely resemble one another. The gonophores of the Hydrocorallinæ and Hydromedusæ are carefully compared; those of the former are not degenerate medusæ.—On a red pigment-forming organism, *Bacillus corallinus*, by C. Slater (plate xxxi.).

October 1891 contains:—On immunity against microbes, by Dr. Armand Ruffer, Part 2 (plates xxxii. and xxxiii.). While the first part of this memoir treated of the struggle which takes place in the healthy body between micro-organisms and amœboid cells, this part details what happens where these organisms have found their way into the tissues of animals. It would be impossible to do justice to the contents of this valuable memoir by an abstract.—On the formation and fate of the primitive streak, with observations on the archenteron and germinal layers of *Rana temporaria*, by Dr. A. Robinson and R. Assheton (plates xxxiv. and xxxv.). The primitive streak is formed in the frog by concrescence of the lips of the blastopore from behind forwards; the ventral moiety of the primitive streak, shortly after the perforation of the anus, ceases to exist and splits up; the dorsal moiety becomes folded upon itself, like and along with the neural plate, and becomes separated from the skin; it gives rise to the whole of the tail with the exception of the greater part of the skin.—On some points in the histology and development of *Myriothela phrygia*, by W. B. Hardy (plates xxxvi. and xxxvii.).—On the structure of an earthworm allied to Nemertodrilus, Mich., with observations on the post-embryonic development of certain organs, by Frank E. Beddard (plates xxxviii. and xxxix.).—On some points in the development of *Scorpio fulvipes*, by M. Laurie (plate xl.). The development of this form adds another to the numerous types of development in the Arrachnida; it is, as shown by its mode of nutrition, a highly specialized form. There is no doubt that the type of development represented by Euscorpius is the more primitive of the two. The chief arguments in favour of this are the remarkable facts of the formation in *Scorpio fulvipes* of a rudimentary amnion, and the formation of yolk-spheres in the earlier stages, and a mass of yolk round which the gut is formed. — Abstract of Maupas's researches on multiplication and fertilization in Ciliate Infusorians, by Dr. Marcus M. Hartog.—On the occurrence of pseudopodia in the Diatomaceous genera Melosira and Cyclotella, by J. G. Grenfell (plate xli.). The author states that he has found pseudopodia in *Cyclotella kutzingiana*, and in one or two small species of Melosira. "At Heytesbury, in Wiltshire, the River Wiley and the brooks were found full of a Melosira in small isolated frustules, with long delicate pseudopodia; a good set of Cyclotellas with pseudopodia were found at Kew Gardens." In the large pond in the gardens of the Botanical Society of London, frustules of Melosira gathered with some specimens of *Archerina boltoni* were found with these pseudopodia; sometimes they were easily seen for a part of their length with a ⅛ object-glass, but in some cases, as in the diatoms from Kew, "they are generally invisible, even when specially looked for." These pseudopodia are best seen on "well dried material," they are fairly stiff, and are non-retractile to ordinary observation. In length they vary, in Cyclotella from two and a half to six times the width of the valves; they are very permanent, in specimens kept in water they remained apparently unchanged for months; they are generally fairly straight, but sometimes they branch and sometimes two or three spring from a short thickened base. "As to the use of these pseudopods, and the question why other Diatoms do not have them, the chief point to be remembered is that these little Melosiras and Cyclotellas occur mainly as isolated frustules, and are without the power of locomotion. Under these circumstances the pseudopodia serve the purposes of protection, means of attachment, and floats." The author says: "facts point conclusively to the substance of these pseudopodia being protoplasm." It is quite probable that "some kind of cuticle is secreted by the protoplasm." As to the possibility of these growths not being pseudopodia at all, he combats the idea, and institutes a comparison between them and the radiating structures met with in Archerina (the reference to Prof. Ray Lankester's description of this very interesting form should be *Q. J. M. Sc.*, vol. xxv., 1885, p. 61), but it will be remembered that Prof. Ray Lankester refers to the "delicate but stiff filaments" in his description of Archerina, and only uses the term pseudopodia in a conventional sense. As yet we are not convinced by a perusal of Mr. Grenfell's paper that the Diatoms possess pseudopodia in any sense of this term, or that they have any affinities with the Heliozoa.—A very ably written review, signed by George Bidder, on "Dendy's Monograph of the Victorian Sponges, Part I.," which has just been published at Melbourne, concludes this part and volume xxxii. (N.S.) of the *Journal*. The first part of Dr. Dendy's monograph is devoted to the account of the Calcareous Sponges, a group on which Mr.

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Bidder has himself been working at Naples for the last five years. His appreciatory remarks on Dendy's researches prove how much of interesting and new matter lies in manuscript in the laboratory at Naples, and leads us to express the hope that Mr. Bidder will soon follow the example of his senior, and give us a monograph of the Calcarea Homocœla of the Gulf of Naples; with more details on the glandular ectoderm.

Travaux de la Société des Naturalistes de St. Pétersbourg, Section de Zoologie et de Physiologie, tome xxi., livr. 1 (Russian).—On the influence of temperature, and the distance from the section of a nerve, on its electrical irritability, by B. F. Verigo.—Observations on the *Araneina*, by V. Wagner.—The minutes of proceedings contain several interesting notes: namely, a list of the Bryozoa of the Murman coast of Russia, by M. Khvorotansky, containing eighty-one species; on the blood of some invertebrates, by V. Wagner, from which it appears that it always contains two different kinds of cells—the granulous and coloured ones, and the amœboid ones or leucocytes, besides some other cells which, however, must be considered as derived from the above two kinds.—M. Shimkevitch's remarks on the artificial incubation of ostriches in the ostrich park at Heliopolis are also worth mentioning.

Bulletin de la Société des Naturalistes de Moscou, 1891, No. 1.—On the group of the sillimanite and the part played by aluminium in the silicates, by W. Wernadsky (in Russian, summed up in French). The paper contains, besides the description of the experiments already published in the *Comptes rendus*, a discussion of the facts, which brings the author to the following conclusions: the compounds of silicon with aluminium have an acid reaction; they may be embodied in one group, that of the sillimanite. Some of them are hydrates, and some others are salts of these, or of other possible anhydrides. Polymorphic varieties arise in this group with the change of physical conditions, without any perceptible change in the chemical composition.—On the morphology and classification of the Chlamydomonads, by Prof. Goroshankin (in German, with three coloured plates); being a full monograph on the family, in which the following new species are described: *Chlamydomonas De-Baryana*, *C. Perty*, *C. Steinii*, *C. Kuteiniowi*, *C. reticulata*, and *C. Ehrenbergii*.—On some peculiarities in the development and the structure of the skull of *Pelobates fuscus*, by A. N. Sewertzow.—Note on the *Hipparion crassum*, by Marie Pavloff (French).—On the fossil plant-bearing deposits of East Russia and Siberia, by C. Kosmovsky (in French). The close similarity between the supposed Jurassic fresh-water deposits of East Russia and Siberia and the "Artinsk" series is briefly indicated.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, November 19.—"On the Loci of Singular Points and Lines which occur in connection with the Theory of the Locus of Ultimate Intersections of a System of Surfaces." By M. J. M. Hill, M.A., Sc.D., Professor of Mathematics at University College, London. Communicated by Prof. Henrici, F.R.S.

Introduction.

In a paper "On the c- and p-Discriminants of Ordinary Integrable Differential Equations of the First Order," published in vol. xix. of the Proceedings of the London Mathematical Society, the factors which occur in the c-discriminant of an equation of the form $f(x, y, c) = 0$, where $f(x, y, c)$ is a rational integral function of x, y, c, are determined analytically.

It is shown[1] that if $E = 0$ be the equation of the envelope locus of the curves $f(x, y, c) = 0$; if $N = 0$ be the equation of their node locus; if $C = 0$ be the equation of their cusp locus, then the factors of the discriminant are E, N^2, C^3.

The object of this paper is to extend these results to surfaces.

PART I.—*The Equation of the System of Surfaces is a Rational Integral Function of the Co-ordinates and one Arbitrary Parameter.*

When there is only one arbitrary parameter, each surface of the system intersects the consecutive surface in a curve, whose equations are the equation of the surface and the equation obtained by differentiating it with regard to the parameter. (These equations will be called the fundamental equations in this part.) Hence each surface touches the envelope along a curve. It is known that the equation of the envelope may be obtained by eliminating the parameter from the fundamental equations and equating a factor of the result to zero. But it frequently happens that there are other factors of the result (or discriminant) which, when equated to zero, do not give the equation of the envelope.

These factors are connected with loci of singular points. If each surface have one singular point, the locus of all the singular points of the surfaces of the system is a curve. Its equations, therefore, cannot be found by equating a factor of the discriminant to zero. But if each surface of the system have upon it a nodal line, then the locus of the nodal lines of all the surfaces is a surface, and its equation may be found by equating to zero a factor of the discriminant.

The singular points in space, the form of which depends only on the terms of the second order, when the origin of co-ordinates is taken at the singular point, are:—

(i.) The conic node.
(ii.) The biplanar node or binode.
(iii.) The uniplanar node or unode.

It is shown that a surface cannot have upon it a curve at every point of which there is a conic node. Hence there are two varieties of nodal lines to be considered; the first, being such that every point is a binode, may be called a binodal line; and the second, being such that every point on it is a unode, may be called a unodal line.

It is shown that if $E = 0$ be the equation of the envelope locus, $B = 0$ the equation of the locus of binodal lines, $U = 0$ the equation of the locus of unodal lines, then the factors of the discriminant are, in general, E, B^2, U^3, respectively.

This is the general theorem, but it is assumed in the course of the investigation, when the discriminant is being formed, that the fundamental equations are satisfied by only one value of the parameter at each point on the envelope locus or on a locus of binodal or unodal lines.

The investigation is accordingly carried a step further, and it is shown that if the fundamental equations are satisfied by two equal values of the parameter at points on an envelope locus, or on a locus of binodal or unodal lines, the factors of the discriminant are E^2, B^3, U^4, respectively.

The geometrical meaning of the condition that the fundamental equations are satisfied by two equal values of the parameter in the case of the envelope is that the line of contact of the envelope with each surface of the system counts three times over as a curve of intersection, instead of twice as in the ordinary case. The meaning of the condition in the case of the loci of singular lines is that each of these loci is also an envelope.

PART II.—*The Equation of the System of Surfaces is a Rational Integral Function of the Co-ordinates and two Arbitrary Parameters.*

When there are two arbitrary parameters in the equation of the system of surfaces, the equation of the locus of ultimate intersections is found by eliminating the parameters between this equation and the two equations obtained by differentiating it with regard to the parameters. (These equations will in this part of the investigation be called the fundamental equations.)

In general the locus of ultimate intersections is a surface. The exceptional cases in which it is not a surface are enumerated at the end of the paper. These include the case where the equation of the system of surfaces is of the first degree in the parameters. Hence it will be supposed that the degree of the equation of the system of surfaces in the parameters is above the first.

In general, the locus of ultimate intersections possesses the envelope property, and the equation of the envelope is determined by equating the discriminant, or a factor of it, to zero.

If factors of the discriminant exist which, when equated to zero, give surfaces not possessing the envelope property, then these surfaces are connected with loci of singular points.

Now the locus of singular points of a system of surfaces whose equation contains two arbitrary parameters is in general a curve. Hence its equations cannot be determined by equating to zero a factor of the discriminant.

But if every surface of the system have a singular point, then *in general* its co-ordinates may be expressed as functions of the two parameters of the surface to which it belongs. Hence the locus of the singular points is a surface. It will be proved that it is a part of the locus of ultimate intersections. Hence its

[1] The theorem was originally given by Prof. Cayley, in the *Messenger of Mathematics*, vol. ii., 1872, pp. 6–12.

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equations can be obtained by equating to zero a factor of the discriminant.

Let E = 0 be the equation of the envelope locus,
C = 0 be the equation of the conic node locus,
B = 0 be the equation of the biplanar node locus,
U = 0 be the equation of the uniplanar node locus.

Now at any point on the locus of ultimate intersections—

(I.) *There may be one system of values of the parameters satisfying the fundamental equations.*

In this case there may be envelope, conic node, or biplanar node loci; and the corresponding factors of the discriminant are E, C^2, B^3 respectively.

(II.) *There may be more than one system of distinct values of the parameters satisfying the fundamental equations.*

In this case the effect of the distinct values is additive. Thus if there be p systems of values at a point on the envelope locus, the factor E would occur to the pth power.

(III.) *Two or more systems of values of the parameters satisfying the fundamental equations may coincide.*

The results must be stated differently in the cases (α) where the degree in the parameters of the equation of the system of surfaces is greater than two; (β) where the degree in the parameters of the equation of the system of surfaces is two.

In the case (α) it will be shown that there may be envelope loci in which the envelope has stationary contact with each surface of the system; conic node loci, which are also envelopes; biplanar node loci, in which the edge of the biplanar node always touches the biplanar node locus; and uniplanar node loci: and the corresponding factors of the discriminant are E^2, C^3, B^4, U^6 respectively.

The case (β) always falls under the next case—

(IV.) *The values of the parameters satisfying the fundamental equations may become indeterminate.*

If the equation of the system of surfaces be of the second degree in the parameters, and the analytical condition hold which expresses that the fundamental equations are satisfied by two coinciding systems of values, then this condition requires to be specially interpreted. For now the second and third fundamental equations are of the first degree in the parameters, so that if they are satisfied by two coinciding systems of values, they must be indeterminate.

It is, however, possible to determine a *single* system of values of the parameters satisfying them. In this case the three surfaces represented by the fundamental equations intersect in a common curve (which is fixed for fixed values of the parameters) lying on the locus of ultimate intersections; whereas in the previous cases they intersect in a finite number of points lying on the locus of ultimate intersections.

The surface of the system, corresponding to the fixed values of the parameters, touches the locus of ultimate intersections along the above-mentioned curve.

In general, there are *two* conic nodes of the system at every point of the locus of ultimate intersections. The parameters of the surfaces having the conic nodes are determined by two quadratic equations, called the parametric quadratics; and in general the roots of *each* parametric quadratic are *unequal*. In this case the corresponding factor of the discriminant is C^2. If the roots of *both* parametric quadratics are *equal*, the two surfaces having conic nodes are replaced by one surface having a biplanar or uniplanar node. In this case the corresponding factors of the discriminant are B^3, U^4, respectively.

If the parameters of *one* of the surfaces having a conic node become *infinite*, this surface may be considered to disappear, and there is but one conic node at each point of the locus of ultimate intersections. In this case the corresponding factor of the discriminant is C^2.

If the parameters of *both* surfaces having conic nodes become *infinite*, both these surfaces may be considered to disappear, and the locus of ultimate intersections is an envelope locus (touching each surface of the system along a curve). In this case the corresponding factor of the discriminant is E^3.

If the parameters of *both* surfaces having conic nodes become *indeterminate*, then there are at each point an infinite number of biplanar nodes, and each surface of the system has a binodal line lying on the locus of ultimate intersections. In this case the corresponding factor of the discriminant is B^4.

Physical Society, November 20.—Prof. W. E. Ayrton, F.R.S., President, in the chair.—Dr. Philippe A. Guye gave a short account and discussion of the various forms which have been given to the general equation expressing the behaviour of liquids and gases under different conditions of volume, temperature, and pressure, by Van der Waals, Clausius, Sarran, Violi, Heilborn, and Tait. He first considered the equation of Van der Waals, which, although only an approximation to the true one, may be made to lead to numerous important deductions. He then showed that, of the various more exact formulæ proposed, that of Sarran is the simplest, and may be used with less expenditure of time and trouble than any of the others. In conclusion, he insisted on the necessity of experimental researches as the only means of arriving at a definite conclusion as to which of the various formulæ is the true one; such researches should involve determinations, as exact as possible, of the critical constants, and of isotherms at high temperatures and great pressures. Prof. Ramsay inquired whether the constants in the formula of Clausius had any physical meaning, or were they merely numbers? M. Guye, in reply, said that, although *some* of the constants in the improved formulæ had physical interpretations, Van der Waals's equation was the only one in which *all* the constants had precise physical significations. Prof. Rücker said it was only necessary to look at the formulæ to see how important a factor Van der Waals's expression had been in later developments of the subject. Although it did not agree with experiment under all conditions, particularly at small volumes, yet it was a close approximation over a considerable range, and was the only formula in which all the constants had definite physical meanings. Prof. Tait had pointed out that the number of constants were too few to fully represent the facts, for, by following Andrews's reasoning, he had shown that about the critical point a straight line cuts the isotherm in five points. Nevertheless, during the last twenty years all the so-called improved formulæ were modifications of Van der Waals's expression, and this, he thought, showed how valuable the original formula was. Prof. Fitzgerald said he once tried how far Clausius's formula agreed with the experimental results published by Messrs. Ramsay and Young, and after several months' work, relinquished it on account of the tremendous labour involved. He thought that such complicated formulæ retarded rather than advanced science; simple ones (even if less accurate) were likely to lead to greater advancement. Prof. Carey Foster remarked that the expression $pv = RT$, which is nearly true for gases, was the starting-point of all subsequent advances. Van der Waals had arrived at a still closer approximation by taking into account the volume occupied by the particles and their mutual pressure. The President said Van der Waals's memoir had been adversely criticized because of its supposed insufficient recognition of Andrews's investigations on the subject. Better acquaintance with the work had, however, shown this criticism to be undeserved.—Dr. C. V. Burton read a paper on a new theory concerning the constitution of matter. It is assumed that it is possible to have in the ether a distribution of strain which is itself in equilibrium. Such a distribution is called a "strain-figure." An atom is looked upon as an aggregation of strain-figures, the possible *varieties* of strain-figures (and hence of atoms) being limited by the conditions of equilibrium, and the *sizes* of possible strain-figures dependent on the coarse-grainedness of the turbulent motion or other structure of the ether. The motion of matter is considered to be merely the transference of a strain distribution from one portion of the ether to another. This the author illustrated by causing a loop to travel along a rope, the loop being regarded as a strain distribution which is propagated along the rope, whilst the rope itself is not transferred. Such transference may occur without encountering any resistance, and the strain-figure will retain the same form, provided the velocity is small compared with that at which gravitation is propagated. The equations of motion of a strain-figure are deduced, and are shown to be identical with those of ordinary matter, provided certain conditions of symmetry are realized. It is also shown under what conditions an atom consisting of strain-figures would have a finite number of degrees of freedom, and some attempt is made to examine how gravitation and other attractions might follow from a distribution of stress in the strained ether. An inquiry is also made into the reason why elements have fixed properties, and their transmutation is discussed. Prof. Fitzgerald, referring to the elastic-solid theory of the ether, said Sir W. Thomson's more recent papers had thrown considerable doubt upon it. The propagation of strain-figures was, he thought, a case of wave motion. In his lectures he had likened the passage of matter through space to that of a drop of water through ice, the ice in front melting, and the rear

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of the drop freezing as the liquid state progressed. Many points raised in the paper were, he said, very interesting, and the suggestion that the discrete nature of atoms is the result of the coarse-grainedness of matter, very good. On the other hand, he considered the static treatment of strain-figures improper, for the ultimate conditions must be kinetic. Dr. O. Lodge agreed with Prof. Fitzgerald in regarding the motion of the loop along the rope as a wave motion whose velocity of propagation is equal to that of the loop. A similar case occurs when a ring of rope is spinning, and has a pulse impressed on it at one point, for the pulse travels at the same speed as the rope. —A paper on a permanent magnetic field, by Mr. W. Hibbert, was postponed until next meeting.

Royal Microscopical Society, November 18.—Dr. R. Braithwaite, President, in the chair.—A special meeting was first held, at which certain alterations in the by-laws were proposed by the Council, and accepted by the Fellows. The ordinary meeting was then constituted.—Mr. C. L. Curties exhibited and described a small heliostat made on the lines laid down by Mr. Comber. It was simple and effective, and was adapted for use in any latitude between 15° and 70°.—Mr. J. W. Gifford read a paper on the resolution of *Amphipleura pellucida*. Mr. Gifford said he had examined a frustule of *A. pellucida* with sodium light illumination, that being the most convenient form of monochromatic light at the time. Under this light *A. pellucida* unmistakably showed dots, which became more marked as the frustule was shifted to the side of the field of vision. The question then arose as to the possibility of photographing the object by the light of the sodium flame, and plates treated with an erythrosine bath were used. It occurred to him that a trough containing a solution of iron perchloride used as a screen would cut off all the blue end of the spectrum, and also some of the green, leaving only the green yellow, yellow and red; but on the other hand, he found the erythrosine plates were only sensitive as far down as the yellow, more especially to the green-yellow. In this way the part of the spectrum used for photographing could be reduced to a narrow band about midway between the D and E lines in the solar spectrum. By this means he succeeded in obtaining the photographs exhibited, and which he had avoided touching up. Whether these markings were true or spurious was a question he did not touch, but they appeared to have as good a claim as those on *Surirella gemma*. He thought it more probable that in both cases they were simply multiplied rings of the midrib and sides produced by the higher order of diffraction spectra according to the Abbe theory. The mounts of *A. pellucida* used were of realgar, or rather a higher sulphide of arsenic, prepared by mixing sulphur with realgar. He found it extremely difficult to make mounts with such a large proportion of realgar. The mounts being of a deep orange colour inclining to red, enabled the coloured screen to be dispensed with. But this highly refracting medium very soon cracks off the cover-glass. The advantage of working with this form of illumination and a suitable colour-correct plate is that ordinary achromatic object-glasses perform almost as well as apochromatic. He could not see the beaded markings with any glass of less aperture than 1·4; the best results being obtained with Powell and Lealand's apochromatic $\frac{1}{10}$ of 1·5, also with their achromatics $\frac{1}{16}$ of 1·5 and $\frac{1}{12}$ of 1·43. He also used in photographing a Zeiss projection ocular.—Mr. E. M. Nelson, speaking of the value of drawings made with Beale's neutral tint reflector, said he had tested the matter by making a drawing of lines on a micrometer scale of $\frac{1}{100}$ mm. under an apochromatic objective giving a magnifying power × 850; he found on measuring that they showed only a very slight displacement.—Mr. Curties exhibited a new form of microscope made on the Nelson model.—Mr. Nelson described some improvements in his apparatus for producing pure monochromatic light for use with the microscope.—Mr. A. W. Bennett gave a *résumé* of his paper on the fresh-water Algæ of South-west Surrey, in which he describes several new species.

CAMBRIDGE.

Philosophical Society, November 9.—Dr. Gaskell in the chair.—The following communications were made:—On a *Peripatus* from Natal, by Mr. A. Sedgwick.—On variations in the colour of cocoons (*Saturnia carpini* and *Eriogaster lanestris*), with reference to recent theories of protective coloration, by Mr. W. Bateson. The cocoons of several moths, *e.g.* the emperor and small egger, vary in colour from dark brown to white. It is believed by some that these colours have a protective value as a means of concealment, and it has been stated by Poulton and others that when spun on leaves which will turn brown, or in dark surroundings, the cocoons are dark, while they are white if spun on white paper. To account for this phenomenon "the existence of a complex nervous circle" has been assumed. The present experiments showed that it is true that larvæ left to spin on their food-leaves produce dark cocoons, and also that if they are taken out and put in white paper the cocoons are white. But it was found that larvæ similarly *taken out* and made to spin in dark substances also spun white cocoons, and indeed that starvation, parasites, or interference at the time of spinning, lead to the production of a white cocoon. On the contrary, if white paper is put amongst the food, so that the larvæ can, of their own choice, walk into it and spin, the cocoons are generally dark. It was noticed in several cases that larvæ which had been shut up evacuated a quantity of dark juice having the natural tint of the cocoon, and the suggestion was hazarded that absence of colour in the cocoon perhaps results from the loss or retention of this juice, which may be of the nature of meconium.—Exhibition of *Phylloxera vastatrix*, by Mr. A. E. Shipley.—On digestion in *Ammocœtes*, by Miss R. Alcock (communicated by Dr. Gaskell).—On the reaction of certain living cells to methylene-blue, by W. B. Hardy.—The chair was taken by Prof. Hughes during the reading of the last two papers.

PARIS.

Academy of Sciences, November 30.—M. Duchartre in the chair.—The reclaiming and cultivation of land in the Camargue, by M. Chambrelent. A description of the irrigation, cultivation, procuring of potable waters, and means of transpor, introduced in the Camargue. This district is contained between two branches of the Rhone, 50 and 58 kilometres long respectively, which reach the sea at points about 40 kilometres from each other. The bifurcation occurs at Arles, about 50 kilometres from the coast. At the beginning of this century the land was absolutely unfertile, and the inhabitants were unprovided with potable water. The author states the success that has attended the efforts that have been made to remedy this state of things, and bring the land under cultivation.—Observations of the asteroid discovered by M. Borrelly, at Marseilles Observatory, on November 27, 1891, by M. Borrelly. The positions are given for November 27 and 28.—*Résumé* of solar observations made at the Royal Observatory of the Roman College, during the third quarter of 1891, by M. P. Tacchini. (See Our Astronomical Column.)—On the tides of the Bay of St. Malo, by M. Heurtault. The author has made tidal observations at St. Malo for the last eighteen years, and states some of the results. The mean monthly level of the sea appears to have a minimum value in April and a maximum in October. The mean annual level increased gradually from 1874 to 1883, and has since been diminishing. The establishment of the port also indicates similar variations. Thus it passed from 6h. 55m. in 1874 to 6h. 10·24m. in 1884, and has since exhibited a tendency to diminish. Its months of maxima are April and September, and of minima July and December, the general mean being 6h. 8·9m. Contrary to Laplace's statement—"*Plus la mer s'élève lorsqu'elle est pleine, plus elle descende en basse mer suivante*"—it has been found from observations of 45 tides, that in only 25 did the lowest sea follow the highest tide; 3 times it immediately preceded, and 17 times it occurred two tides before. —Phosphides of boron, by M. A. Besson. The preparation and properties of boron phosphide, PB, are described; the possible existence of a more stable compound is also indicated.—On the bromine derivatives of methyl chloride, by M. A. Besson. The compounds CH_2BrCl and $CHBr_2Cl$ have been prepared. The author has not yet succeeded in isolating CBr_3Cl. (1) The compound CH_2BrCl is a colourless liquid distilling without decomposition at + 68°, solidifying at − 55°; its specific gravity is 1·90. (2) $CHBr_2Cl$ boils between 117° and 119°, and solidifies at − 32°.—On a modification of the calorimetric bomb of M. Berthelot, and upon the industrial determination of the calorific power of combustibles, by M. Pierre Mahler.—On the fixation of free nitrogen by plants, by MM. Th. Schlœsing, Jun., and Em. Laurent. The conclusions drawn from the data obtained are the following:—(1) There are some inferior green plants capable of fixing gaseous nitrogen. (2) Under the conditions of experiment peas take up much atmospheric nitrogen, whereas fallow soils, oats, mustard, cress, and spurrey are not capable of fixing a measurable quantity. The paper has remarks by M. Berthelot appended.—The ammonia in the atmosphere and in the rain of

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a tropical region, by MM. V. Marcano and A. Muntz. The observations were made at Caracas, in the Gulf of Venezuela, lat. 10°·3 N., altitude 922 metres. An examination of twenty samples of rain gave a mean proportion of ammonia of 1·58 milligram per litre, with a minimum of 0·37 and a maximum of 4·01. The proportion of gaseous ammonia present has been determined by exposing a known surface of acidulated water to the air and observing the ammonia absorbed in a certain time. Eleven determinations, extending over 174 days, have been been made, and they show that, on the average, an acid surface of 1 mq. absorbed, in twenty-four hours, 12·52 mgr. of ammonia, with a minimum of 5·30 mgr. and a maximum of 27 mgr. It appears, therefore, that the air of the tropical station is not so rich in gaseous ammonia as that of temperate regions.—Influence of the sun's rays on the bacilli of fermentation found on the surface of grapes, by M. V. Martinand.—On some effects of the parasitism of plants, by M. A. Magnin.

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THURSDAY, DECEMBER 10.

ROYAL SOCIETY, at 4.30.—On a Compensated Air-Thermometer: H. L. Callendar.—Note on the Necessity of using Well-annealed and Homogeneous Glass for the Mirrors of Telescopes: A. A. Common, F.R.S.—On some of the Properties of Water and of Steam: Prof. Ramsay, F.R.S., and Dr. Young.—On the Surya Siddhanta (Hindoo Astronomy): W. Brennand.—Repulsion and Rotation produced by Alternating Electric Currents: G. T. Walker.

MATHEMATICAL SOCIETY, at 8.—The Equations of Propagation of Disturbances in Gyrostatically-loaded Media: Dr. J. Larmor.—Theory of Elastic Wires: A. B. Basset, F.R.S.—Researches in the Calculus of Variations: II. Discrimination of Maxima and Minima Solutions when the Variables are connected by Algebraical Equations, the Limits being supposed Fixed: E. P. Culverwell.

INSTITUTION OF ELECTRICAL ENGINEERS, at 8.—Annual General Meeting.—Election of Council and Officers.—On the Specification of Insulated Conductors for Electric Lighting and other Purposes: W. H. Preece, F.R.S.

LONDON INSTITUTION, at 7.—An Hour with my Mozart Manuscripts (Illustrated): Prof. Bridge.

CAMERA CLUB, at 8.—A Short Description and Demonstration of New Telescopic Lens for Photography: T. R. Dallmeyer.—The Use of the Lantern for Scientific Illustration: Dr. A. H. Fison.

FRIDAY, DECEMBER 11.

ROYAL ASTRONOMICAL SOCIETY, at 8.

INSTITUTION OF CIVIL ENGINEERS, at 7.30.—Meters for Recording the Consumption of Electrical Energy: C. H. Wordingham.

CAMERA CLUB, at 8.—Retouching Class.

SATURDAY, DECEMBER 12.

ROYAL BOTANIC SOCIETY, at 3.45.

SUNDAY, DECEMBER 13.

SUNDAY LECTURE SOCIETY, at 4.—The Origin and History of the Thames (with Oxyhydrogen Lantern Illustrations): Prof. J. F. Blake.

MONDAY, DECEMBER 14.

SOCIETY OF ARTS, at 8.—The Pigments and Vehicles of the Old Masters: A. P. Laurie.

ARISTOTELIAN SOCIETY, at 8.—The True Sense of the Term *a priori*: J. H. Muirhead.

LONDON INSTITUTION, at 5.—Tropical Plants and Flowers (Illustrated): D. Morris.

CAMERA CLUB, at 8.30.—Development: Lyonel Clark.

TUESDAY, DECEMBER 15.

ROYAL STATISTICAL SOCIETY, at 7.45.—Enumeration and Classification of Paupers and State Pensions for the Aged: Charles Booth.

UNIVERSITY COLLEGE BIOLOGICAL SOCIETY, at 5.15.—The Sources of Nitrogenous Food of Leguminous Plants: H. Thompson.

INSTITUTION OF CIVIL ENGINEERS, at 8.—The Sale of Water by Meter in Berlin: Henry Gill. (Discussion.)

WEDNESDAY, DECEMBER 16.

SOCIETY OF ARTS, at 8.—Typological Museums, as Exemplified by the Pitt Rivers Museum at Oxford: General Pitt Rivers.

ROYAL METEOROLOGICAL SOCIETY, at 7.—Report on the Thunderstorms of 1888 and 1889: William Marriott.—On the Prevalence of Fog in London during the Twenty Years 1871–90: Frederick J. Brodie.

ROYAL MICROSCOPICAL SOCIETY, at 8.—On the Resolution of Podura: Hon. J. G. P. Vereker.

INSTITUTION OF CIVIL ENGINEERS, at 2.—Students' Visit to the Stations of the Westminster Electric Supply Corporation, 11 Millbank Street, and Eccleston Place, S.W.

CAMERA CLUB, at 8.—Retouching Class.

THURSDAY, DECEMBER 17.

ROYAL SOCIETY, at 4.30.

LINNEAN SOCIETY, at 8.—Development of the Head of the Imago of Chironomous: Prof. L. C. Miall and A. R. Hammond.—On Two Species of Cumacea in New Zealand: G. M. Thomson.

CHEMICAL SOCIETY, at 8.—The Composition of Cooked Vegetables: Miss K. Williams.—On some Metallic Hydrosulphides: S. E. Linder and H. Picton.—On the Physical Constitution of some Solutions of Insoluble Sulphides: Harold Picton.—Solution and Pseudo-solution: H. Picton and S. E. Lindor.—The Change proceeding in Acidified Solutions of Sodium Thiosulphate when the Products are retained within the System; and The Action of Sulphurous Acid on Flowers of Sulphur: Dr. A. Colefax.—The α and β modifications of Chlorobenzene Hexachloride: Dr. Matthews.—Camphrone, a Product of the Action of Dehydrating Agents on Camphor: Drs. Armstrong and Kipping.—Studies on the Dibromonaphthalenes: Dr. Armstrong and Mr. Rossiter.

LONDON INSTITUTION, at 6.—Winchester Cathedral (illustrated): Very Rev. the Dean of Winchester.

CAMERA CLUB, at 8.30.—Simplified Collographic Process (Description and Demonstration): Leon Warnerke.

FRIDAY, DECEMBER 18.

PHYSICAL SOCIETY, at 5.—On Interference with Alternating Currents: H. Kilgour.

CAMERA CLUB, at 8.—Retouching Class.

BOOKS, PAMPHLETS, AND SERIALS RECEIVED.

BOOKS.—The Universal Atlas, Part IX. (Cassell).—The Power which Propel and Guide the Planets: S. Laidlaw (Kegan Paul).—Progressive Mathematical Exercises, First Series: A. T. Richardson (Macmillan).—The Principles of Chemistry, 2 vols.: D. Mendeléeff; translated by G. Kamensky; edited by A. J. Greenaway (Longmans).—Lehrbuch der Vergleichenden Entwicklungsgeschichte der Wirbellosen Thiere, Specieller Theil, Zweites Heft: Dr. E. Korschelt and Dr. K. Heider (Jena, Fischer).—Proceedings of the American Association, August 1890 (Salem).—Oriental Cicadidæ, Part 4: W. L. Distant (London).—Annalen der k.k. Universitäts-Sternwarte in Wien, Band vii. (Williams and Norgate).—Travels in Africa during the Years 1879–1883: Dr. W. Junker; translated by A. H. Keane (Chapman and Hall).—An Essay on Reasoning: E. T. Dixon (Cambridge, Deighton).—Proceedings of the U.S. National Museum, vol. xiii., 1890 (Washington).—An Introduction to Chemical Theory: Dr. A. Scott (Black).—Himalayan Journals: Sir J. D. Hooker (Ward, Lock).—Falling in Love, &c.: Grant Allen; new edit. (Smith, Elder).—Nature and Man in America: N. S. Shaler (Smith, Elder).—Annuaire 1891, par le Bureau des Longitudes, Paris (Gauthier-Villars).—Connaissance des Temps ou des Mouvements Célestes, 1893 (Gauthier-Villars).—Ephémérides des Etoiles de Culmination Lunaire et de Longitude pour 1891: M. M. Lœwy (Gauthier-Villars).—The Harveian Oration on Harvey in Ancient and Modern Medicine: Dr. W. H. Dickinson (Longmans).—The Cause of an Ice Age: Sir R. S. Ball (Kegan Paul).—Scientific Results of the Second Yarkand Mission—Introductory Note and Map, 1878–91.—Scientific Results of the Second Yarkand Mission—Aves: Dr. R. B. Sharpe (Taylor and Francis).—Animal Sketches: C. Lloyd Morgan (Arnold).—A Manual of Physics: Dr. W. Peddie (Baillière).—Memory, its Logical Relations and Cultivation: Dr. F. W. Edridge-Green; 2nd edition (Baillière).

PAMPHLETS.—Connaissance des Temps, Extrait pour l'an 1892 (Paris, Gauthier-Villars).—Australian Museum, Sydney; Hand-List of Australian Mammals: J. D. Ogilby (Sydney).—Compass-Deviation; Syllabus of Examination in the Laws of Deviation (Imray).

SERIALS.—Records of the Australian Museum, vol. i., No. 9 (Sydney).—North American Fauna, No. 5 (Washington).—Proceedings of the Academy of Natural Sciences of Philadelphia, 1891, Part 2 (Philadelphia).—Brain, Parts 54 and 55 (Macmillan).

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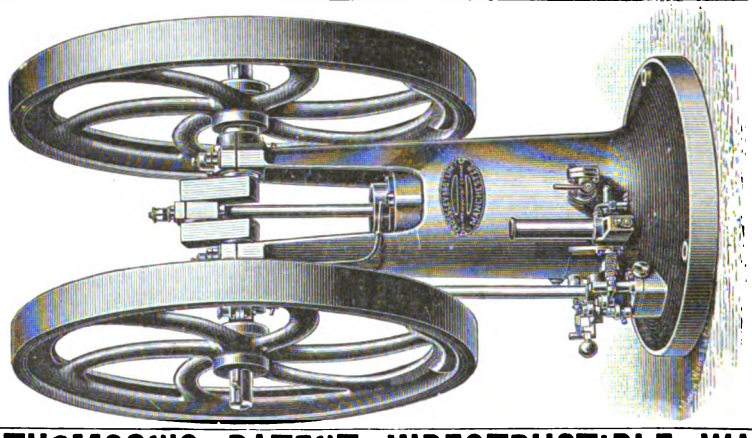

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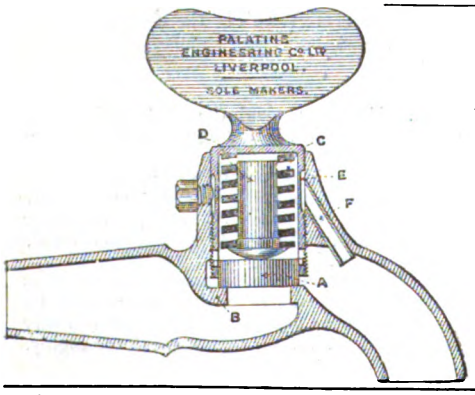

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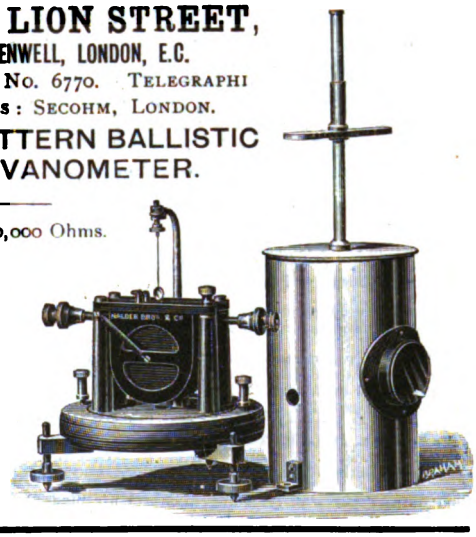

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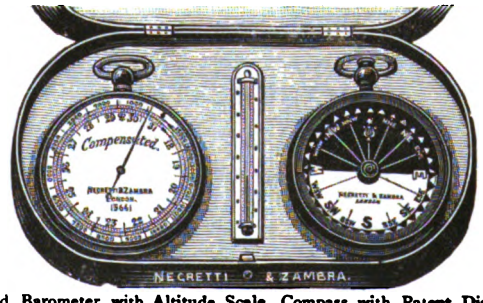

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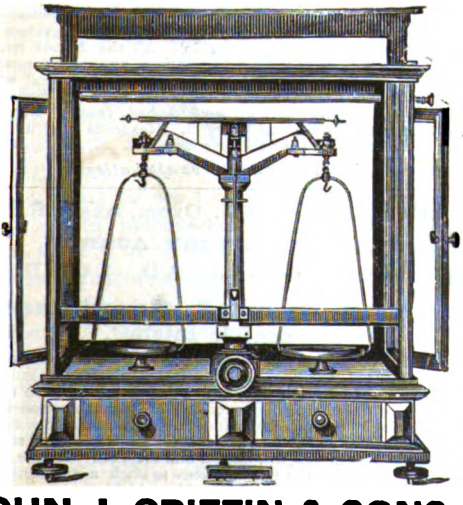

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THURSDAY, DECEMBER 17, 1891.

TWO ZOOLOGICAL TEXT-BOOKS.

Text-book of Comparative Anatomy. By Arnold Lang, Professor of Zoology in the University of Zürich. Part I. Translated into English. (London: Macmillan and Co.)

Lehrbuch der vergleichenden Entwickelungsgeschichte der Wirbellosen Thiere. Von Dr. E. Korschelt und Dr. K. Heider. Parts I. and II. (Jena: Fischer.)

THERE can be no doubt that the first-named of these two treatises, containing, as it does, a number of new engravings and process-cuts, and a recognition and exposition of some of the recent advances in zoological science relating to Invertebrata, will be useful to junior students in our Universities.

Whilst it will, no doubt, fulfil the author's purpose, and have a measure of success in its original German form, I must confess that it does not appear to me to be altogether satisfactory, whether we view it as an elementary sketch for junior students or as a work designed to assist the serious devotee of zoological research.

The most serious defect in the book from the latter point of view is that no attempt whatever is made to refer a given statement to the author who is responsible for it. A very short bibliography is appended to each chapter, but the reader of the book receives no assistance in tracing a new or striking observation to an original source. This curious reticence as to original authorities is carried to an extreme. For instance, on p. 137 there is a brief description (without figures) of Cœloplana and Ctenoplana; Korotneff is alluded to anonymously as "the discoverer of Ctenoplana"; neither his name nor that of Kowalewsky, the discoverer of Cœloplana, are mentioned; and the student has no means afforded him, either through the text or through the scanty bibliography, of finding out anything more as to these two remarkable forms. The same defect characterizes every page of the book; a very little trouble would have sufficed to remedy this, and thus not only to make the book useful to students but also to do justice and honour to those whose statements and observations Prof. Lang uses in order to build up his treatise.

The exact classification adopted by a writer nowadays, in so far as zoology is concerned, is to a large extent a matter of taste. One cannot expect to find oneself in agreement with a colleague on all matters of the kind. Prof. Lang is an authority on the Platodes or flat-worms, and I am glad to see that he assigns them a distinct position as an independent "phylum" of the animal kingdom; but I think that he has seriously injured the usefulness of his work by recognizing the "Vermes" as a phylum, including in it such diverse groups as the Nemertina, the Nemathelmia, the Annulata, the Prosopygia, the Rotatoria, and as an appendage (why an appendage when we have already such a mixed lot?) the Chætognatha. Prof. Lang, after doing this, proceeds to condemn his own action by stating that the so-called phylum of Vermes "is by no means a natural, well-demarcated division of the animal kingdom; now, as heretofore, it is like a lumber-room, to which all those groups are relegated which cannot be placed elsewhere." Prof. Lang was not compelled to maintain this ridiculous lumber-room. The consequence of doing so is that he is unable to treat any of the included classes fairly, and that some of the most important problems in morphology are tacitly assumed as solved, or are withheld from the student's consideration. Under the heading "Blood-vascular System," in the chapter on Vermes, we find brought together, and treated as though morphologically identical structures—(1) the canal system of the Nemertines; (2) the sub-cuticular network of the Acanthocephala; (3) the sinuses, vessels, and capillaries of the Leeches; (4) the closed vascular system of Chætopods; (5) the tentacular vessels of Sipunculids; (6) the red-corpuscled vascular system of Phoronis; and (7) the unexplored vascular systems of Brachiopods. Neither the method of juxtaposition adopted nor the space given permits the author to discuss whether any two of these so-called "blood-vascular systems" are homologous with one another, or whether any one of them is entitled to the name at all. It seems to me that, whilst senior students will be disappointed by the absence of any attempt to deal with the difficult problem of the real nature of the canals and their contained fluids in the cases of Nos. 1 and 3, the junior student will be seriously misled by the easy assumption that any system of canals in the so-called "Vermes" may be called "a blood-vascular system," and is homologous with any other, and with the standard blood-vascular system, viz. that of man. The entire chapter on the Vermes seems to me to be misleading, owing to the author's attempt to deal in a few pages with a number of very different but important groups under this unfortunate heading.

I need hardly say that I am sorry that Prof. Lang adheres to the old view (p. 540) that "the Arachnoidea are not nearly related to the Xiphosura and fossil Gigantostraca, but are racially connected with the Antennata, and are to be considered as Tracheata which have lost their antennæ." It is perhaps only natural that I should distrust the judgment of a zoologist who can at the present day maintain the above propositions. However much I may admire his original work on the Planarians, I cannot consider him a good general guide for the student of zoology. Though I regret Prof. Lang's opinion on this subject, I am not surprised at it, for, in the statement which follows his expression of opinion, he gives a one-sided and erroneous summary of the facts which he thinks can be adduced on either side of the question. Here, as so often elsewhere in the book, one notes the injustice done by Prof. Lang in not citing the name and the work of the author whom he is imperfectly quoting. The student who reads what Prof. Lang has to say on the relationship of the Arachnida to Limulus and the Eurypterines will be misled as to the mere facts of the case, and will not be helped by any reference towards obtaining a fuller knowledge of this interesting matter.

Though many of the illustrations are excellent, Prof. Lang's book cannot be recommended to English readers either for its originality or for its faithful exposition of cotemporary knowledge.

The translation appears to be fairly well executed, though the word "apparati," on p. 268, and the repeated

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use of "Disconanthe" and "Siphonanthe," in place of "Disconanthæ" and "Siphonanthæ," are astonishing, when we note that the translator is M.A. of the University which refuses to inquire whether "compulsory Greek" is useful or not.

A very different work from Prof. Lang's is the treatise on the embryology of the Invertebrata by Drs. Korschelt and Heider, of the University of Berlin, of which an English translation is in preparation. Two parts of this admirable book have appeared, containing 900 pages and more than 500 woodcuts, dealing with the embryology of the Porifera, Cnidaria, Ctenophora, Platyhelmia, Orthonectidæ, Dicyemidæ, Nemertina, Nemathelminthes, Acanthocephali, Rotatoria, Annelida (including Echiurids and Leeches herein), Sipunculidæ, Chætognatha, Enteropneusta, Echinoderma, and the Arthropoda. The various classes of the last-named phylum occupy two-thirds of the work at present issued. There remain still to be dealt with the Mollusca, the Bryozoa, and the Tunicata.

The best idea which can be given of the scope and value of this work is obtained when we compare it with Frank Balfour's treatise on comparative embryology. It is not too much to say that it is the most valuable text-book for the zoological student which has appeared since Balfour's book, and is a worthy successor to it. The mass of literature, vast as it was ten years ago, has increased enormously in the interval. Drs. Korschelt and Heider have carefully gone over it all; and not only that, but they accurately and clearly give each author's contribution to the subject in hand, citing authority for every statement made, so that the student can go to the original treatises for fuller detail. I do not know of any scientific treatise which shows so clearly the authors' desire to do justice to every fellow-worker of whatever nationality, and to produce a work which shall be a complete and trustworthy guide to the recent literature of a prodigiously prolific subject. Very often the authors abstain from offering a decided opinion upon a matter where the observations made have led previous writers to diverse conclusions: such cases are those in which the facts are incompletely observed and obviously require renewed investigation. But where the observed facts on one side or the other are of a decisive character, the authors, after giving both sides in detail, exercise a judicial function.

The book deals not only with the earlier but also with the later stages of development, and not merely with the facts of embryology, but with the conclusions as to the affinities of groups, which are so severely tested by the progress of embryological inquiry.

It is not possible to particularize in regard to such a work as this, but I have been struck by the very full way in which the morphology of the Crustacea, and each succeeding class of Arthropods, has been dealt with in every respect in which embryology throws light upon it. A very fully illustrated chapter gives a complete and impartial account of the researches of Sedgwick, Sheldon, and Kennell on the development of different species of Peripatus. The question as to the relationship of the Arachnida to Limulus and the Eurypterines, alluded to above, is fully treated by Drs. Korschelt and Heider. They summarize the arguments with clearness, and state and weigh fairly nearly every fact which has been adduced in favour of the association of Limulus and the Eurypterines with the Arachnida. Their conclusion is as widely different from Lang's as is their method of discussion.

There can be no doubt that we have in this new treatise on comparative embryology one of those invaluable, indispensable works for the production of which authors receive the gratitude and esteem of their fellow-workers in all lands. It is a truly first-rate book.

E. RAY LANKESTER.

MODERN ARTILLERY.

The Artillery of the Future and the New Powders. By James Atkinson Longridge, M.I.C.E. (London: E. and F. N. Spon, 1891.)

MR. LONGRIDGE keeps pegging away at his favourite subject, which he originally brought forward, now more than thirty years ago, in the Proceedings of the Institution of Civil Engineers.

His valuable ideas are at length receiving recognition, and his principle of strengthening guns, by layers of wire wound with appropriate initial tension, is now largely employed in the construction of modern ordnance.

The chief object of the present book is to point out that the fullest application of his principle of wire strengthening permits of the use of much higher powder pressures than are considered admissible in Government circles; and that thereby guns may be much shortened and reduced in weight, while at the same time the full power of the most modern high explosives can be utilized.

This is to a great extent a return on the steps which have guided our gun designers in recent years; their chief object being, to reduce initial pressure as much as possible, say down to 17 tons per square inch, and to get the full power out of the powder by a great increase in the length of bore, up to 40 and even 50 calibres in length in guns of large size.

Mr. Longridge is fully acquainted with all the valuable and original work in gunnery science which has been developed of late years in France by Sarrau, Veille, and Sebert; and analyzes carefully in chapters ii. and iii. the various experimental methods and empirical formulas in use for the measurement of powder pressure in the bore of a gun.

In chapter iii. he attempts a theoretical explanation and formula for maximum pressure and total energy, but his investigation rests upon an assumption of the *adiabatic law;* and the mathematical treatment is not presented in an inviting or elegant form. Considering the unexplained chemical combinations which affect the rate of combustion of the modern smokeless powders, this mathematical assumption can only roughly account for an average performance, and leaves unexplained the violent abnormal effects sometimes experienced and shown in General Wardell's curves, to guard against which the gunmaker has to exercise a vigilant caution.

General Brackenbury is quoted as calling gunpowder the Spirit of Artillery, and in his official capacity, Superintendent of the Royal Gunpowder Factory, as a maker of gunpowder, saying metaphorically, "How sad the body is so weak" in preference to "What a pity the spirit is so strong."

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But so long as steel is the strongest material of which the body—the gun—can be constructed, makers of powder and explosives, new and old, must be content to moderate and regulate the strength of their compounds. Much was expected of gun-cotton in its early days as a propulsive agent, but these hopes were falsified by the uncertainty of its action.

Again, with the vast extent of our Empire, climatic conditions have to be considered in their bearing on the action and preservation of explosives, conditions which do not affect French or German artillerists, who know the exact limits over which their warlike operations must take place. A. G. G.

GIANTS AND ACROMEGALY.

The Skeleton of the Irish Giant, Cornelius Magrath. By Dr. D. J. Cunningham, F.R.S. (Dublin: Published by the Royal Irish Academy, 1891).

NOTWITHSTANDING the close attention which has been applied to the clinical aspects of disease during so many centuries, every now and then some observer, more acute than his brethren, recognizes a morbid condition which had not previously been satisfactorily discriminated, and gives to it a name. Although in some cases the form of disease is thought to be new, and is described as such, it is generally found, when the records of medicine are examined, that corresponding cases and symptoms had been noticed previously, although their import had not been properly understood, and they had not been distinguished by a special name.

Amongst the latest contributions in this direction is a memoir published in 1886 by M. Pierre Marie, in which he described a morbid condition where the hands and feet were enlarged out of proportion to the rest of the body, chiefly due to a hypertrophy of the soft parts; and where the face had become remarkably elongated and deformed, partly from hypertrophy of the soft parts, but more especially from an increase in magnitude of the bones of the face, of the glabella and supraciliary ridges of the frontal bone, and of the pituitary fossa. To this condition M. Marie gave the name of Acromegaly. The attention of physicians and pathologists having thus been directed to the subject, several similar cases were described in the course of the next three or four years; and a few other cases and specimens previously recorded in medical literature were recognized as having been similarly affected. One of the most important of these was a skeleton in the Anatomical Museum of the University of Edinburgh, which was determined by Dr. H. Alexis Thomson to be a case of acromegaly, and the giant characters of the skeleton were associated with the peculiarly hypertrophied condition of the soft parts above referred to.

It was the perusal of Dr. Thomson's account of the Edinburgh skeleton which led Prof. Cunningham to pay especial attention to the characters displayed by the skeleton of the Irish giant Cornelius Magrath, which has been in the museum of Trinity College, Dublin, for 131 years, and to conclude that it also was an example of acromegaly. Dr. Cunningham's memoir, in addition to the anatomical description of the skeleton, contains much interesting information relative to Magrath himself, which he has collected from various quarters. Magrath was born in Tipperary in 1736, and died in Dublin in 1760, at the age of 23. He seems to have attained a height of 6 feet 8¾ inches when he was only sixteen years old; and for several years he travelled about as a show, and visited many of the great cities of Europe. The accounts which were given of his height in the periodical literature of the day, after he had reached his full dimensions, varied considerably; and a most exaggerated statement, that he was 8 feet 4 inches, made apparently with the view of out-rivalling the altitude of the skeleton of another Irish giant, Charles Byrne, in the Hunterian Museum, London, has found its way into anatomical literature. Dr. Cunningham has subjected all these statements to a careful analysis, and has studied and examined the skeleton itself, from which he concludes that the articulated skeleton is only 7 feet 2¼ inches high, and that this in all probability expresses the maximum height during life. Magrath is thus by no means the tallest giant whose height has been put on record. Charles Byrne was three or four inches taller; and Topinard, Ranke and Virchow have recorded examples of persons who ranged in height from 7 feet 3⅜ inches to 8 feet 4¼ inches. From an examination which Prof. Cunningham has made of the skeleton of Byrne in the Hunterian Museum, he has come to the conclusion that in certain particulars, *e.g.* the magnitude of the lower jaw, the dilated pituitary fossa, and the great size of the feet, it presents some of the characters of acromegaly. It must not, however, be supposed that giant growth is necessarily associated with the condition of acromegaly; for although it is not unusual to find the lower jaw disproportionately large in giants, yet it by no means follows that the other signs of acromegaly should be present.

Dr. Cunningham suggests that the morbid condition which M. Marie christened acromegaly should be known by the more etymologically correct term of "megalacria."

PEAKS AND PASSES IN NEW ZEALAND.

With Axe and Rope in the New Zealand Alps. By George Edward Mannering. With Illustrations. (London: Longmans, Green, and Co., 1891.)

TEN years ago the ice scenery of the New Zealand Alps was almost unknown even to the colonists. But in 1882 the Rev. W. S. Green, with two first-class Swiss guides, explored the glacier region beneath the highest peak—Aorangi, or Mount Cook—and arrived, after a long, difficult, and dangerous climb, on the summit of that mountain. His delightful volume "The High Alps of New Zealand," and the laborious explorations of Dr. von Lendenfeld in the following year, indicated that a region, certainly not inferior in grandeur and beauty to the Alps of Europe, could be reached in a journey of little more than two days from Christchurch. Since then the "Britain of the South" has become proud of possessing the "playground of Australasia"; the number of visitors has been rapidly increasing; an hotel has been built in a convenient situation near the foot of one of the glaciers; surveys have been undertaken; and the author of this volume, with one or two friends—inexperienced in mountain craft

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but inured to rough work in a wild country—commenced, in 1886, a series of expeditions in the "High Alps" of the Antipodes.

The Southern Alps proper, for over one hundred miles, form the backbone of the South Island, running roughly from north-east to south-west, the crest of the chain lying much nearer to the western than to the eastern coast. In the neighbourhood of Aorangi the former is about twenty miles away, while the distance of the other is quite five times as much. Thus the valleys fall more rapidly towards the west than towards the east, on which side also a wide tract of plain separates the sea from the foot of the hills. But in one respect, the New Zealand Alps, at any rate on their eastern flank, differ from their European namesakes, for they are pierced more deeply by the lowlands. Even at the foot of Aorangi, in the vicinity of which almost all the highest peaks are situated, the comparatively level floor of the Tasman valley is rather less than 2500 feet above the sea. Thus, although the New Zealand peaks are considerably lower than those of the European Alps—for few of them surpass 10,000 feet, and the highest summit of Aorangi is only 12,349 feet—they tower as high and as steep above their actual bases as the Oberland giants above the valley of the Lutschine. As Mr. Mannering says, Aorangi rises "for nearly 10,000 feet from the Hooker glacier, and Mount Sefton 8500 feet from the Mueller glacier, whilst the western precipices of Mount Tasman (11,475 feet) are stupendous." These words indicate another peculiarity of the New Zealand Alps. Here the snow-line lies very much lower than in Switzerland; in this central district it is only about 5000 feet above the sea. Thus the glaciers are actually greater, and descend much below those of Switzerland. The Tasman glacier, which may be compared with the Gross Aletsch of that country, is from 18 to 20 miles long, and terminates at a height of 2456 feet above the sea. On the western side the ice approaches occasionally to within 600 feet. Thus in the New Zealand Alps the Alpine climber meets with the same difficulties, and is rewarded by the same class of scenery, as he finds in the Old World amid peaks and passes three thousand feet higher. For instance, the Hochstetter Dome, first ascended by Dr. von Lendenfeld, though only 9315 feet above the sea, and presenting no special difficulties, is a very long and laborious excursion over ice and snow. Mr. Mannering and his two companions found it "twelve hours' hard going" from their camp—von Lendenfeld's party were out more than double the time.

But great as are these glaciers, they are, like those in our own hemisphere, attenuated representatives of their predecessors—for New Zealand also has had its Ice age. Once they extended far away into the lowlands. There, erratics and ice-worn rocks abound for miles. Lakes Tekapo and Pukaki are dammed by moraines, and in the valley of the Tasman River some singular terraces can be traced for 40 miles from the point where they commence, near the end of the glacier, at a height of some 2000 feet above the valley. Much *débris* is still transported by the glaciers of the eastern side of the chain, but those on the western are cleaner. The main range consists of stratified rocks, which disintegrate, as the climber soon discovers to his sorrow, rather readily; and Mr. Mannering attributes the difference in the amount of *débris* to the fact that the beds have a westerly dip. This, at any rate, accounts for the precipitous character of the eastern face of the mountains. We infer, also, that denudation has been more rapid on this side, for Aorangi projects in advance of the watershed, like the Viso and other peaks on the Italian side of the Alps.

The ascent of Aorangi is evidently a long, difficult, and even dangerous excursion. The route is quite as circuitous as that formerly followed in the ascent of the Bernina; for the great ice-fall of the Hochstetter glacier, 4000 feet high, has to be turned, and another glacier basin crossed, before beginning the steep ascent of the actual peak. Mr. Green's party was forced to turn back without actually touching the culminating point, though they had overcome all difficulties, for the short half-hour which the completion of the excursion would have required might have made return impossible. As it was, they were benighted among the glaciers. Mr. Mannering and his friend, after five attempts, reached a point about 100 feet below where Mr. Green halted, was compelled to turn back for the same reason, and had a dangerous descent over the snow-fields in the darkness. But as an Alpine Club was founded a few months since in New Zealand, there will probably not be many peaks to climb or passes to discover when another decade has passed.

Mountaineering in New Zealand is not for climbers fond of luxury. Guides and porters are at present unknown. Mr. Mannering and his companions had to carry their own "swag"—and heavy loads these were—cut their own steps, do everything and discover everything for themselves, for they were self-taught mountaineers. Sometimes he had only one companion, and then the labour and the danger were alike increased. The weather also seems to be more unsettled than it is in the Alps. The rainfall is heavy—150 inches in some places. Thus fresh snow often adds to the difficulties and the dangers of excursions, and falls of stones seem common, as might be expected. Mr. Mannering tells most pleasantly and unaffectedly a story of pluck, endurance, and skill, of which our kinsmen in New Zealand may be justly proud. He is a careful observer of Nature, and a true lover of mountain scenery, as well as a daring climber. His book contains a number of illustrations, taken from photographs, which show that the peak and glacier scenery of the Aorangi group is worthy of the author's enthusiastic praise; and it is not only very pleasant reading, but also adds much to our knowledge of the region. It may be added that, if the traveller is not satisfied with the perils of rocks and snow, those of flooded rivers are a common experience; and Mr. Mannering, in his final chapter, describes the doubtful pleasures of a canoe voyage down the Waitaki River.

T. G. BONNEY.

OUR BOOK SHELF.

Manual of the Science of Religion. By P. D. Chantpie de la Saussaye. Translated by Beatrice S. Colyer Fergusson. (London: Longmans, 1891.)

THOUGH the title of this volume seems to imply that it is a complete thing, it is really only the first half of Prof. de la Saussaye's book. Nearly three-fourths of the Amsterdam Professor's manual is devoted to a sketch of the chief ethnic religions and of Islam; whereas the English

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book, corresponding as it does to the first of the two German volumes, omits the ancient religions of Persia, Greece, Rome, and Germany, as well as Mohammedanism. Without these we have nothing that can fairly be called by the name borne on the title of the translation; and the omission of Persia in particular makes the book most tantalizingly imperfect as regards a single connected group of faiths. It is not fair either to the author or to his readers to give a part of the work as if it were a whole, and to set it forth to the world without even putting "vol. i." on the title-page.

The work of translation has been performed with care, and generally, so far as we have been able to test it, with accuracy. But the book is rather heavy reading, as translations from the German are apt to be, when the translator has not realized that the difference between the idioms of England and Germany is so great that a successful version must recast whole sentences instead of aiming at a literal reproduction of words and clauses.

As regards the substance of the book, the reception which it has obtained, in its German form, is a sufficient proof that Prof. de la Saussaye has met a felt want in the literature of his subject. A book that covers so wide a field cannot be without errors in detail. No man can know at first hand all the ancient literatures that are dealt with; nor can it be supposed that, on matters where specialists are often at variance, one who is not a specialist can always hit the mark. The general principles of the science of religion are not yet worked out with sufficient clearness to give the student of religions in general sure points of view for the criticism of the divergent results that have been reached by students of special religious literatures. Perhaps to say this is to say by implication that a general manual of the subject is an undertaking for which the time is not yet ripe; and certainly the science of religion (as distinct from the scientific study of individual religions in their historical development) is still in a very elementary stage. But Prof. de la Saussaye is no dogmatist; he frankly admits the obscurity in which many fundamental problems are still involved, and he writes throughout with great impartiality and moderation, as well as with extensive knowledge of recent researches. His book will be very useful to all who wish to know the present state of inquiry, and do not forget that many things in his exposition are to be taken as still doubtful, even where the author himself does not expressly accompany them with notes of uncertainty. W. R. S.

Euclid's Elements of Geometry. Book XI. By A. E. Layng, M.A. (London: Blackie and Son, Limited, 1891.)

OF all Euclid's books, the eleventh is one that forms a stumbling-block to the beginner in solid geometry. Not that the proofs in themselves are of a difficult nature, but simply that the figures have to be drawn perspectively to illustrate the various planes, and the student finds it hard to bring himself to believe the equality of angles and lines which appear to him to be unequal.

The author of this book, like one or two others before him, by varying the thickness of the lines used in construction, simplifies matters very considerably, for by this means the eye can distinguish directly the different planes. Of the propositions themselves, little need be said, unless we mention the use throughout of all the well-known symbols: the occasional interpolated worked-out examples, and the notes and exercises, although not in any great quantity, will be found very useful. In the collection of miscellaneous examples, theorems relating to tetrahedrons, pyramids, spheres, &c., are included. Preceding the series of examination-papers, which are here arranged in a progressive order of difficulty, and taken from papers set lately, are two appendices, the first dealing with transversals, harmonic section, and pole and polars, the second with a few alternate proofs of propositions. The manner in which most of the proofs are worked out is both neat and brief, and the definitions are all clearly stated and illustrated. We may add that, although the work is not the best of its kind we have seen, yet it has many good points which recommend it to the student of geometry. W.

Illustrations of the Flora of Japan, to serve as an Atlas to the Nippon-Shokubutsushi. By Tomitarō Makino. (Tōkyō, Japan: Keigyōsha 1, Urazimbōchō, 1891.)

THIS is a monthly publication, containing excellent uncoloured figures of plants, with analyses of their floral structure, fruit, and seed, and descriptions in English as well as in Japanese. The drawing and lithography, by Mr. Makino himself, are quite equal to the average work in this country—indeed, one might say above the average; the lithography being light and effective, with few lines in it. Moreover, the English descriptions are intelligible, correct, and idiomatic, and not too long, nor superfluous. Botanically and horticulturally this production of the Far East will be welcome, and even indispensable, in the West, as many new species are described. Already nine parts have appeared, with illustrations of fifty-seven species belonging to various natural orders. No system of classification is followed; whatever is of interest or novel being taken as it presents itself. W. B. H.

About Ceylon and Borneo. By Walter J. Clutterbuck, F.R.G.S. (London: Longmans, Green, and Co., 1891.)

IN this volume Mr. Clutterbuck gives some account of Ceylon as he saw it during a recent visit, and as it was fourteen years ago, when he resided for a short time in the island. He then describes what he saw in the course of a visit to Brunei and British North Borneo. Readers who like books of travel will find a good deal to interest them in the author's impressions, which are recorded in a lively style.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Wind Direction.

A SHORT time ago there was some correspondence in your columns on the relations of north-east and south-west winds in recent years; Mr. Prince, of Crowborough, having observed at that place that while, as a rule, the south-west winds were in excess of the north-east, the reverse had occurred in each of the five years 1885 to 1889. The Greenwich records were examined by Mr. Ellis with regard to this point, but he found them at variance with those of Crowborough, the south-west winds having continued in excess of the north-east throughout those five years.

By combining several directions, the Greenwich figures, as tabulated by Mr. Ellis in his paper to the Royal Meteorological Society (Quart. Journ., October 1890, p. 222), will be found to reveal some curious relations, which seem to invite attention. I have added together the figures (numbers of days) for north-east, east, and south-east winds on the one hand, and those for north-west, west, and south-west on the other; then smoothed each set of sums by means of five-year averages. The results are shown in the two curves of the accompanying diagram. The continuous curve (a) represents north-east, east, and south-east winds (and its vertical scale is at the left). The dotted line curve (b) represents north-west, west, and south-west winds (and

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its vertical scale is at the right). It will be understood that each year-point of those curves represents an average of five years.

It would appear that easterly winds (north-east, east, and south-east) at Greenwich have been increasing in prevalence, on the whole, since about 1865; the five years' average for that year is 83·8, and this grows to 111·4 in 1886 (about one-third). On the other hand, westerly winds (north-west, west, and south-west) have diminished, on the whole, since 1861; the five years' average for that year is 210·8, and this diminishes to 159·0 in 1887 (about one-fourth).

Judging by the past, we might perhaps consider that we must be near a decided turn of the curves; possibly past it in the case of easterly winds; in which case we should look for more westerly and less easterly wind in the near future.

(a) 1865 *min.*

1863	1864	1865	1866	1867	Average.
77 ...	104 ...	76 ...	72 ...	90	83·8

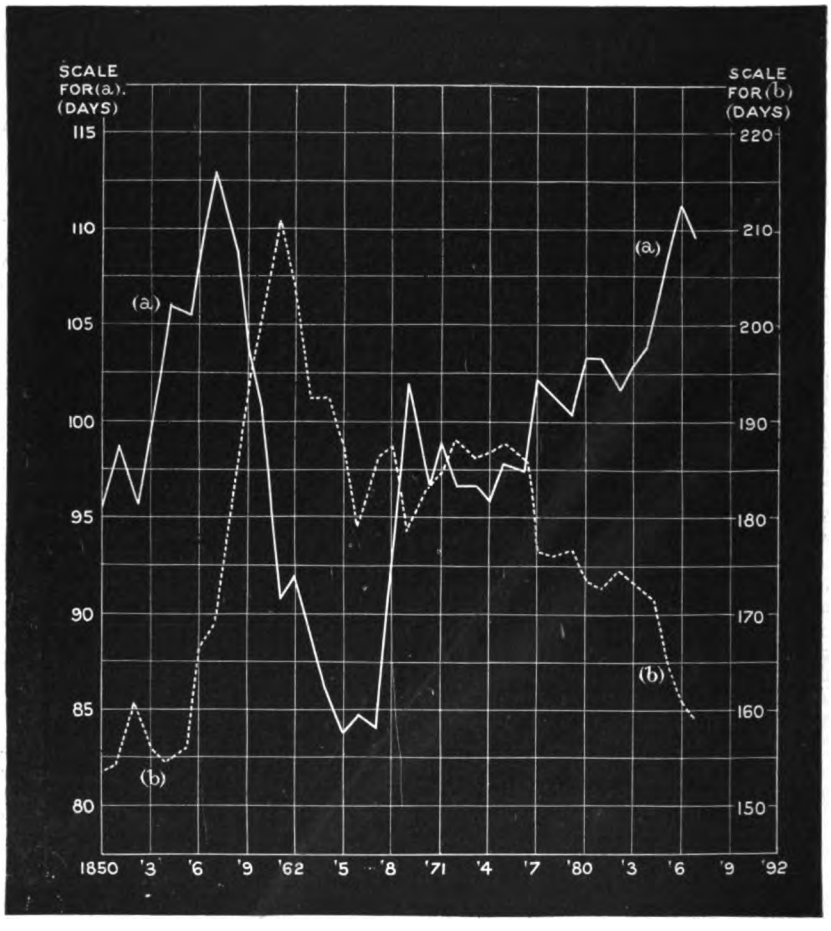
(b) 1861 *max.*

1859	1860	1861	1862	1863	Average.
199 ...	215 ...	203 ...	211 ...	226	210·8

(b) 1887 *low point.*

1885	1886	1887	1888	1889	Average.
157 ...	158 ...	155 ...	164 ...	161	159·0

A. B. M



(a) = North-east, east, and south-east wind (Greenwich). (b) = North-west, west, and south-west wind (Greenwich). Actual figures smoothed by means of five-year averages.

From the "crest" of (a) in 1857 to the last point is thirty years. Are those long variations possibly a manifestation of the thirty-five years' period of Brueckner?

These curves might have begun at an earlier date, for the figures commence in 1841. But in the earlier years, and apparently to about 1855, a larger proportion of days seem to have been reckoned as "calms" than afterwards (owing to a difference of rule), so that the curves to about 1855 are not exactly comparable with the remaining portions.

I append a few figures, showing the derivation of maximum and minimum (or low point) averages:—

(a) 1857 *max.*

1855	1856	1857	1858	1859	Average.
114 ...	111 ...	113 ...	126 ...	99	112·6

The Migration of the Lemming.

My attention having recently been drawn to the question of the migration of that little Norwegian rodent the lemming, as a serious obstacle to the theory of natural selection, and hence to evolution generally, I write to call attention to what appears to me to be a possible factor in the *starting* of the migration.

It is that when the lemmings become too numerous for the means of subsistence upon the inland plateaux, which may be described as their home, the "fittest" lemmings, by battle, turn out from the district all those of their weaker brethren who are unable to withstand the contest.

The unfittest being thus driven out from their home, are forced to migrate somewhere. Why they move incessantly to the westward seems a problem yet to be satisfactorily solved.

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My reason for suggesting such an origin for the migration is that it takes place every three or four years from the same plateaux. It is very evident, therefore, that—abiogenesis being now out of court—some lemmings must be left there to continue the species. Now, it is not likely that the weakest are left behind, otherwise their survival year after year would be quite problematical. Do not the facts point unquestionably to the strongest being left to continue the race?

To the criticism that there is no evidence of fighting having taken place amongst the migrants, my reply would be that no one, so far as I can learn, has seen a migration start, or seen one immediately after it has started—the only time, that is, when the effects of such fighting would be apparent, for after a few days those seriously injured would have died and have been left behind, while those only slightly injured would have recovered sufficiently to be indistinguishable from the remainder.

Churchfield, Edgbaston. F. HOWARD COLLINS.

The New Railway from Upminster to Romford, Essex.

ON the above railway, now being constructed, there is a section of unusual interest a few yards north-east of the church at Hornchurch, showing the Chalky Boulder Clay (15 feet seen) under sand and gravel belonging to the highest terrace of the Thames Valley, and resting on London Clay. Hitherto, Boulder Clay has not been seen in this district in connection with Thames Valley deposits, its most southerly exposures lying about three miles northward, on London Clay or Bagshot Beds.

I have been carefully watching this cutting for some time, with a view of sending an account of it to the Geological Society when it shall have been completely excavated. But, though much has still to be done, the half of it finished has already been sloped, and the arrangement of the beds—clear a month ago—greatly obscured. It has therefore been suggested to me that a few lines on the subject in NATURE may be the means of enabling geologists interested to visit this section while the Boulder Clay is still clearly visible in some portion of the cutting.

It may be useful to add that the distance from Hornchurch Station is about a mile, and that the visitor, after leaving the church on his right hand, should take the first road on his left.

T. V. HOLMES.

28 Croom's Hill, Greenwich Park, S.E.,
December 7.

Peculiar Eyes.

THE inability of keeping one eye shut and the other open at the same time, is a fact well known to drill-sergeants. I well remember, when a conscript some sixteen years ago, how a great number of recruits were unable, even after repeated efforts, to do so; but I had no difficulty about it. At that time, too, my eyes were about equal in power; but at present while the right eye is of normal power, the left eye presents a much less distinct image. I can only ascribe this to the habit of working at the microscope with the right eye without closing the left. It is especially at this work that the defective sight in the latter is noticeable. I do not think my ten months of rifle practice has anything to do with it, except, perhaps, in emphasizing the tendency to use the right eye, the image of which is now so predominant, that in covering a bull's eye, for instance, it is immaterial whether the left eye be closed or not.

G. K. GUDE.

5 Giesbach Road, Upper Holloway, December 7.

Grafted Plants.

REFERRING to Prof. Henslow's paper on "A Theory of Heredity based on Forces" (November 26, p. 93), the behaviour of grafted plants seems to require, for its explanation, the possession by both stock and graft of something analogous to a distinct individuality, call it what we may. It is difficult enough to understand, especially in the case of nearly-related forms, why the stock generally has no, or so little, influence on the graft; but, assuming the absence of individuality, the difficulty is largely increased. The graft takes its nourishment through the stock, and yet retains its characteristics unimpaired. I argue from this that not only does the graft possess an individuality of its own, but that this is so marked that it can take its nourishment direct from the stock, while at the same time straining out, as it were, whatever it is that constitutes the individuality of the stock. The phenomena presented by parasitic plants seem to bear out this view.

W. H. BEEBY.

Intelligence in Birds.

A FEW weeks ago I received a specimen of *Podoces panaeri*, the typical desert bird of Central Asia, which had been kept for some months in captivity at Perowsk. The first thing the creature began to do, when located by me in a spacious *volière*, was to pick some food (cooked rice with baked eggs), and to bury it in the very thick sand layer with which the floor of the cage was provided. This was the incessant occupation of the bird on the first day of its instalment. But the task was almost completely abandoned from the next day; the bird, evidently remembering the conditions of its former life in captivity, found it useless to make provision for the future when a fresh supply of food daily appeared.

The fact referred to seems to indicate, first, that the birds in question are in the habit of making provision in the wild state, the powerful and slightly curved bill being admirably adapted for the purpose of making holes, even in a hard ground. It shows, also, how abruptly the habits of animals can be modified when the conditions of their environment are changed.

Now, a question naturally arises, How must we regard this habit of burying food—as the result of a long inheritance, or as an effect of constant imitation of older birds by younger ones?

A. WILKINS.

Tashkend, Central Asia, November 8/20.

SIR ANDREW CROMBIE RAMSAY.

THOUGH this illustrious geologist has been laid aside by growing infirmity for the last ten years, the news of his death will carry regret into the hearts of many men of science, not in this country only but all over the world. Born in Glasgow, and intended for a mercantile profession there, he spent some few years in business; but, partly on account of delicate health, betook himself for rest and open-air exercise to the island of Arran. One of the friends of his early years, Prof. Nichol, of Glasgow University, the well-known writer on astronomical subjects, had much influence in directing his studies into a scientific channel, so that the marvellous geological lessons to be learnt from the rocks of Arran soon arrested Ramsay's attention. Throwing himself with all the ardour of an enthusiastic nature into the pursuit which he now took up, he was led to climb the mountains and traverse the glens throughout the length and breadth of Arran. In this way, face to face with the facts of Nature, and amid some of the most charming scenery of his native country, he taught himself the rudiments of geology, and acquired that clearness of insight for geological structure, that love of mountain-forms, and that freshness and originality of interpretation, which marked him out from his associates in later years. But above all, by actually mapping the grouping of the rocks, he gained that precision in field-work which was to bear such notable fruit in his connection with the Geological Survey. He constructed a geological model of Arran on the scale of two inches to a mile, and made copious notes of the geological structure of all parts of the island.

The meeting of the British Association in Glasgow in the year 1840 proved to be the turning-point in his career. The model and map of Arran which he had made were exhibited at the Geological Section, and he gave a brief account of them and of the geology of the island. Among the geologists who listened to him was Murchison, who, struck with his ability and his devotion to the science, offered to take him on an expedition which the author of the "Silurian System" had then projected to America. Ramsay accordingly went up to London, but found that the voyage across the Atlantic had been abandoned for a journey into Russia, and that he was not to take part in it. Murchison, however, spoke so warmly in favour of his

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young friend to Sir Henry De la Beche, the Director-General of the Geological Survey, that a post was at once found for him on the staff of the Survey, and before many days Ramsay was at work at Tenby. He joined the service in the spring of 1841, immediately after the publication of the little volume on Arran, which embodied the fruits of his labours in previous years. From that time onward his life was spent continuously in the work of the Survey until he retired at the end of 1881. So capable a lieutenant did he prove himself to the chief of the staff, that after only four years he was appointed Local Director for Great Britain.

From the first Ramsay showed that, with habits of patient observation and cautious induction, he combined a faculty for bold and broad generalization. His remarkable paper on the denudation of South Wales, published in 1846, was one of the earliest essays in which the amount and effects of denudation were worked out from detailed surveys of the geological structure of the ground. He then struck the key-note which may be heard through nearly all his subsequent contributions to scientific literature. He was one of the earliest observers to realize that the existing topography of the land has a long and interesting history, much of which may still be deciphered by the use of geological investigation.

The name of A. C. Ramsay will ever be honourably associated with the story of the gradual working out of the records of the Ice Age. Following up the results obtained by Agassiz, Buckland, Darwin, and others in this country, he threw himself with all his ardour into the study of the glaciation of Wales, tracing the limits of the glaciers of that region, and extending his expeperience by frequent excursions among the Swiss Alps. His scattered papers in scientific journals undoubtedly did much to stimulate general interest in the history of the Glacial Period, and to create a special and voluminous literature of this subject. His views differed much from those of some of the older geologists of the day, and led to some active controversy. Especially did opposition arise when, after studying long and carefully the erosive action of land-ice, he came to the conclusion that certain lake-basins in various parts of the world had been scooped out by ice. Murchison, Lyell, and others of less fame, entered the lists against him; but he had a considerable following among the younger geologists. And this controversy still fitfully continues.

In connection with his glacial work, mention should be made of his bold endeavour to prove that ice-action had been in operation more than once in the geological past. His paper on the Permian breccias of England called attention to the evidence of transport of fragments of rock from Wales, and to the resemblance between these fragments and those in glacial moraines and boulder-clay. He subsequently detected what he thought to be similar traces of ice-carried materials in the Old Red Sandstone; and in one of the last papers which he wrote he gathered together the various pieces of evidence in favour of a long succession of Glacial periods in the geological past.

Two of the most suggestive essays he ever wrote were his well-known Presidential Addresses to the Geological Society in 1863 and 1864, in which he worked out, from his wide practical acquaintance with the stratified formations of Britain, the idea of breaks in the succession of organic remains in the geological record. To the geologist and the palæontologist these papers marked a distinct epoch in the advance of geological inquiry; while to the biologist concerned with the history of the evolution of organized existence on this planet they were full of luminous thought.

By far the largest part of Sir Andrew's contributions to geological literature is to be found in the maps, sections, and memoirs of the Geological Survey. The mapping of the volcanic districts of North Wales, in which he took the leading part, will ever remain the best monument of his skill as a field-geologist. His exhaustive memoir on that region has long since taken its place as one of the standard works of reference in our geological literature.

In his later years he seems to have taken pleasure in reverting to some of the inquiries which he started in an early part of his career. He returned with renewed zest to the study of the history of topographical features, discoursed as to how Anglesey became an island, and traced out the story of the River Dee. In successive editions of the work on the "Physical Geography and Geology of Great Britain," which at first was given as six lectures to an audience of working men, he worked out in greater fulness the chief stages through which the surface of this country seemed to him to have passed before it acquired its present features.

Of the value of his scientific labours full recognition was made by his contemporaries. He was elected President of the Geological Society in 1862, President of the Geological Section of the British Association in 1866 and again in 1881, and President of the Association itself in 1880, when the meeting was held at Swansea. He received the Wollaston Medal of the Geological Society, the Neill Medal of the Royal Society of Edinburgh, and a Royal Medal from the Royal Society. He was chosen into the honorary list of many learned Societies at home and abroad. On the death of Sir Roderick Murchison in 1871, he was appointed Director-General of the Geological Survey. At the end of 1881 he resigned this office, was knighted for his distinguished services, and soon thereafter went to reside at Beaumaris, where his strength has gradually given way, until he died on the evening of the 9th inst.

There was in Sir Andrew Ramsay such simplicity and frankness that men of the most diverse natures were attracted to him, and as they came to know him more intimately the gaiety and kind-heartedness of his disposition attached them to him in the closest friendship. Fond of literature, and glad to relieve the pressure of his scientific work by excursions into the literary field, he had acquired a range of knowledge and of taste which gave a special interest to his conversation. Now and then he found time to write an article for the *Saturday Review* in which this literary side of his nature would find scope for its exercise. But the daily grind of the official treadmill left him all too little time for such diversions. His death removes from our midst one of the foremost geologists of our day, and from the friends who knew him in his prime, a large-hearted, lovable man, whose memory they will cherish till they too pass away. A. G.

ON VAN DER WAALS'S ISOTHERMAL EQUATION.[1]

ONE of the objections raised against this equation by Prof. Tait in NATURE, vol. xliv. pp. 546 and 627, brings clearly to light the importance of the question whether the finite size of the particles should be accounted for by an equation of the form—

$$p_1(v - b) = \tfrac{1}{3}\Sigma mu^2, \quad \ldots\ldots\ldots (1)$$

where p_1 represents the internal pressure, equal to the sum of the external pression p, and the molecular pressure a/v^2, and b some multiple of the total volume b_1 of the particles; or if this equation must rather have the form—

$$p_1 v = \tfrac{1}{3}\Sigma mu^2\left(1 + \frac{b}{v}\right). \quad \ldots\ldots (2)$$

[1] Prof. Korteweg's paper and accompanying letter are of date November 4, but owing to an accidental delay they did not reach me until after the appearance of my last communication (NATURE, November 26, p. 80). Otherwise I should, of course, have made reference to them. It will be seen that Prof. Korteweg draws attention to the form of the virial equation applicable in one dimension. RAYLEIGH.
December 2.

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In the *first* case, in substituting $p + a/v^2$ for p_1, and $R(1 + at)$ for $\frac{1}{2}\Sigma mu^2$, the well-known formula of Prof. Van der Waals is arrived at. In the *second* case, the same substitution leads to a quite worthless formula, unfit to explain even qualitatively the conduct of gases under compression.

The *first* form is the one which presents itself most naturally when, as was done by Van der Waals, the extension of the molecules is considered as a diminution of the volume in which they are moving; the *second* is obtained as a first approximation, when the virial equation is extended to the repulsive forces which come into play at the collisions. Of course, both methods, if they could be worked out with absolute rigour, would give the *same* result; but, this being impossible for both of them, the question as to which gives the better approximation is not at all an unreal one.

Now, it is extremely improbable that this question should have to be answered in a different way for linear and for three-dimensional space; yet for linear space the first method leads to a quite easy and *absolutely* rigorous solution, and the equation thus obtained is analogous to the *first* form.

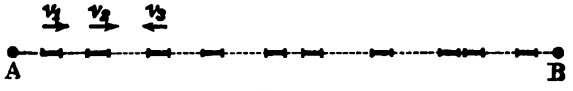

FIG. 1.

In order to prove this, let A B (Fig. 1) represent a linear space of length l, bounded by two rigid walls, A and B, and let there be moving in this space some perfectly elastic particles, all of the same mass, m, and length, λ, but having different velocities, $v_1, v_2, \ldots v_n$. At every encounter of these particles there will be simply an exchange between their velocities; therefore at every moment one of the particles will have the velocity v_1. On this particle we fix our attention, following it on its way till the next collision. After this collision we leave it to its fate, directing our attention to the other particle, which has now acquired the velocity v_1. Proceeding in this manner, it is obvious that at every collision a distance λ is economized, which has not to be travelled over by the centres of the molecules. Starting, then, from the wall A and passing over to the wall B and back again, the number of collisions is $2(n-1)$, and the distance economized $2n\lambda$, (adding 2λ for the collisions against the walls). The distance travelled over by the centres consequently being $2l - 2n\lambda$, it is clear that the number of collisions with velocity v_1 against one of the walls amounts to $\frac{v_1}{2(L - n\lambda)}$ in one unit of time, and the corresponding change of momentum to $\frac{mv_1^2}{L - n\lambda}$, so that the pressure on the wall is measured by

$$p = \frac{\Sigma mv^2}{L - n\lambda} \quad \ldots\ldots\ldots\ldots (3)$$

Of course, for space of two and three dimensions, the problem is much more complicated. Yet in 1877 I gave a solution[1] of it for spherical particles which, according to my opinion, is rigorous so far as the several encounters between the molecules may be looked at as independent of one another. For a short time after each collision the possibilities of fresh collisions are considerably influenced by the proximity of the departing molecule. This influence, certainly of very difficult mathematical treatment, is disregarded in my calculations.

The outcome of these calculations[2] is that of every unit of distance only the $(1 - 4b_1/v)$ th part has to be travelled over by the centres of the molecules, the $4b_1/v$ th part being economized at the collisions. Therefore the number of the encounters with the bounding walls is augmented in the proportion $1 : 1 - 4b_1/v$, and the formula $pv = \frac{1}{2}\Sigma mu^2$ is to be changed to

$$p(v - 4b_1) = \tfrac{1}{2}\Sigma mu^2. \quad \ldots\ldots\ldots (4)$$

In 1881, my friend H. A. Lorentz applied the virial equation to the calculation of the influence of the size of the particles on the pressure. In this manner he obtained the formula—

$$pv = \tfrac{1}{2}\Sigma mu^2 \cdot \left(1 + \frac{4b_1}{v}\right). \quad \ldots\ldots\ldots (5)$$

His paper, published in *Wiedemann's Annalen*, Bd. xii. p. 127, was inserted in the German and English versions of Van der Waals's pamphlet "On the Continuity of the Liquid and Gaseous States of Matter," at the end of the sixth chapter.

Considered as a determination of the factor, with which the total volume b_1 of the particles, when introduced in $v - b$, is to be multiplied, our results were identical, and confirmed the opinion expressed by Van der Waals about the value of this coefficient. Mr. Lorentz viewed his results in no other light, and had no intention at all to substitute his formula (5) for that given by Van der Waals. Indeed, in the passage of his paper which I quote here, he clearly indicates the weak point of his calculation:—"Strictly speaking, a correction ought to be made here, indicated by Mr. Van der Waals; in calculating the number of encounters, the extension[1] of the molecules should have been taken into account. The matter is simplified, however, if the influence of the virial arising from the repulsive forces, or the size of the molecules, is small; and if a correction to the first order is sufficient, then the uncorrected value of the number of encounters may be used in calculating the small repulsive virial."

Now it is not impossible to apply to Lorentz's formula the correction alluded to in this passage. In 1875 I calculated for the first time,[2] by a more rigorous method, the shortening of the mean free path of spherical particles, in consequence of their extension in the direction of motion. Some months later, Mr. Van der Vaals succeeded in the same calculations by a somewhat different method, extending it to the case of two sets of particles of different diameters. Both calculations lead to the same result, viz. that the mean free path is shortened in the proportion $v : v - 4b_1$; therefore the number of the collisions, and the term in the virial equation dependent upon these collisions, must be augmented in the reciprocal proportion; but then this equation takes the form—

$$pv = \tfrac{1}{2}\Sigma mu^2\left(1 + \frac{4b_1}{v} \cdot \frac{v}{v - 4b_1}\right) = \tfrac{1}{2}\Sigma mu^2 \cdot \frac{v}{v - 4b_1}, \quad \ldots (6)$$

and becomes identical with the equation (3) of Van der Waals.[3]

In this manner the true formula is obtained by means of the virial equation, as it has been by the method of economized distances, and these verifications of the equation derived by Van der Waals are not without importance. Indeed, I always held the opinion that it is not quite allowable to conclude directly from the diminution of the free path of the molecule to a proportional augmentation of the pressure on the bounding walls. The number of the mutual encounters of the molecules, and the number of their collisions with the walls (or, rather, their passages through an ideal plane), are not

[1] *Verslagen en Mededeelingen der Kon. Ak. v. Amsterdam*, 2e Reeks, Deel x.; *Archives Neerlandaises*, t. xii.

[2] I am bound to acknowledge that the same correction, which is indicated further on for Lorentz's calculations, has to be applied to the number of collisions given in my paper.

[1] The extension in the direction of the motion is meant here. I have translated the first phrase from the original paper in *Wied. Ann.*, where it runs: "Streng genommen müsste man also hier eine Correction anbringen, wie sie von Herrn van der Waals angegeben würde; man hätte nämlich bei der Stosszahl die Grösse der Molecüle zu berücksichtigen."

[2] *Verslagen en Mededeelingen*, 2 Reeks, Deel x.; *Archives Neerlandaises*, t. xii.

[3] I owe this remark to a verbal communication by Van der Waals.

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proportional numbers under *all* circumstances. A change in the shape of the molecules, or an augmentation of their diameters, will affect the first number in a much greater proportion than the second. But, as I have shown, the equation of Van der Waals holds good, independently of this assumption.

D. T. KORTEWEG.

Amsterdam, November 4.

THE BIRD-GALLERY IN THE BRITISH MUSEUM.

A LONG-NEEDED and much-wished-for reform, to which the attention of naturalists should be specially invited, has been commenced in the Bird-gallery of the British Museum. Under the old *régime* at Bloomsbury, the rule was, as it is even now in most of the Continental Museums, that every specimen should be stuffed, and exhibited in the public gallery. The natural, if not the necessary, consequence of such a rule is that, as time progresses, the shelves become crowded with badly mounted specimens, which are very unpleasing to the general observer, and most inconvenient to the scientific worker.

In the British Museum, however, the idea of mounting every specimen has been long ago abandoned. The main collection for scientific work is, we need hardly say, that of skins. These are arranged in cabinets, in numbers which it would be impossible to find space for if "mounted." When thus disposed of they are much more easy to find, and more convenient for examination, than "mounted" specimens Though it may be sometimes necessary to refer to the Bird-gallery, the working ornithologist of this country, as a rule, uses only the skin collection.

This being so, the question arises as to what is the best way of making the Bird-gallery useful, and attractive to the general public. As to this there can be no question, it appears, that the Bird-gallery should be fitted up as an "Index Museum," and should contain a series of the principal types of bird-life arranged in systematic order from the highest to the lowest. Every family should be placed in a separate case, in its proper position between the two groups to which it is most nearly allied. In each family a series of well-mounted specimens should illustrate the principal sub-families and genera, and the male and female and other plumage of the leading species. Nests and eggs should be added to show the mode of nidification, and maps to show the areas of distribution. Diagrams and preparations of particular structures should be placed at the head of each group, to exhibit its special peculiarities; and finally, every specimen and diagram should be clearly labelled and explained. It will readily be understood that a Bird-gallery filled up in this way would be a most instructive object, and much more useful and attractive than the crowded rows of uniformly-set-up specimens that are offered to view in most public Museums. Some such plan as this, we take it, is what the authorities of the British Museum have now in view.

For a commencement, the family of Woodpeckers has been selected, and a case devoted to its illustration has been fitted up. A series of well-mounted specimens shows the leading forms of the group, and diagrams, preparations, and maps exhibit its principal peculiarities and the distribution of the species.

This is at present only the beginning of a very important change of plan. But there can be no question that if the scheme is carried out, and the whole Bird-gallery is treated in a similar way, an admirable reform will have been effected.

THE OCTOBER ERUPTION NORTH-WEST OF PANTELLERIA.

SOME time after the news arrived in this country of the volcanic outburst in the neighbourhood of Pantelleria, my friend Mr. Gerard Butler, F.G.S., undertook to visit the island, and to investigate the interesting phenomena that were being exhibited there. Mr. Butler has now returned, having made a large collection of specimens of rocks and minerals, and I trust that before long we shall have fuller information concerning this remarkable district. The following short note embodies the general results of his inquiries concerning the recent eruptions; but telegrams received since his return state that renewed outbursts have led to the formation of an island at the spot, and mariners have been warned to avoid it.

JOHN W. JUDD.

Royal College of Science, London, December 14.

In NATURE of December 3 (p. 120), a short sketch is given of a paper by M. Ricco on the above, which those interested in the subject may read in the *Comptes rendus* for November 25.

It may be worth while for one who visited Pantelleria soon after the eruption to point out that there appears to be no foundation for the idea conveyed by many English accounts and by the words "island," "erupted island," in NATURE (*loc. cit.*), that an island comparable to Graham's Island was formed.

It seems that by a submarine eruption which, after prefatory earthquakes between October 14 and 17, was first observed on the latter day, about 5 kilometres to the north-west of Pantelleria, a narrow band of floating bombs, extending for about a kilometre in a north-east and south-west direction, was produced.

The persistence during the eruption of this linear band may perhaps indicate the line of fracture of the sea bottom.

There appears to have been always deep water at the scene of eruption. Ricco tells us that soundings at the middle and ends of the floating shoal of bombs found no bottom at 320 metres.

The brittle cindery bombs readily broke up, giving vent to the superheated steam they contained; when, or upon their becoming otherwise waterlogged, they sank, so that, on October 26, soon after the eruptive action ceased, all traces of it had disappeared in deep water.

G. W. BUTLER.

NOTES.

WE regret to have to record the death of Prof. Stas, the eminent Belgian chemist. He died at the age of seventy-eight.

AT last Thursday's meeting of the Royal Society, the President read from the chair a letter from Prof. Dewar, which had been put into his hand as he entered the meeting-room, in which Prof. Dewar stated that he had at 3 p.m. that afternoon "placed a quantity of liquid oxygen in the state of rapid ebullition in air (and therefore at a temperature of -181° C.) between the poles of the historic Faraday magnet in a cup-shaped piece of rock salt (which is not moistened by liquid oxygen and therefore keeps it in the spheroidal state)," and to his surprise, Prof. Dewar saw the liquid oxygen, as soon as the magnet was stimulated, "suddenly leap up to the poles and remain there permanently attracted until it evaporated."

ACCORDING to information sent to Berlin, Emin Pasha and Dr. Stuhlmann, travelling in the region between Lakes Victoria, Tanganyika, and Albert Edward, have discovered what they take to be the ultimate source of the Nile. This is a river called Kifu, which is supposed to have its sources in the Uhha country, lying to the east of the northern part of Lake Tanganyika,

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about 4° of south latitude. It flows into the southern end of Lake Albert Edward.

THE death of Dr. F. C. Dietrich, Keeper of the Botanical Museum at Berlin, is announced. He was eighty-six years of age.

AT the annual general meeting of the Institution of Electrical Engineers on Thursday, December 10, Prof. Ayrton was elected President for the coming year. The following are the Vice-Presidents: Alexander Siemens, R. E. Crompton, Sir David Salomons, and Sir Henry Mance. In moving the adoption of the annual report, Prof. Crookes said that the number of members elected during the past year was greater than in almost any previous year. He announced that Prof. Nikola Tesla is on his way to England, and had promised to lecture before the Institution in January next. Prof. Crookes added that the Council would spare no pains to insure that the lecture should be thoroughly well experimentally illustrated. Mr. W. H. Preece, F.R.S. (Past-President), read a paper on "The Specification of Insulated Conductors for Electric Lighting and other Purposes." In this paper the fallacy of the present mode of specifying electric light conductors was exposed, and a new standard of insulation, based on the well-known qualities of gutta-percha, was proposed. The qualities of the numerous insulating materials now in the market were measured and determined in this new standard, and it was shown that any classification of cables should be based on the pressures to be resisted, and should depend on the thickness of the insulating wall. The introduction of cheap and nasty cables, owing to competition and the absence of specification and inspection, was strongly commented on. It was shown that all danger was eliminated by the use of proper material and proper design. The paper concluded with the recital of Mr. Preece's latest specification.

PROF. A. HANSEN, of Darmstadt, has been appointed to the Professorship of Botany and Directorship of the Botanic Garden at Giessen.

PROF. E. WARNING, of Copenhagen, is at present engaged on a botanical expedition to the West Indies and Venezuela. Herr G. Schweinfurth and Prof. O. Penzig have returned from their journey in Abyssinia; and Herren J. Bornmüller and Sintenis from their botanical expedition, in the course of which they have visited the island of Thasos, Mount Athos, and the Thessalian Olympus.

THE following are the lecture arrangements of the Royal Institution before Easter, so far as they relate to science:—Prof. John G. McKendrick, six Christmas lectures to juveniles, on life in motion, or the animal machine; Prof. Victor Horsley, twelve lectures on the structure and functions of the nervous system (the brain); Prof. E. Ray Lankester, three lectures on some recent biological discoveries; Dr. B. Arthur Whitelegge, three lectures on epidemic waves; Prof. J. A. Fleming, three lectures on the induction coil and transformer; the Right Hon. Lord Rayleigh, six lectures on matter, at rest and in motion. The Friday evening meetings will begin on January 22, when a discourse will be given by the Right Hon. Lord Rayleigh, on the composition of water; succeeding discourses will probably be given, among others, by Sir George Douglas, Prof. Roberts-Austen, Mr. G. J. Symons, Prof. Percy F. Frankland, Sir David Salomons, Prof. L. C. Miall, Prof. Oliver Lodge, Mr. John Evans, and Prof. W. E. Ayrton.

LAST week a deputation of gentlemen interested in the University Extension movement had an interview with Lord Cranbrook, President of the Privy Council, to ask for a Government grant in aid of the local lectures delivered under the auspices of the organizing bodies. Among those present were Sir George Stokes, Prof Bryce, Prof. Jebb, Mr. James Stuart, the President of Magdalen College, Oxford, the Master of University College, Oxford, and the Master of Selwyn College, Cambridge. Lord Cranbrook reminded the deputation that his official duties related only to public elementary schools, and that a Government grant could be obtained for the University Extension movement only from the Treasury. He expressed sympathy, however, with the objects of the movement, and promised to consider carefully and to bring before his colleagues the arguments advanced by the deputation. Referring to the general question of secondary education, Lord Cranbrook said it was most desirable that clever boys and girls who have passed through the elementary course should be enabled, by bursaries or in some other way, to go to intermediate schools, and thus be prepared for such instruction as is offered by University Extension lecturers. He feared, however, that those who expected this object to be attained by means of a Government grant might "have to wait for some time."

ON the invitation of the Council of the Photographic Society of Great Britain, Mr. Leon Warnerke lately undertook to submit to the Society a description of the photographic technical schools on the Continent. With that object in view he visited, during last summer, Belgium, Germany, Austria, and Russia, taking notes with pencil and camera. The results are embodied in an interesting paper which was read at a recent meeting of the Society, and is now printed in the *Photographic Journal*.

THE organizing joint committee of the Essex County Council and the Essex Field Club on technical instruction have issued a circular announcing that they have resolved to appoint a certain number of lecturers on science subjects. The services of these teachers are offered free (with the exception of travelling and hotel expenses of the lecturers, where necessary) to local technical instruction committees, under certain conditions to be settled hereafter; the local committees guaranteeing audiences or classes of students (not less than twenty in number), providing rooms, gas, &c., and defraying all necessary local expenses. Syllabuses of short courses of lectures already approved are sent with the circular. They relate to elementary vegetable physiology, economic entomology, and elementary practical mechanics.

THE Royal Commission for the Chicago Exhibition are anxious to comply with a request made to them by the executive authorities of the Exhibition, that a typical collection of economic British minerals may be included in the British Section, and they are now applying to owners and managers of mines, asking for specimens of the principal British minerals. Mr. B. H. Brough, the Instructor in Mine-surveying at the Royal College of Science, South Kensington, has kindly undertaken to classify and arrange the collection, and any suitable specimens may be addressed to him. What is required is not specimens of special value or rarity, but samples of ordinary ores, &c., so that the collection when complete may be fully illustrative of the mineral resources of the kingdom. At the close of the Exhibition the collection will be presented to an American Museum.

PROF. H. A. HAZEN, acting under instructions from the U.S. Weather Bureau, is in Chicago preparing a report on its weather—the mean temperature, the winds, snows, showers, humidity, early frosts and late snows. The report will be based on all the observations and records made for the last fifty years the object being to convince everyone interested in the approaching Exposition that Chicago is exceptionally favoured in point of fine weather.

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MR. ROSEWATER, who was a distinguished member of the U.S. Military Telegraph Corps during the American civil war, and is now President of the Old Timers Telegraphic Association, has lately been studying the various Government telegraph systems in use in Europe. The results of his investigations will shortly be submitted to what is expected to be an unusually interesting and important meeting of the New York Electric Club.

A COPY of "Whitaker's Almanack for 1892" has been sent to us a few days in advance of publication. Great care has been taken, as usual, to keep the Almanack up to date. Additional space is devoted to educational matters, and for the first time educational progress and occurrences are dealt with in a separate article. There is also a separate article on agricultural education. Other subjects separately treated are the rise, progress, and achievements of the great lines of ocean steamers, naval gunnery, and the results of the census. Of course, various sections resemble in subject those of former years, but even these are for the most part entirely fresh in substance. In many instances the changes wrought during the interval of a single year are so numerous that scarcely a line of the section in which the subject is treated remains unaltered.

OWING to his declining to take up his residence in Rio de Janeiro, Dr. Fritz Müller, of Blumenau, Sta. Catharina, has been summarily dismissed by the new Government of Brazil from his post of "Naturalista viajante" to the Museum at Rio de Janeiro. The great services which Dr. Müller has rendered both to zoological and botanical science during his forty years' residence in Brazil are too well known and too widely acknowledged to need dilating on. Dr. F. Müller is now close on completing his seventieth year; and Dr. Karl Müller, of Halle, the editor of *Natur*, proposes to seize the opportunity of collecting from his fellow-naturalists some substantial recognition of the honour in which he is held.

DR. H. E. HAMBERG has communicated to the Swedish Academy of Sciences a paper on the radiation of the upper clouds round barometric minima, prepared from the cloud observations available at Upsala Observatory for the years 1874–89. The arrangement of the highest clouds—cirrus and cirro-stratus—in the form of parallel bands has long been noticed by meteorologists in this country, and various papers on the subject have been written by Mr. W. C. Ley, MM. Hildebrandsson, Köppen, and others; and the movements of these clouds, in conjunction with the wind prevailing at the earth's surface, are at times sufficient to determine approximately the direction in which an atmospheric disturbance exists, even without the use of synoptic charts. For instance, a barometric minimum often exists in a direction nearly perpendicular to that of the radiation, and, generally, on that part of the horizon where the bands of upper clouds are most dense, or whence they seem to radiate, but it is always necessary to take into account the direction of the wind at the earth's surface. The author draws the following conclusions from his investigation:—(1) The radiation of the upper clouds is closely connected with barometric minima. (2) Near a barometric minimum, with the pressure below 29·9 inches, the radiation forms with the radius of the depression an angle of about 70°, the deviation of the radiation from the direction of the surface wind being positive (*i.e.* to the right), by some degrees, on the south-west of the barometric minimum, and negative on the south-east of it. (3) Further from the barometric minimum, with a pressure of 29·9 inches and above, the inclination to the radius is rather greater, about 75°, except where the barometric minimum lies to the north of the place of observation, in which case it is much lower. (4) The angle formed by the radiation is generally greater in the rear of a barometric minimum, reaching nearly 90° in a high pressure; on the other hand, it is smaller in front, especially to the south-east of a minimum, and further from the centre. (5) Compared to the general circulation of the air in a barometric minimum, the radiation of the upper clouds most resembles the direction of the wind near the earth's surface. The meaning of this last sentence is not obvious; but the other conclusions agree, on the whole, with the views of other meteorologists who have studied the subject.

THE *Meteorologische Zeitschrift* for November contains a summary, by Dr. J. Hann, of the meteorological observations taken at Cairo from 1868–88. The observations have been published *in extenso*, together with a good introduction upon the climate, in the Bulletin of the Egyptian Institute, and although similar observations have occasionally been published before, the present series contains much new and useful material. The most striking feature in the climate of this part of Egypt is the *Chamsin*, the hot and dust-bearing wind which makes its appearance in March or April for about three to four days at a time, and robs a large portion of the trees of their leaves. In the intervals during which this wind is not blowing the weather is pleasant and clear during spring-time, and the nights fresh and calm. During the summer the north winds prevail, with high temperature, very clear air, and great dryness. Towards September humidity appears with the rise of the Nile, the ground is at times covered with heavy dew, and the heat becomes oppressive on account of the moisture. In October and November fog occasionally occurs in the morning, and rain begins to fall. After this season the temperature is uniform and pleasant. Snow is unknown, frost very seldom occurs, and rain is not very frequent. The absolute maximum temperature of the 21 years period was 117° in August 1881, which was also closely approached in May 1880, viz. 116°·4. The absolute minimum was 28°·4 in February 1880, and the mean annual temperature was 70°·5. Rainfall is only given for the years 1887–88, in which 0·87 and 1·67 inches fell respectively. The relative humidity sinks at times even on a daily average to 12 per cent., and has been known to fall as low as 3 per cent. at certain hours. Thunderstorms and hail are very rare. The original work contains a long investigation on the connection between the height of the Nile and the weather, a comparison between the present climate and that at the beginning of this century, and several carefully prepared diagrams referring to all meteorological elements.

THE refraction and velocity of sound in porous bodies allowing passage of sound, such as sponge, wadding, felt, &c., have been recently made a subject of investigation by Herr Hesehus (*Rep. der Physik*). His plan was to make a plane-convex spherical lens of iron wire net, and fit it, filled with the porous body (variously condensed), into an aperture in a screen. A pipe of variable tone was sounded on one side, and the behaviour of a sensitive flame noted on the other. From the distance of the focus, when found, could be deduced the refractive index and the velocity in the lens. The refrangibility grows with increasing density of the porous substance, while the velocity on the average is lessened; the latter is also less, the greater the sound-wave. The author gives details of experiments in which the velocity varied from 146 to 261 metres per second (with ebonite shavings). From an empiric formula which he gives, he makes deductions regarding the propagation of sound in tubes, considered as only a special case of its spread through the pores and passages of a porous body. He hopes further research in this field may do something to elucidate the passage of light and electricity through media.

THE Report of the United States Commissioner of Education has been submitted to the Secretary of the Interior. It says that the usefulness of the Bureau depends directly upon what it

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prints and publishes, and therefore urges an appropriation of 30,000 dollars for general printing for the fiscal year 1892-93, and makes a special request for a specific appropriation of 20,000 dollars to continue the series of educational histories of the several States. The Commissioner reports that there were enrolled in 1889-90 in the public schools of the United States of elementary and secondary grade 12,686,973 pupils, as against 9,867,505 in 1880. The enrolment formed 20·27 per cent. of the population of 1890. The average daily attendance of pupils on each school day in 1890 was 8,144,938. The whole number of public school teachers in the past year was—males, 125,602; females, 233,333. The total amount expended during the past fiscal year for public school purposes was 140,277,484 dollars, is against 63,396,666 dollars in 1870, and 78,004,687 dollars in 1880. The expenditure per capita of population in 1880 was 1.56 dollars, while in 1890 it was 2.24 dollars.

THE U.S. Bureau of Education has issued, as one of its "Circulars of Information," an excellent paper on "Sanitary Conditions for School-houses," by Albert P. Marble, Superintendent of the Public Schools of Worcester, Mass. Dr. Marble has for many years studied the problems of ventilation, heating, lighting, draining, and school-house construction; and his suggestions are well worthy of consideration in this country, as well as in America. The value of the Circular is increased by an appendix, in which are given a number of designs of school buildings of various sizes, carefully selected with a view to commodiousness, healthfulness, and economy of construction. In an official statement prefixed to the Circular, attention is especially called to a series of nineteen plates constituting the prize designs selected and published by the State of New York in 1888.

IN the interesting paper on insectivorous plants, read before the Royal Horticultural Society on September 22, 1891, and now published in the Society's Journal, Mr. R. Lindsay refers to the experiments by which Mr. Francis Darwin has shown the amount of benefit accruing to insectivorous plants from nitrogenous food. Mr. Lindsay says his own experience in the culture of Dionæa is that when two sets of plants are grown side by side under the same conditions in every respect, except that insects are excluded from the one and admitted to the other, the latter, or fed plants, are found to be stronger and far superior to the former during the following season. He points out the importance of remembering that the natural conditions under which these plants are found are different from what they are under cultivation. In their native habitats they grow in very poor soil and make feeble roots, and under these conditions may require to capture more insects by their leaves to make up for their root deficiency. Under culture, however, fairly good roots for the size of plant are developed. "Darwin," says Mr. Lindsay, "mentions that the roots of Dionæa are very small: those of a moderately fine plant which he examined consisted of two branches, about one inch in length, springing from a bulbous enlargement. I have frequently found Dionæa roots six inches in length; but they are deciduous, and I can only conjecture that the roots mentioned by Darwin were not fully grown at the time they were measured. What is here stated of the natural habits of Dionæa applies more or less to all insectivorous plants."

GOOSEBERRIES are so much liked by most people that it is very desirable the season for them should, if possible, be prolonged. According to Mr. D. Thomson, who has a good paper on gooseberries in the current number of the Journal of the Royal Horticultural Society, this can be done easily in the northern part of Great Britain. At Scotch shows it is quite usual to see fine fresh gooseberries about the middle of September. These, as a rule, are gathered from ordinary bushes that have perhaps been shaded with mats or canvas after becoming ripe. The best way to lengthen out the season of gooseberries, Mr. Thomson says, is to plant a portion of a wall with a due north aspect with some Warringtons, and train them on the multiple-cordon system, and keep the laterals spurred in precisely the same way as is adopted with red currants on fences or walls, or in fact with gooseberry bushes grown in the ordinary way. The main shoots should not be closer than 10 inches. If a coping of wood be placed on the wall to throw off wet, a net being used to protect the fruit from birds, the gooseberries can be kept fresh till far into October, and are then very useful and acceptable.

THE authorities responsible for the working of the free public libraries of Manchester cannot complain that these institutions are inadequately appreciated. From the Thirty-ninth Annual Report on the subject to the Council of the city we learn that, during the year ended September 5, 1891, the number of visits made by readers and borrowers to the Manchester libraries and reading-rooms reached an aggregate of 4,327,038, against a total for the preceding twelve months of 4,195,109. The number of volumes lent for home reading was 702,803. Of these, only thirteen are missing.

AT a recent meeting of the Chemical Section of the Franklin Institute, Dr. Bruno Terne read a paper on the utilization of the by-products of the coke industry. In the course of his remarks he said it seemed strange, and nevertheless was a fact, that, with all the ingenuity of the American people in the advancement of the purely mechanical part of the technical industries, they have been and are still slow in the development of chemical industries. "If," said Dr. Terne, "you will visit our coal region to-day, you will find the nightly sky illumined from the fires of the coke ovens, and every one of the brilliant fires bears testimony that we are wasting the richness of our land in order to pay the wiser European coke manufacturer, who saves his ammonia and sends it to us in the form of sulphate of ammonia; and who also saves his tar, which, after passing through the complex processes of modern organic chemistry, reaches our shores in the form of aniline dies, saccharin, nitrobenzol, &c." Dr. Terne thinks that every pound of ammonia used in America ought to be produced there, and that every pound of soda should be made from American salt wells by the ammonia process.

MR. COLEMAN SELLERS contributes to the December number of the *Engineering Magazine*, New York, the first of a series of articles on what he calls "American Supremacy in Mechanics." Incidentally, he notes that most English inventions brought to the United States have to be "Americanized, simplified, made accessible in the case of machinery, and constructed with a view to ease of repair as well as to durability when under the care of careless attendants." Mr. Sellers does not think it would be worth the while of Americans to copy "the solidity and immense weight that some deem a merit in English machinery."

ACCORDING to the "World's Fair Notes," sent to us from Chicago, the party which, under the direction of Mr. Putnam, has been making excavations in the mounds of Ohio, made an important discovery on November 14. While at work on a mound 500 feet long, 200 feet wide, and 28 feet high, the excavators found near the centre of the mound, at a depth of 14 feet, the massive skeleton of a man incased in copper armour. The head was covered by an oval-shaped copper cap; the jaws had copper mouldings; the arms were dressed in copper, while copper plates covered the chest and stomach, and on each side of the head, on protruding sticks, were wooden antlers ornamented with copper. The mouth was stuffed with genuine pearls of immense size, but much decayed. Around the neck was a necklace of bears' teeth, set with pearls. At the side of this skeleton was a female skeleton.

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ACCORDING to a telegram received in New York from Mexico, the Mexican Government has ordered the inhabitants of the villages in the neighbourhood of the town of Colima to abandon their homes and seek refuge elsewhere, as the volcano in the vicinity, which was recently in eruption, shows signs of fresh activity, and the country for miles around it is illuminated by the flames issuing from the crater.

THE census of 1890 in Austria-Hungary shows that the rate at which the population increased during the preceding ten years was very different in the two great divisions of the Monarchy. The increase in so-called Cisleithania was 7·9 per cent.; in Transleithania, 10·82 per cent. In the individual provinces the increase was very unequal. In Lower Austria it was 13·8 per cent., this high rate being due to the attractive force of Vienna. Then came Bukowina with 13·1 per cent.; Galicia, 10·4 per cent.; Silesia, 6·5 per cent.; Moravia, 5·5 per cent.; Bohemia, 5 per cent.; the Alpine lands, from 3·2 to 3·6 per cent.; and Tyrol, 0·9 per cent. A different set of figures is yielded by the increase of the various nationalities. Among these the Poles stand highest, with 15 per cent.; then the Serbo-Croatians, 14 per cent; the Ruthenians, 11 per cent.; the Germans, 5·66 per cent.; the Czechs, 5·65 per cent.; the Slovenians, 3·18 per cent.; and the Italians, under 1 per cent.

MR. HENRY LAVER records in the current number of the *Zoologist* the capture of a spotted eagle at Elmstead, near Colchester, on October 29, 1891. On that day a farm labourer saw a strange bird, evidently in an exhausted condition, alight in the field in which he was working. When he went after it, it rose, and flew about a hundred yards. He soon came up to it, and, after some little difficulty, from its pugnacity, captured it alive and uninjured, and in a few days sold it to a gipsy, who in turn disposed of it to Mr. Pettitt, the local taxidermist. Mr. Laver says its plumage appears to indicate good health, and that its appetite favours that idea. If any injury led to its capture, all marks of it have quite disappeared.

THE new instalment of the Transactions of the Leicester Literary and Philosophical Society (vol. ii. part ix.) contains an abstract of an interesting lecture by Mr. Harold Littledale, of the College, Baroda, on some of his experiences with big game in India. Mr. Littledale gave an especially good account of shooting in the Himalayas. The ibex and markhoor were found at altitudes varying from 10,000 to 20,000 feet, and could be obtained only by perseverance in the face of many dangers and obstacles. Of the markhoor (*Capra megaceros*), a splendid animal which is becoming increasingly rare, he obtained ten examples, and ibex had also fallen to his gun, with 45 inch horns—the maximum development being about 52 inches. Various species of sheep also occurred, as the magnificent *Ovis poli*, which the lecturer had not yet met with, *Ovis ammon*, *Ovis cycloceros*, &c. The chamois was found commonly in the Himalayas, and Hodgson's antelope could be shot at elevations of 20,000 feet. Amongst the other mountain animals described were the snow leopard, Sikkim stag, and musk-deer (*Moschus moschiferus*), with its tusks about 5 inches in length.

MR. W. H. ROSSER has written for the benefit of candidates preparing for the Board of Trade examinations a general explanation of what is usually known as the "Compass Syllabus." It is entitled "Compass Deviation: a Syllabus of Examination in the Laws of Deviation, and in the Means of Compensating it," and is published by Messrs. James Imray and Son. The pamphlet is to be regarded as an appendix to Mr. Rosser's "Deviation of the Compass considered practically."

MM. H. LÉVEILLÉ AND A. SADA, of Pondicherry, have started a new botanical journal with the title *Le Monde des Plantes: Revue Mensuelle de Botanique.* The first number appeared on October 1. It is published at Le Mans (Sarthe).

THE Council of the Owens College have published the first volume of "Studies in Anatomy." It is edited by Prof. A. H. Young, and presents a part of the results of investigations conducted in the anatomical department of the College during the last three or four years.

MESSRS. BAILLIÈRE, TINDALL, AND COX have issued a second edition of Dr. Edridge-Green's work on "Memory: Its Logical Relations and Cultivation."

A NEW edition of "Falling in Love: with other Essays on More Exact Branches of Science," by Mr. Grant Allen, has been published by Messrs. Smith, Elder, and Co.

MESSRS. BEMROSE AND SONS have issued a second edition of a "Hand-book to the Geology of Derbyshire," by the Rev. J. Magens Mello. The work has been rewritten, and is illustrated with a map and sections.

A CURIOUS compound of lead, sodium, and ammonia, $Pb_4Na.2NH_3$, is described by M. Joannis in the current number of the *Comptes rendus*. M. Joannis has been studying the nature and reactions of the substance known as sodammonium, obtained by dissolving metallic sodium in liquefied ammonia. The deep blue liquid thus produced has been shown in a previous communication (see NATURE, vol. xliii. p. 399) to decompose slowly at the ordinary temperature into hydrogen gas and sodamide, a compound of the composition $NaNH_2$, which M. Joannis isolated in the form of colourless crystals. That such a compound as sodammonium $(NaNH_3)_n$, really exists in the blue solution in liquefied ammonia would appear to be the most natural assumption from these experiments. The reactions of sodammonium now described lend additional support to this view. When a rod of pure lead is placed in a saturated solution of sodammonium in water, the reddish-brown liquid becomes rapidly blue, and finally assumes a deep green tint. A small quantity of hydrogen is evolved at the same time owing to the decomposition of a portion of the sodammonium into sodamide, as above described. The lead gradually disappears, and a solid substance possessing an indigo-blue colour is deposited. This blue substance is found upon analysis to consist of the compound $Pb_4Na.2NH_3$, and would appear to be a sodammonium in which a portion of the sodium is replaced by lead. It dissolves readily in liquefied ammonia with formation of a solution possessing a bottle-green tint. It is not very stable, dissociating spontaneously on standing, with production of a grey substance very much resembling spongy platinum. Upon exposure to air it becomes warm owing to its rapid oxidation. It behaves in a somewhat remarkable manner towards water. When introduced in small quantities at a time into ordinary water, the first portions dissolve completely, the oxygen dissolved in the water oxidizing the lead to litharge, which at once dissolves in the alkaline solution formed. As soon, however, as the oxygen in the water is used up, further additions of the substance result in the precipitation of black flocculæ of metallic lead. Another interesting reaction of sodammonium is that with metallic mercury, which behaves in an entirely different manner from lead. When the solution of sodammonium in liquefied ammonia is poured over a globule of mercury, rapid action occurs, with the ultimate elimination of the whole of the ammonia, and production of a sodium amalgam of the composition $NaHg_8$, which has been obtained in well-formed crystals. This reaction is the more interesting inasmuch as M. Berthelot, from purely thermo-chemical considerations, has previously indicated the possible existence of such a compound of sodium and mercury.

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THE additions to the Zoological Society's Gardens during the past week include a Sooty Mangabey (*Cercocebus fuliginosus* ♀) from West Africa, presented by Mr. H. E. Dampier, J.P.; a Rufous-necked Weaver-Bird (*Hyphantornis textor* ♂) from West Africa, presented by Commander W. M. Latham, R.N., F.Z.S.; a White Stork (*Ciconia alba*), European, presented by Mr. Walter Chamberlain, F.Z.S.; eighteen Grenadier Weaver Birds (*Euplectes oryx*), ten Golden-backed Weaver Birds (*Pyromelana aurea*), nine Black-capped Weaver Birds (*Hyphantornis nigriceps*), four Red-bellied Waxbills (*Estrelda rufiventris*), three Triangular-spotted Pigeons (*Columba guinea*), four Dwarf Chameleons (*Chamæleon pumilus*) from South Africa, presented by Mr. R. W. Murray.

OUR ASTRONOMICAL COLUMN.

JUPITER AND HIS FIRST SATELLITE.—A series of observations of spots and markings on the planet Jupiter were communicated to the Royal Astronomical Society at the November meeting by Mr. Barnard. A careful study of numerous details observed during a period of twelve years has led to the conclusion "that the red colour of any of the markings is an indication of their age; or in other words, when a spot or marking (other than the white spots) first appears it is dark or black, but after some time turns red." Several examples are given of this transition, and the great red spot seems to be no exception to the rule. Measurements of transits of the broken chain of small black spots just north of the north equatorial belt, discovered by Mr. Barnard during the present year, show that the spots have a very large relative motion, for they complete a revolution around Jupiter in about thirty-seven days. The oblong dusky spot discovered near the great red spot last year is diminishing its longitude by about 0°·54 per day, and so completes a revolution relatively to the latter in about 167 days. This, and other new red markings in the southern hemisphere, seem to have their origin in the region of the great red spot. Their period of rotation is about the same as the round white spots in the same hemisphere, the longitudes of which diminish by about 0°·6 per day. The observations show that the great red spot is stationary in longitude, and possibly shorter and broader now than in 1880. Further observations of the first satellite have been made in order to throw light upon the apparent duplicity of this body in transit, distinctly seen by Mr. Barnard on September 8, 1890. It is noted:—"The phenomena seen on these occasions would rather discourage the idea of actual duplicity. At these times the satellite has appeared egg-shaped when in relief on the dark belt. . . . I am confident that this particular phase, and perhaps also that of apparent duplicity, is explained by a bright belt on the satellite or by darkness of the polar regions, which is the same thing." Mr. Stanley Williams has suggested that the phenomenon observed on September 8, 1890, may have been due to the satellite having been seen in transit as a dark spot close to a dark spot on the surface of Jupiter which transited at the same time.

SPECTRA OF THE SUN AND METALS.—Some extremely fine comparative photographic spark-spectra of the sun and metallic elements were exhibited by Mr. F. McClean at the meeting referred to in the above note. The spectra extend, in six sections, from λ 3800 to λ 5750—that is, from about L of the solar spectrum to near D. They are divided into two series—one containing spectra of the sun, iron, platinum, iridium, osmium, palladium, rhodium, ruthenium, gold, and silver; the other containing spectra of the sun, iron, manganese, cobalt, nickel, chromium, aluminium, and copper. The scale of wave-length adopted is that of Ångström's map. Since the spark was taken in air all the spectra have air-lines running through them. The purest materials obtainable were used as electrodes: nevertheless a large number of lines due to foreign substances appear on the photographs. The commonest impurity is calcium, its lines being present in very nearly all the spectra. No attempt has been made to eliminate the lines having their origin in such impurities; hence, as Mr. McClean remarks, it is impossible "to obtain any complete results from these two series of photographs alone. Photographs of the spectra of all the common oxidizable metals, and particularly of calcium, barium, magnesium, and titanium are first required."

Astronomische Nachrichten, No. 3068, contains the following ephemeris for the comet Tempel$_3$-Swift for 12h. Paris mean time:—

1891.	α. h. m. s.	δ. ° ′ ″	Log Δ.
Dec. 17 ...	1 38 47 ...	+27 4 45 ...	9·4220
18 ...	46 1 ...	13 33	
19 ...	53 10 ...	21 1 ...	9·4339
20 ...	2 0 13 ...	27 3	
21 ...	7 10 ...	31 55 ...	9·4467
22 ...	14 1 ...	35 32	
23 ...	20 44 ...	38 4 ...	9·4606
24 ...	27 19 ...	39 24	
25 ...	33 47 ...	39 50 ...	9·4753
26 ...	40 7 ...	39 18	
27 ...	46 19 ...	37 59 ...	9·4907
28 ...	52 22 ...	35 47	
29 ...	58 17 ...	32 57 ...	9·5068
30 ...	3 4 5 ...	29 24	
31 ...	9 43 ...	25 2 ...	9·5235

Astronomische Nachrichten, No. 3069, contains a paper by Prof. Pickering on the distribution of energy in stellar spectaa. Since stellar magnitudes obtained by various processes, such as photography, eye-observations, &c., cannot be compared when the light of stars is of different colours, the method he proposes is to adopt a single wave-length in the spectrum to which all intensities should be referred, a curve or series of numbers being necessary to give a measure of the rays of each different wave-length. For rays of different wave-lengths he says: "The intensities may be determined by comparing the densities of different portions of the photographic spectrum." The line fixed upon was that of the hydrogen line G, "as it is near the centre of the photographic spectrum." The photographs he employed were those forming part of the Henry Draper Memorial, all taken under similar conditions, and in each one separately twenty points were taken and compared by comparison with a standard photographic wedge. Each of the measures thus obtained was converted into logarithmic intervals, and the measure, corresponding with that of the hydrogen line G of wave-length 434, was deducted. By subtracting the values of the logarithm of the energy of the solar light, the remainder showed "the excess or deficit of energy of the star as compared with that of the sun, eliminating the various sources of error enumerated above." In the table below we give the results for three stars as obtained by Prof. Pickering:—

λ.	Log E.	α Can. Maj.	α Aur.	α Orion.	E.
390 ...	− 0·26 ...	+ 0·37 ...	+ 0·32 ...	− 0·57 ...	0·55
400 ...	− 0·19 ...	+ 0·10 ...	+ 0·08 ...	− 0·38 ...	0·65
410 ...	− 0·12 ...	+ 0·10 ...	+ 0·03 ...	− 0·23 ...	0·76
420 ...	− 0·07 ...	+ 0·00 ...	+ 0·01 ...	− 0·11 ...	0·85
430 ...	− 0·02 ...	+ 0·00 ...	+ 0·00 ...	− 0·02 ...	0·95
440 ...	+ 0·02 ...	− 0·01 ...	+ 0·00 ...	+ 0·05 ...	1·05
. ...	. ...	. ...	. ...	. ...	.
. ...	. ...	. ...	. ...	. ...	.
. ...	. ...	. ...	. ...	. ...	.
. ...	. ...	. ...	. ...	. ...	.

The values in the second column representing the *logarithm* of the energy of the solar light, while those in the last one represent the energy of sunlight itself. Thus in the case of α Orionis, the energy for the wave-length 390 is represented by − 0·57, sunlight being 0·55. The absolute energy is found "by adding the tabular number to that given for sunlight in the second column," so that we have − 0·26 − 0·57 = − 0·83, corresponding to a ratio of 0·15. Thus the energy of the light of α Orionis of wave-length 390 is only about one-seventh of that of wave-length 434.

In this number, also, Mr. Truman Saffard contributes a paper on the observation of North Polar stars in the vertical of Polaris. After mentioning the difficulty of observing polars in the daytime, of connecting other polars with double transits of Polaris, and of the independence that now exists in the various Polar catalogues, he describes a method which tends to eliminate many of these deficiencies. It consists in adjusting a transit so that Polaris will be near its centre wire at eastern elongation, which takes place about 19h. 23m. sidereal time, and the two stars Camelopardalis 25 H and Schwerd 1172 (Carrington 2965), which pass the same vertical within about half an hour of this time, the latter above Pole, earlier than the Pole-star reaches

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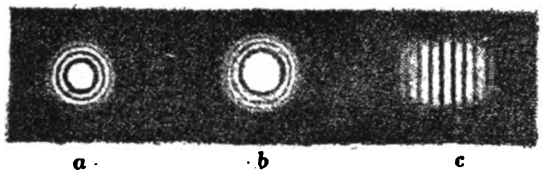
the elongation, the former, below Pole, later. In this way the right ascension of Polaris plays a small part in its azimuth of elongation, which is dependent solely on the declination and latitude. Assuming the present declinations of the two stars mentioned, with probable errors of $< \pm 0''.2$ and $\pm 0''.3$ respectively, he finds that the right ascension would probably be in error by $\pm 0^s.002$ and $\pm 0^s.004$. In fact, the probable errors "dependent upon anything but the transit of the star to be determined will be much less if the present method is used (with an equal instrument), than if stars in the same declination, but opposite Polaris in right ascension, were observed by direct comparisons in the meridian." By applying this method to other stars of different right ascensions and "gradually increasing declinations," as the R.A. of Polaris or its opposite is approached, numerous co-ordinates thoroughly independent can be obtained, and will "provide zero points for the proposed number of photographic plates 2° square, and consequently help to settle the places of all stars in that region."

MEASUREMENT OF JUPITER'S SATELLITES BY INTERFERENCE.

IT has long been known that even in a telescope which is theoretically perfect, the image of a luminous point is composed of a series of concentric circles with a bright patch of light at the common centre. This system of circles can easily be observed by examining any bright star with a telescope provided with a circular diaphragm which diminishes the effective aperture. The appearance of the image is shown in Fig. 1, *a*. In the case of an object of finite angular magnitude the image could be constructed by drawing a system of such rings about every point in the geometrical image. The result for a small disk (corresponding to the appearance of one of the satellites of Jupiter as seen with a 12-inch telescope whose effective aperture

Fig 1



a *b* *c*

has been reduced to six inches) is given in Fig. 1, *b*; the chief points of difference between this and Fig. 1, *a*, being the greater size of the bright central disk, and the lesser clearness of the surrounding rings. The larger the disk the more nearly will the appearance of the image correspond to that of the object; and the smaller the object the more nearly does it correspond with Fig. 1, *a*, and the more difficult will be the measurement of its actual size. Thus, in the case just cited, the actual angular diameter is about one second of arc, and the uncertainty may amount to half this value or even more.

The relative uncertainty, other things being equal, will be less in proportion to the increase in the aperture, so that with the 36-inch telescope the measurement of the diameters of Jupiter's satellites should be accurate to within ten per cent. under favourable conditions.

It is important to note that in all such measurements the image observed is a diffraction phenomenon—the rings being interference fringes, and the settings being made on the position of that part of a fringe which is most easily identified. But such measurements must vary with the atmospheric conditions and especially with the observer—for no two observers will agree upon the exact part of the fringe to be measured, and the uncertainties are exaggerated when the fringes are disturbed by atmospheric tremors.

If, now, it be possible to find a relation between the size of the object and the *clearness* of the interference fringes, an independent method of measuring such minute objects will be furnished; and it is the purpose of this paper to show that such a method is not only feasible, but in all probability gives results far more accurate than micrometric measurements of the image.

In a paper on the "Application of Interference Methods to Astronomical Measurements"[1] an arrangement was described for producing such fringes, by providing the cap of the objective with two parallel slits, adjustable in width and distance apart. If such a combination be focussed on a star, then, instead of the concentric rings before mentioned, there will be a series of straight equidistant bands whose length is parallel with the slits, the central one being brightest,[1] Fig. 1, *c*.

The general theory of these fringes may be found in the *Philosophical Magazine* for March 1891. The general equation showing the relation between the *visibility* of the fringes and the distance between the slits is:

$$V^2 = \frac{\left[\int \phi(x) \cos kx\, dx\right]^2 + \left[\int \phi(x) \sin kx\, dx\right]^2}{\left[\int \phi(x)\, dx\right]^2} \quad . \quad (1)$$

which reduces to the simpler form

$$V = \frac{\int \phi(x) \cos kx\, dx}{\int \phi(x)\, dx} \quad . \quad (2)$$

when the object viewed is symmetrical.

A number of applications of this formula are discussed in the former paper, but for the present purpose attention will be confined to the case in which the object viewed (or rather its projection) is a circular disk, uniformly illuminated.

In this case equation (2) becomes

$$V = \int_0^1 \sqrt{1-\omega^2} \cdot \cos \pi \frac{\alpha}{\alpha_0} \omega \cdot d\omega \quad . \quad (3)$$

in which α is the angular diameter of the object, and α_0 is the smallest angle resolvable by an equivalent aperture; that is, the ratio of a light-wave to the distance between the slits.

The curve expressing this relation is given in Fig. 2, in which the ordinates are values of the visibility of the fringes, and the abscissæ are the corresponding values of the α/α_0.

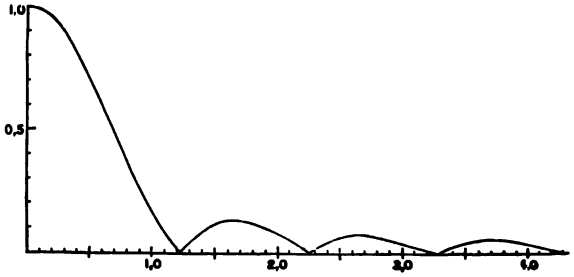


FIG. 2.

From this it will appear that the fringes disappear at recurring intervals, and in a laboratory experiment as many as four such disappearances were noted, and the average error in the resulting value of α, the angular magnitude of the disk, was found to be less than two per cent.

From the curve it is evident that the first disappearance is most readily and accurately observed, and for this we have

$$\frac{\alpha}{\alpha_0} = 1.22;$$

whence, putting s for the distance between the centres of the slits, and taking for the wave-length of the brightest part of the spectrum 0·0005 mm.,[2] and dividing by the value of a second in radians we have

$$\alpha = \frac{1.38}{s} \quad . \quad (4)$$

In consequence of the kind invitation extended by Prof. Holden, it was decided to make a practical test of the usefulness of the proposed method at Mount Hamilton.

[1] *Philosophical Magazine*, July 1890.

[1] These will be superposed on another set of fringes due to diffraction from the edges of the slits; but the latter are too faint and broad to cause any confusion.

[2] The wave-length will, of course, vary somewhat with the object observed, but may be made constant by interposing a red glass.

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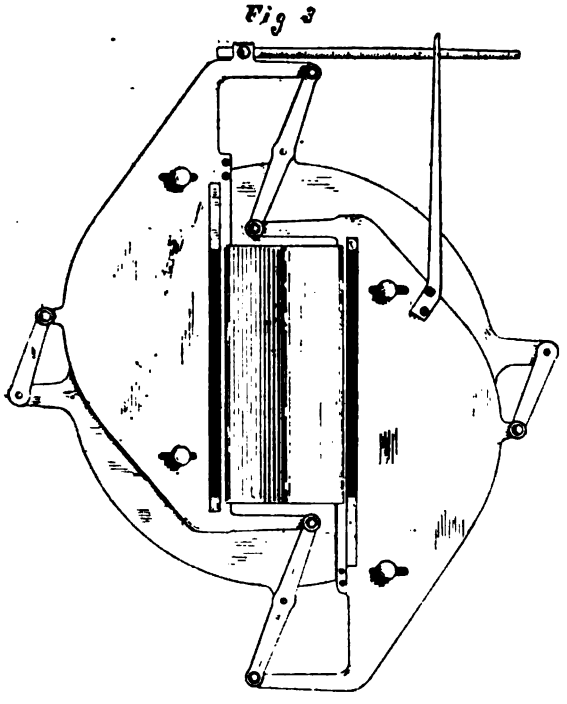
For the preliminary experiments which are to be described it was thought desirable to use the 12-inch equatorial. Accordingly, a cap, provided with two adjustable slits, was fitted over the objective, and provided with a rod by means of which the distance between the slits could be altered gradually and at will by the observer, while the distance was measured on a millimetre scale attached to the sliding jaws. This arrangement, which was constructed under the supervision of Mr. F. L. O. Wadsworth, of Clark University, is shown in the accompanying diagram, Fig. 3.

Fig 3



With this apparatus the satellites of Jupiter were measured, with results as given in the following table :—

TABLE I.

No. of Satellites.	I.	II.	III.	IV.	Seeing.
August 2 ...	1"·29 ...	1"·19 ...	1"·88 ...	1"·68 ...	Poor.
August 3 ...	1·29 ...	— ...	1·59 ...	1·68 ...	Poor.
August 6 ...	1·30 ...	1·21 ..	1·69 ...	1·56 ...	Poor.
August 7 ...	1·30 ...	1·18 ...	1·77 ...	1·71 ...	Good.
Mean...	1·29	1·19	1·73	1·66	

These are the values of the angular diameters of the satellites of Jupiter as seen from the earth. To reduce these to Jupiter's mean distance these values are to be multiplied by 0·79, which gives for the final values—

I.	II.	III.	IV.
1"·02 ...	0"·94 ...	1"·37 ...	1"·31

For the sake of comparison these values are recorded in the following table, together with those given by Engelmann, Struve, and Hough, and the last column contains some results kindly furnished by Prof. Burnham with the 36-inch on the same date (August 7) as the last of the series by A. A. M. :—

TABLE II.

No. of Satellite.	A. A. M.	Eng.	St.	Ho.	Bu,
I...	1·02 ...	1·08 ...	1·02 ...	1·11 ...	1·11
II.	0·94 ...	0·91 ...	0·91 ...	0·98 ...	1·00
III.	1·37 ...	1·54 ...	1·49 ...	1·78 ...	1·78
IV.	1·31 ...	1·28 ...	1·27 ...	1·46 ...	1·61

It was found impossible to see the reappearance of the fringes on increasing the distance, yet the results of Table I. show that the disappearance could still be sharply marked. Indeed the concordance of the observations made under different circumstances on different nights was even closer than was expected. With a larger telescope both the brightness of the fringes and their distance apart will be increased, and it may be confidently predicted that the accuracy will then be even greater.

The values given in the second column, "Engelmann," are probably more reliable than the succeeding ones, but it is well worth noting that the results obtained by interference agree with the others quite as well as these agree with each other.

It should also be noted that the distance between the slits was about four inches. It may therefore be stated that for such measurements as have just been described, a telescope sufficiently large to admit a separation of four inches—say a six-inch—suitably provided with adjustable slits is fully equal to the largest telescopes now used without them.

It is hoped that within a few months the 36-inch equatorial will be supplied with a similar apparatus and observations begun for the definite measurement of the satellites of Jupiter and Saturn and such of the asteroids as may come within the range of the instrument.

In concluding, I wish to take this opportunity of expressing my appreciation of the courtesy of Director Holden in placing all the facilities of the Observatory at my disposal, and of the hearty co-operation of all the astronomers of the Observatory, especially the valuable assistance of Prof. W. W. Campbell in making the observations.

A. A. MICHELSON.

Mount Hamilton.

THE SAMOAN CYCLONE OF MARCH 16, 1889.

THE Samoan hurricane of March 16, 1889, is one of those historic storms that have been rendered for ever memorable by the episodes of disaster and gallantry that attended them; by the escape of H.M.S. *Calliope*, which forced her way out of Apia harbour in the teeth of the hurricane, amid the cheers of the brave American sailors, who, themselves face to face with imminent death, forgot for a moment their own dire peril in their admiration of the daring and successful act of seamanship that rescued their more fortunate brothers. Mr. Everard Hayden, of the U.S. Hydrographic Office, has lately issued a preliminary Report on this storm, which, despite the regrettable meagreness of the data at his command, has, nevertheless, a certain scientific interest, inasmuch as less is known of the cyclones of the Pacific than of those of most other tropical seas.

The Apia storm, like the cyclones of the South Indian Ocean, was evidently formed on the northern limits of the south-east trades, and was one of a series that were generated in this region in March 1889. The first of these, in Mr. Hayden's opinion, appears to have originated on the 5th of the month, some 500 miles north-north-east from the Samoan Islands, and to have travelled first in a south-westerly direction, recurving in the latitude of these islands, but at 150 to 200 miles to the west of them, after which it took a south-eastward course between Tonga and Nuië. It seems to have been a storm of great severity, and its passage was felt at Apia on the 6th and 7th, though not with any great intensity. It was succeeded by the cyclone that forms the principal subject of Mr. Hayden's Report. This, he thinks, was formed about March 13, some 300 miles to the north-east of the Samoan Islands, and on the 15th its centre passed either directly over, or a little to the north of, Apia harbour, moving, therefore, on a south-west course. He considers that it then sharply recurved, and that, with greatly increased strength, it passed a second time over Apia on the 16th, the day of the great naval disaster. The chief facts which led Mr. Hayden to this conclusion are those observed at Apia itself, for no positive evidence is forthcoming from the supposed birthplace of the storm, and only one ship reports the state of the weather anywhere to the north of Samoa. The peculiar feature of the Apia observations is, that the barometer fell steadily from the 12th to the afternoon of the 15th (about 0·7 inch), then rose (about 0·25 inch) during the latter part of that day, and then again fell on the 16th to a reading slightly lower than that of the previous day. On the 15th, squalls of moderate force (wind southerly, force 2 to 6) were experienced, and in the after part of the day, as the barometer rose, the direction changed from south to north and east. There had been no heavy sea, and it was thought that the gale was over. At midnight, however, the barometer began

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falling again, the wind had increased, and the sea was high. The barometer continued falling, and the gale rapidly developed its full strength. From early morning of the 16th, for nearly twenty-four hours, it blew a hurricane, and the catastrophes commenced with the loss of the *Eber*.

Any attempted interpretation of facts so meagre must necessarily be in a great measure speculative. We have given that of Mr. Hayden, and others have been suggested. One, that of Lieutenant Witzel, is to the effect that the storm of the 16th was distinct from that of the previous day, and originated over Savaii (the island to the west of Upolu, in which is the harbour of Apia). Another, by Mr. Dutton, is that the storm of the 15th, after approaching the Samoan Islands on a south-west track, recurved to west and north-west, and during the following night again recurved sharply, describing a loop north of Savaii, and then returning towards Upolu, whence it moved southwards and south-eastwards. Our own interpretation is somewhat different from any of these, and seems to be more in accordance with the habits of tropical cyclones, the movements of which are by no means so erratic as that implied by Mr. Dutton's hypothesis, while it does not involve the extremely and, we think, improbably sharp recurvature suggested by Mr. Hayden, nor the equally improbable generation of a second vortex only one day in the rear of the storm of the 15th, as supposed by Lieutenant Witzel. None of these explanations seem to take account of the circumstances that attend the formation of tropical cyclones, which, as we have elsewhere pointed out, differ in many respects from the storms of the temperate zone.

It is evident from the considerable and steady fall of the barometer at Apia from March 12 to 15, that the Samoan Islands lay within the area of disturbance in which the storm was generated, and that the formation of the vortex was simply the concentration of this disturbance, which probably took place nearer to Apia than is supposed by Mr. Hayden, but still at such a distance that the first effect of the concentration—viz. a slight rise of the barometer in the area immediately around, and especially on the polar side—was felt at the Samoan Islands. If, then, as seems probable, the vortex was not formed until the afternoon of the 15th, this, in conjunction with the ordinary diurnal rise between 4 and 10 p.m., would account for the slight rise observed at Apia on the latter part of that day, and only the second fall to a minimum on the 16th was due to the actual passage of the cyclone. From the severity of the storm, as felt at Apia harbour, it is clear that Upolu must have been traversed by at least a portion of the inner vortex, but it could hardly have been very close to the centre, seeing that the barometer never fell to 29 inches; and therefore the long duration of the hurricane (24 hours) can only be explained by the very slow rate at which the storm was then travelling. This slow rate of progression strengthens the probability that it had not proceeded far from its birthplace, since, as a rule, tropical cyclones move forward slowly at first, and only gradually acquire greater speed of translation. It also strengthens the inference that it had originated not very far to the north or north-east of Upolu.

This explanation, as already remarked, can only be regarded as tentative, but it seems at least worthy of consideration by those who may have fuller data at hand. H. F. B.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The Sheepshanks Astronomical Exibition has been awarded to P. H. Cowell, Scholar of Trinity College.

A memorial signed by 107 members of the Senate is published by the Vice-Chancellor: it expresses the opinion that "the whole question of degrees in science should be considered by the University." Among the signatures are some of those who took the Greek as well as some who took the anti-Greek side in the recent controversy.

In view of the fall in the aggregate of the Colleges the Council of the Senate propose to obtain powers for deferring the next increment of the College contribution to the University from 1893 to 1895, and the following increment (from £25,000 to £30,000) for seven years further—namely, to 1903.

Sir George Gabriel Stokes and Prof. Macalister, M.D., are among the delegates appointed to represent the University at the Dublin Tercentenary Festival next year.

Mr. E. W. Hobson, of Christ's College, is approved as Deputy for the Lowndean Professor of Astronomy for the Lent and Easter Terms.

We regret to hear that Prof. Adams's health does not yet allow him to resume his duties.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, November 19.—"The Thermal Emissivity of Thin Wires in Air." By W. E. Ayrton, F.R.S., and H. Kilgour.

In 1884 it was observed experimentally that whereas the electric current required to maintain a *thick* wire of given material, under given conditions, at a given temperature was approximately proportional to the diameter of the wire raised to the power three halves, the current was more nearly proportional to the first power of the diameter if the wire were *thin*. When this difference in the behaviour of a thick and thin wire was first noticed, it was regarded as being quite unexpected. But, as pointed out by one of us in the course of a discussion at a meeting of the Royal Society, the unexpected character of the result was due to people having assumed that the loss of heat from radiation and convection per square centimetre of surface per 1° excess temperature was a constant, and independent of the size and shape of the cooling body.

The very valuable investigations that have been made on emissivity by Mr. Macfarlane, Prof. Tait, Mr. Crookes, Mr. J. T. Bottomley, and by Mr. Schleiermacher, had for their object the determination of the variation of the emissivity with changes of the surface and with change in the density of the gas surrounding the cooling body, but it was not part of these investigations to determine the change in the emissivity that is produced by change in the shape and size of the cooling body. Indeed, so little has been the attention devoted to the very large change that can be brought about in the value of the emissivity by simply changing the dimensions of the cooling body, that in Prof. Everett's very valuable book of "Units and Physical Constants" the absolute results obtained by Mr. Macfarlane are given as the "results of experiments on the loss of heat from blackened and polished copper in air at atmospheric pressure," and no reference is made either to the shape or to the size of the cooling body.

[*November* 19, 1891.—Since this paper was sent in to the Royal Society a new edition of this book has appeared, and, in consequence of a suggestion made to Prof. Everett, the word "balls" has been added after the word "copper" in this new edition, as well as the following paragraph:—

"*Influence of Size.*—According to Prof. Ayrton, who quotes a table in 'Box on Heat,' the coefficient of emission increases as the size of the emitting body diminishes, and for a blackened sphere of radius r centims. may be stated

$$\cdot0004928 + \frac{\cdot0003609}{r}.$$

The value of r in Macfarlane's experiments was 2."]

The laws which govern the loss of heat from very thin cylindrical conductors have not only considerable scientific interest in showing how the shape of a body affects the convection currents, but they are of especial importance to the electrical engineer in connection with glow lamps, hot-wire voltmeters, fuses, &c. We therefore thought it desirable to ascertain the way in which the law of cooling for thick wires, which involved the diameter raised to the power three halves, passed into the law for the cooling of thin wires, involving only the first power of the diameter. For this object, the investigation described in the paper was commenced at the beginning of 1888, and the emissivity was measured of nine platinum wires, having the diameters of 1·2, 2·0, 2·9, 4·0, 6·0, 8·1, 9·3, 11·1, and 14 mils, or thousandths of an inch.

Suspecting that some of the published results on the currents required to fuse wires had been much influenced by the cooling action of the blocks to which the ends of the wires were attached, we started by making a calculation of the length necessary to give to our wires, so that the loss of heat by conduction should not introduce any important error into the determination of the emissivity. To do this it was necessary to calculate the distribution of temperature along a wire through which a steady current was flowing, and from which heat was lost by radiation,

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convection, and conduction, and it was further necessary to improve on the calculation one of us had published on this subject in the *Electrician* for 1879, by taking into account the fact that the emissivity, as well as the thermal and electric conducting power, of the wire differed at different points in consequence of the difference of temperature.

Until we had completed the experiments described in this paper, we could, of course, only employ in this calculation values that we had guessed at as being something near the truth for the emissivity of platinum wire for different diameters and at different temperatures. Hence, after the completion of the experiments, we took up the mathematical investigation again, substituting for the emissivity such a function of the diameter of the wire and the temperature of the point as we had experimentally found it to be. Section IV. of the paper contains the investigation by which we finally arrived at the calculated distribution of temperature along the wire, and we have to express our sincere thanks to Prof. Henrici (whom we consulted as to the best method of practically solving the rather complex differential equation arrived at) for the warm interest that he has taken in the mathematical treatment of the subject, and for the many suggestions which he has made, and which have enabled us to arrive at the mathematical solution given in the paper.

Each wire to be tested was stretched along the axis of a horizontal water-jacketed cylinder 32·5 cm. long, 7·62 cm. external and 5·8 cm. internal diameter, the inner surface of which was blackened and kept at a constant temperature by a stream of cold water flowing through the jacket. The rate at which heat was lost by any one of the wires was measured by the product of the current passing through it into the P.D. (potential difference) maintained between its ends, while the ratio of the P.D. to the current gave the resistance of the wire, and, therefore, its temperature. Experiments were in this way made with various currents flowing through each of the nine wires.

As the variation of resistance with temperature is known to vary with different specimens of platinum, experiments were separately made to determine the actual law of variation of resistance with temperature up to 300° C. for each piece of wire that had been employed in the emissivity experiments.

In this later determination various thermometers were used, and the subsequent comparison of these thermometers with a Kew standard thermometer involved a vast amount of labour, from the fact that it is, or at any rate was not possible three years ago, to purchase from the Kew Observatory a standard thermometer reading from, say, 200° to 300° C., with a short, wide chamber at the base in which the mercury expanded below 200° C. All that could be obtained was a long thermometer which had been carefully tested between 0° and 100° C., and the remainder of whose tube had been simply calibrated for uniformity of bore. The consequence was that when we desired to compare one of our thermometers reading, say, from 200° to 300° C., with the Kew standard, their bulbs were very far apart when both were immersed in the oil-bath, and with the tops of the mercury columns just above the surface of the oil. A short description is given in the paper of the devices employed to overcome this difficulty, and which enabled an accurate comparison to be made between the thermometers.

On examining the curves, accompanying the complete paper, which show the emissivity for each temperature for each of the nine wires, we see that—

(1) For any given temperature the emissivity is the higher the finer the wire.

(2) For each wire the emissivity increases with the temperature, and the rate of increase is the greater the finer the wire. For the finest wire the rate of increase of emissivity with temperature is very striking.

(3) Hence the effect of surface on the total loss of heat (by radiation and convection) per second, per square centimetre, per 1° C. excess temperature, increases as the temperature rises.

On comparing the loss of heat from the wire of 1·2 mils diameter when at 300° C. with that from the wire of 6 mils diameter when at 15° C., both being in an inclosure at 10° C., we see that the former loses per square centimetre of surface per second not

$$\frac{300-10}{15-10}, \text{ or } 58 \text{ times}$$

as much heat as the latter, as it would if the emissivity were the same; but, instead,

$$60 \times 58, \text{ or } 3840 \text{ times}$$

as much heat; arising from the fact that the emissivity—that is, the number of calories (gramme C.°) lost per second, per square centimetre of surface, per 1° C. excess temperature—of the 1·2 mil wire at 300° C. is 60 times as great as that of the 6 mil wire at 15°, the emissivity of the latter wire varying very rapidly near 15° C.

From the curves which accompany the complete paper, each curve giving the variation of emissivity with temperature for a particular wire, the following table has been drawn up, giving the emissivities of the various wires at eight useful temperatures, and it will be observed that, in consequence of our investigation having been made on wires of which the thickest was thinner than the thinnest ever previously used in absolute determinations of emissivity, the emissivities we have experimentally obtained are far greater than any previously arrived at.

Diameter of wire in		Emissivities.							
Mils.	Millimetres.	40° C.	60° C.	80° C.	100° C.	150 C.	200° C.	250° C.	300° C.
1·2	0·0305	0·008230	0·009560	0·010300	0·010846	0·011875	0·012783	0·013625	0·014400
2·0	0·0508	0·005950	0·006860	0·007500	0·007900	0·008600	0·009070	0·009480	0·009850
2·9	0·0737	0·002193	0·003336	0·004086	0·004552	0·005095	0·005379	0·005628	0·005845
6·0	0·1524	0·002460	0·002660	0·002806	0·002930	0·003212	0·003460	0·003666	0·003837
8·1	0·2057	—	—	—	0·002804	0·002939	0·003076	0·003217	0·003352
9·3	0·2362	—	—	—	0·002297	0·002448	0·002586	0·002718	0·002843
11·1	0·2819	—	—	—	0·002053	0·002216	0·002363	0·002490	0·002608
14·0	0·3556	—	—	—	0·001894	0·002027	0·002136	0·002224	0·002286

The wire of 4 mils diameter is omitted from the table, as the experiments showed that its specific resistance was much greater, its temperature coefficient much smaller, and its emissivity much smaller than if it had been of platinum. This piece of wire probably, therefore, contained iridium or silver.

We find that the emissivity of thin platinum wires of different diameters at the same temperature can be very fairly expressed by a constant plus a constant into the reciprocal of the diameter of the wire. For example, we find that

$$\text{At } 100^\circ \text{ C. } e = 0{\cdot}0010360 + 0{\cdot}0120776 d^{-1}, \quad \ldots \quad (1)$$
$$\text{,, } 200 \text{ ,, } e = 0{\cdot}0011113 + 0{\cdot}0143028 d^{-1}, \quad \ldots \quad (2)$$
$$\text{,, } 300 \text{ ,, } e = 0{\cdot}0011353 + 0{\cdot}016084\ d^{-1}, \quad \ldots \quad (3)$$

where d is the diameter of the wire in mils, or thousandths of an inch.

The statement, not unfrequently made, that the current required to maintain a wire of a given material at a given temperature above that of the surrounding envelope is proportional to the diameter of the wire raised to the power three halves, is equivalent to stating that the emissivity is independent of the diameter. Now from the three formulæ (1), (2), (3), given above for e, we may conclude—

That for a temperature of 100° C. the value of d in the formula

$$e = 0{\cdot}0010360 + 0{\cdot}0120776 d^{-1}$$

must be something like 220 mils, or 5·6 mm., in order that the neglect of the second term may not make an error in e of more than 5 per cent., and something like 1·15 inch, or 29·3 mm., if the error is not to exceed 1 per cent.

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That for a temperature of 200° C. the value of d in the formula

$$e = 0{\cdot}0011113 + 0{\cdot}0143028d^{-1}$$

must be something like 244 mils, or 6·2 mm., in order that the neglect of the second term may not make an error in e of more than 5 per cent., and something like 1·28 inches, or 32·5 mm., if the error is not to exceed 1 per cent.

And that for a temperature of 300° C. the value of d in the formula

$$e = 0{\cdot}0011353 + 0{\cdot}016084d^{-1}$$

must be something like 267 mils, or 6·8 mm., in order that the neglect of the second term may not make an error in e of more than 5 per cent., and something like 1·39 inches, or 35·3 mm., if the error is not to exceed 1 per cent.

Generally, then, we may conclude that to assume that the emissivity is a constant for wires whose diameters vary from a small value up to 1 inch is to make a large error in the case of the greater number of the wires, and an error of hundreds per cent. in the case of some of them.

Using the formula (3) which we have arrived at for determining the emissivity of platinum wires of different diameters at 300° C., it follows that to maintain a platinum wire 0·75 mil in diameter at 300° C. would require a current density of 331,000 amperes per square inch; and, if the emissivity of a copper wire of the same diameter and at the same temperature may be taken as being the same, it follows that to maintain a copper wire 0·75 mil in diameter at 300° C. would require a current density of 790,000 amperes per square inch.

November 26.—"A New Mode of Respiration in the Myriapoda." By F. G. Sinclair (formerly F. G. Heathcote), M.A., Fellow of the Cambridge Philosophical Society.

The Scutigeridæ respire by means of a series of organs arranged in the middle dorsal line at the posterior edge of every dorsal scale except the last.

Each organ consists of a slit bounded by four curved ridges, two at the edges of the slit, and two external to the latter. The slit leads into an air sac. From the sac a number of tubes are given off; these tubes are arranged in two semicircular masses. The ends of the tubes project into the pericardium in such a manner that the ends are bathed in the blood and aërate it just before it is returned into the heart by means of the ostia. In the living animal the blood can be seen through the transparent chitin of the dorsal surface surrounding the ends of the tubes; and in the organ and surrounding tissues cut out of a Scutigera directly it is killed the blood corpuscles can be seen clustering round the tube ends. If the mass of tubes of a freshly killed specimen are teased out under the microscope in glycerine, they can be seen to be filled with air. The tubes each branch several times. Each tube is lined with chitin, which is a continuation of the chitin of the exo-skeleton. Each tube is also clothed with cells, which are a continuation of the hypodermis. The tubes end in a blunt point of very delicate chitin.

I therefore hold that the respiratory organ in Scutigera holds a position intermediate between the tracheæ of Myriapods and the lungs of Spiders. I hold with A. Leuckart (*Zeitsch. für Wiss. Zool.*, vol. i. p. 246, 1849, "Ueber den Bau und Bedeutung der sog. Lungen bei den Arachniden") that the tracheæ have developed into the lungs of Spiders and Scorpions, and I think that the organs in question form a series of which the lowest term are the tracheæ, the next the organ of Scutigera, then the lungs of Spiders, and then of Scorpions.

Zoological Society, November 17.—Dr. A. Günther, F.R.S., Vice-President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of October 1891, and called special attention to the following: a young Buffon's Skua (*Stercorarius parasiticus*), captured near Christchurch, Hampshire, and presented by Mr. E. Hart, and a Land-Crab (*Geocarcinus ruricola*) from the island of Fernando de Noronha, brought home and presented by Mr. D. Wilson-Barker.—The Secretary read a letter from Dr. G. Martorelli, of Milan, inclosing a coloured drawing of both sexes of a hybrid Duck bred in the public Garden of Milan, between *Branta rufina* ♂ and *Anas boschas* ♀.—Mr. G. A. Boulenger gave an account of the various forms of the Tadpoles of the European Batrachians, and a statement of the characters by which the different species may be distinguished in this stage of their existence.—A communication was read from Mr. Edgar A. Smith, containing descriptions of new species of Shells from New South Wales, New Guinea, and the Caroline and Solomon Islands, based on specimens lately presented to the British Museum by Mr. John Brazier, of Sydney.—Lord Walsingham gave an account of the Microlepidoptera of the West Indies, based primarily on the collections made in St. Vincent and other islands by Mr. H. H. Smith, under the direction of the joint Committee of the British Association and the Royal Society for the exploration of the Lesser Antilles.—A communication was read from M. E. Simon containing the first portion of an account of the Spiders of the island of St. Vincent, based on specimens obtained under the direction of the same Committee.—A communication was read from Mr. H. Nevill, urging the importance of founding an experimental Zoological Station in the tropics, and advocating the claims of Trincomalee in Ceylon for such an institution.—Dr. Johnson Symington read a paper on the nose, the organ of Jacobson, and the dumb-bell shaped bone in the *Ornithorhynchus*.—Mr. A. Smith-Woodward read a paper on a Mammalian tooth from the Wealden formation of Hastings, being the first trace of a Cretaceous Mammal discovered in Europe. This remarkable fossil the author was inclined to refer provisionally to the genus *Plagiaulax* of the Purbeck Beds, and to call *Plagiaulax dawsoni*, after its discoverer.—A communication was received from Mr. C. Davies Sherborne, giving an exact account of the dates of issue of the parts, plates, and text of Schreber's "Säugethiere." Great difficulties in synonymy had arisen from previously imperfect knowledge of these dates.

December 1.—Mr. Henry Seebohm in the chair.—Mr. Sclater exhibited a specimen of a Shearwater obtained near Sydney, and brought from Australia by Prof. Anderson Stuart. This specimen had been determined by Mr. Salvin to belong to *Puffinus gavia*, a New Zealand species not hitherto known to occur in Australia.—Mr. Seebohm exhibited and made remarks on specimens of several very interesting birds recently obtained in Ireland. Amongst these was an example of the Yellow-browed Warbler (*Phylloscopus superciliosus*) obtained on the Tearaght Rock, the most westerly station in Europe.—Dr. E. Hamilton exhibited a specimen of the Red-breasted Snipe of North America (*Macrorhamphus griseus*) obtained in Scotland.—Mr. W. B. Tegetmeier exhibited some specimens illustrative of the abnormal form of the bill in birds caused by injuries to that organ during life.—Mr. G. A. Boulenger read some notes on specimens of Reptiles from Transcaspia recently received by the British Museum, and pointed out that examples of several well-known Indian species occurred in this collection.—A communication was read from Miss E. M. Sharpe containing the second portion of her descriptions of new Butterflies from British East Africa, collected by Mr. F. J. Jackson during his recent expedition.—Mr. A. D. Michael read a paper upon the association which he had observed between certain Acarines of the family Gamasidæ and certain species of Ants. The author came to the following conclusions: (1) that there is an association between some Gamasids and Ants; (2) that a species of Gamasid usually associates with one or two species of Ant preferentially; (3) that the Gamasids of Ants'-nests are not usually found elsewhere; (4) that the Gamasid abandons the nest if the Ant does; (5) that the Gamasids live upon friendly terms with the Ants; (6) that the Gamasids are not true parasites; (7) that they do not injure the Ants or their young; (8) that the Gamasids will eat dead Ants, and are probably either scavengers or messmates.—A communication was read from Mr. Edward Bartlett containing an account of the specimens of Rhinoceros from Borneo contained in the Museum of Sarawak.—A communication was read from Mr. T. T. Somerville, of Christiania, containing notes on the Lemming (*Myodes lemmus*).

Anthropological Institute, November 24.—E. W. Brabrook, Vice-President, in the chair.—A paper on the perforated stones of South Africa, by H. Mitford Barber, was read.—An account of the Similkameen Indians of British Columbia, by Mrs. S. S. Allison, was read. The tribe at present inhabiting the upper valley of the Similkameen are immediately descended from a small band of the warlike Chilcotins, who established themselves in the upper valley of the river about a hundred and fifty years ago, and intermarried with the Spokans. They have much deteriorated, both physically and mentally, within the last twenty years, and are rapidly becoming extinct. The average stature of the men is about 5 feet 6 inches; their frames are lithe and muscular

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nd their movements quick and graceful. Their complexion is ery light, and they have small hands and feet. The colour of heir hair varies from jet-black to red-brown, and in some cases is almost curly. They are born horsemen and capital shots. The sharp horns of the mountain goat were formerly fixed on hafts of hard wood and used as spears both in hunting and warfare; stone knives and hatchets were also used. The summer lwellings of the Similkameen Indians were made of mats of edar bark, manufactured by the Hope Indians, which were hrown over a circular frame of poles. The winter houses were imply pits dug in the ground and roofed with poles and earth. All sickness was supposed to be the work of an evil spirit, who astened on a victim and hung on, drawing away his life, until harmed away by the doctor, who worked himself into a state of frenzy, singing and dancing while he was trying to lure the evil spirit from his patient. Many of the medicine-men exercise strong mesmeric power over their patients, and they use several herbs as medicines; their panacea for all ills, however, is the vapour-bath. When an Indian died he was laid out in state on a couch of skins; everything put on the body was new; his bow and arrows were laid at his side, along with his knife. His friends then assembled round him to feast, and when the feast was over his friends advanced, and taking his hand bade him farewell. Immediately after a funeral takes place the encampment is moved, lest the spirit of the deceased should revisit it. A widow or widower is forbidden to eat meat and certain vegetables for a month, and must wear quantities of spruce bush inside their shirts, next their skin. Cannibalism was never known among the Similkameens. In the mountain is a certain stone which is much venerated by the Indians, and it is said that striking it will produce rain. Polygamy was allowed, and if the husband and wife tired of each other, the price of the woman, or its equivalent, was returned by her father or guardian, and the parties were then free to contract another matrimonial alliance; but adultery, though it was generally compromised, was sometimes punished by cutting off the woman's nose or splitting her ears. Occasionally sick persons were buried before they were quite dead, and a good deal of infanticide was practised. The author has not found these Indians to be thieves, and gives them a general good character in other respects.

Geological Society, November 25.—Sir Archibald Geikie, F.R.S., President, in the chair.—The following communications were read:—On the os pubis of *Polacanthus Foxi*, by Prof. H. G. Seeley, F.R.S. Hitherto the evidence of the systematic position of *Polacanthus* has not been very precise. The author has detected the missing pubis as an isolated specimen. This he regards as the anterior portion of the left pubis, and appends a full description of the bone. He furthermore gives a critical account of our knowledge of other pelvic bones of the genus, and is led to associate *Agathaumus*, *Crataeomus*, *Omosaurus*, and *Polacanthus* in near alliance, in the Scelidosaurian division of the Order *Ornithischia*.—A comparison of the red rocks of the South Devon coast with those of the Midland and Western Counties, by Prof. Edward Hull, F.R.S. The author believes, with Dr. Irving, that the red rocks of Devonshire are representatives of the Permian and Trias which occupy so large a portion of the district bordering Wales and Salop, and which extend into the Midland Counties, and comments on the remarkable resemblance between the representative beds on either side of the dividing ridge of Palæozoic rocks which underlies East Anglia and emerges beneath the Jurassic strata in Somersetshire. He believes that the breccia forming the base of the series in the Torquay district is a representative of the Lower Permian division, but differs from Dr. Irving, in assigning the red sandstones and marls of Exmouth to the Trias, and not to the Permian as that author has done. He compares them with the Lower Red and Mottled Sandstones, and regards the Marls as of local origin, thus causing the beds to diverge from the normal type. The Budleigh Salterton Pebble-beds, with overlying sandstones and pebbly beds, he assigns to the horizon of the Pebble-beds of the Midland area, and points out that fossils of Silurian and Devonian types occur in the pebbles of both areas. The Upper Division of the Bunter is well shown at Sidmouth, and the author takes a calcareous breccia, two feet thick, which is found in the cliffs, as the basement-bed of the Keuper division.—Supplementary note to the paper on the "Red Rocks of the Devon Coast-section," Q.J.G.S., 1888, by the Rev. A. Irving. In this note the author accepts Prof. Hull's determination (see above) of the breccia at Sidmouth as the base of the Keuper, and discusses the age of the sandstones containing vertebrate remains discovered by Messrs. Whitaker, Metcalfe, and Johnston-Lavis. He brings forward evidence in support of his view that these are really of Upper Bunter age, notwithstanding the character of the organisms. He adds new material in support of his contention that the sandstones and marls which Prof. Hull assigns to the Lower Bunter are really Permian; but he is inclined to think that the breccias (in part, at least) pass laterally into the sandstones, and do not underlie them. From this it follows that the break between the Permian and Trias of Devon is marked by the absence of the Lower Bunter of the Midlands, and the author quotes remarks of Mr. Ussher in support of his view that there is an unconformity at the base of the Pebble-bed. In conclusion the author refers to the difficulties of ascertaining the exact age of the breccias, and notes that we cannot prove that the highest Carboniferous beds are present in Devonshire. He observes that there is no valid reason why the great breccia-sandstone series of Devon should not be the true equivalent of the Lower Rothliegendes both in time and position in the sequence, and that some portions of them may be even older than the Rothliegendes of some districts. He discusses the evidence furnished by the igneous rocks, and points out the abnormal position both for the British and German areas which these would occupy, if the breccias were of Triassic age. The reading of this paper was followed by a discussion, in which Mr. H. B. Woodward, Mr. Hudleston, Mr. Topley, Prof. Boyd Dawkins, the President, Prof. Hull, and the author took part.

December 9.—Sir Archibald Geikie, F.R.S., President, in the chair.—The following communications were read:—On the rocks mapped as Cambrian in Caernarvonshire, by the Rev. J. F. Blake.—High-level Glacial gravels, Gloppa, Cyrn-y-bwch, near Oswestry, by A. C. Nicholson (communicated by W. Shone). These gravels are found at Gloppa, and are situated at a height of from 900 to 1160 feet above sea-level, on the eastern slope of a ridge of Millstone Grit which forms the western border of the Cheshire and Shropshire plain. The beds present the appearance of having been abruptly cut off on the north-eastern slope. The gravels are in places much contorted, and false-bedding is frequent. They contain numerous striated erratics. Amongst the boulders are Silurian grits and argillites, granites like those of Eskdale, Criffel, &c., Carboniferous rocks, Lias shale, and Chalk flints. The shells are often broken, rolled, and striated, but the bulk of them are in fairly good condition. A list of the shells is given, including nine Arctic and Scandinavian forms not now living in British seas, nine northern types, also found in British seas, two southern types, and nearly fifty species of ordinary British forms. Comparative lists of the shells of Moel Tryfan and of those now living in Liverpool Bay are placed side by side with the list of shells from Gloppa. The reading of this paper was followed by a discussion, in which Dr. Hicks, Prof. Hull, Mr. Shone, Prof. Blake, the President, and the author took part. Some remarks sent by Mr. Clement Reid were read by the Secretary.—The subterranean denudation of the Glacial Drift, a probable cause of submerged peat and forest-beds, by W. Shone.

Royal Microscopical Society, November 30.—*Conversazione*.—There was a numerous attendance at this meeting, which passed off very successfully. The following objects and instruments were exhibited:—*Megalotrocha albo-flavicans*, by Mr. F. W. Andrew.—Foraminifera from the London Clay, by Rev. G. Bailey.—*Amphipleura pellucida*, *Arachnodiscus Ehrenbergi*, Polycystinæ from Barbadoes, a microscope with new substage focussing arrangement, by Messrs. R. and J. Beck.—Foraminifera from the Chalk, by Mr. E. T. Browne.—Hyaline Foraminifera from the Folkestone Gault—viz. *Vitriwebbina Sollasi*, n. sp. (Chapman), *V. lævis*, Sol., *Polymorphina Orbignii*, n. var. *cervicornis* (Chapman), by Mr. F. Chapman.—A thickened nodule of *Nitella translucens*, by Mr. E. Dadswell.—*Volvox* and *Batrachospermum* in saturated solution of common salt; diatom structure in medium (Br Ant_1, Br Ars_1, $Piperine_2$), by Mr. J. E. Ingpen.—*Filaria sanguinis hominis* (*diurna* and *nocturna*), prepared by Dr. P. Manson; *Bacillus anthrax* in lung; microscopes with new focussing arrangement to substage, by Messrs. Johnson.—*Epistylis flavicans*, *Lophopus crystallinus*, *Argulus foliaceus*, by Mr. R. Macer.—Transverse section of fertile head of *Equisetum arvense* showing spores and elators in *situ*, section of *Pilia grandiflora* showing reticulate and pitted cells, by Mr. G. E. Mainland.—*Hoplophora carinata* v. *pul-*

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cherrina, a South European mite, by Mr. A. D. Michael.—Photographs and drawings illustrating the absorption of the tubercle and other bacilli by the leucocytes, photographs of micro organisms in dental caries, by Mr. J. H. Mummery.—Exhibition of natural history objects with the projection microscope, monochromatic light apparatus for microscopic work, by Messrs. E. M. Nelson and C. L. Curties.—Phagocytes inclosing tubercle-effusion from dorsal lymph-sac of frog, by Mr. Pound.—*Cherryfield rhomboides* in balsam, with a new $\frac{1}{12}$ apochromatic homogeneous immersion 1·4 N.A., by Messrs. Powell and Lealand.—A collection of different species of Rotifera, by Mr. C. Rousselet.—Photograph of a new apparatus for measuring drawings made with the camera lucida, by Sir Walter Sendall, K.C.M.G.—Petrological slides, transparencies of rock sections, Foraminifera, &c., by Mr. G. F. Smith.—Starch from potato fruit under ¼ inch, with polariscope, by Mr. W. T. Suffolk.—Photographs of *Polura* scales, by the Hon. J. G. Vereker.—Section of passion-flower, by Mr. J. J. Vezey.—Blight of grape vine (*Phylloxera*); *Bacillus mallei* (glanders); Pacinian corpuscles in mesentery of cat, chlorophyll of moss, Diatomaceæ from Jutland, a slide containing 100 species of *Pleurosigma*, by Messrs. Watson.

Entomological Society, December 2.—The Right Hon. Lord Walsingham, F.R.S., Vice-President, in the chair.—Dr. D. Sharp, F.R.S., exhibited and commented on a number of photographs of various species of *Lucanidæ* belonging to M. René Oberthür.—Mr. C. G Barrett exhibited specimens of local forms and varieties of Lepidoptera, taken by Mr. Percy Russ near Sligo, including *Pieris napi*, var. near *'bryoniæ'; Anthocharis cardamines* (male), with the orange blotch edged with yellow, and yellowish forms of the female of the same species; very blue forms of *Polyommatus alsus*; males of *P. alexis*, with the hind margin of the under wings spotted with black, and very handsome forms of the female.—The Rev. S. St. John exhibited two specimens of *Lycæna argiades*, taken in Sonersetshire by Dr. Marsh in 1884; three specimens of *Deilephila euphorbiæ*, bred from larvæ found feeding on *Euphorbia paralias* on the Cornish coast in September, 1889; and a series of various forms of *Anchocelis pistacina*, all taken in a garden at Arundel. Lord Walsingham, F.R.S., Mr. Barrett, and Mr. McLachlan, F.R.S., took part in the discussion which ensued.—Mr. Jenner-Weir exhibited and made remarks on two dark specimens of *Zygæna minos* which had been caught in Carnarvonshire. He remarked that the specimens were not representatives of complete melanism, and suggested that the word "phæism"—from φαιός, dusky—would be a correct word to apply to this and similar departures from the normal coloration of a species.—Mr. C. J. Gahan exhibited specimens of the common "book-louse," *Atropos pulsatoria*, Fabr., which he had heard making a ticking noise similar to that made by the "death-watch" (*Anobium*).—Mr. B. A. Bower exhibited the following rare species of Micro-Lepidoptera: *Spilonota pauperana*, Frol.; *Gelechia osseella*, Stn.; *Chrysoclysta bimaculella*, Haw.; and *Elachista cingilella*, Fisch.—Mr. R. Adkin exhibited a variety of *Anthocharis cardamines*, and one specimen of *Sesia scoliæformis* bred from a larva found at Rannoch.—Mr. G. T. Baker read a paper entitled "Notes on *Lycæna* (recte *Thecla*) *rhymnus*, *tengstrœmii*, and *pretiosa*." A discussion followed, in which Lord Walsingham, Captain Elwes, and Mr. Baker took part.—Mr. F. Merrifield read a paper entitled "The effects of artificial temperature on the colouring of *Vanessa urticæ* and certain other species of Lepidoptera." The author stated that both broods of all three species of *Selenia*, *Platypteryx falcataria*, *Vanessa urticæ*, *Bombyx quercus* and var. *callunæ*, and *Chelonia caja* were affected by temperature in the pupal stage, the lower temperature generally producing the greater intensity and darkness of colour; some of the *Vanessa urticæ* made a near approach to the var. *polaris* of Northern Europe. A long discussion ensued, in which Mr. E. B. Poulton, F.R.S., Prof. Meldola, F.R.S., Mr. Barrett, Mr. Jenner-Weir, and Lord Walsingham took part.—Mr. W. Bateson read a paper entitled "On the variation in the colour of the cocoons of *Eriogaster lanestris* and *Saturnia carpini*," and exhibited a large number of specimens in illustration of the paper. Lord Walsingham congratulated Mr. Bateson on his paper, and on the intelligent care and method shown in his experiments, and said that he was glad to see that at Cambridge there was an entomologist ready to enter this interesting field of investigation, and perhaps at some future day to contest the palm with Mr. Poulton as representing the sister University of Oxford. He had noticed that the larvæ of *S. carpini*, if left in a box with dead food, and probably partially starved, made a light-coloured cocoon; but that when the cocoon was made under natural conditions, on living food-plants on the moors, it was of a dark colour. Mr. Poulton and Prof. Meldola continued the discussion.

Linnean Society, December 3.—Prof. Stewart, President, in the chair.—The President announced the recent bequest by the late Sir George MacLeay, K.C.M.G., of a marble bust of his father, the late Dr. William Sharp MacLeay, formerly a Fellow and Vice-President of the Society.—The President then exhibited a series of specimens of a South American beetle, showing the extremes of variation of colour observable within the limits of a single species.—Mr. J. E. Harting exhibited a photograph of an abnormally situated nest of the chimney swallow (*Hirundo rustica*), which had been built for the second time on a swinging hook in an outhouse; and made some remarks on three recorded cases of swallows nesting in trees, a most unusual habit.—The Botanical Secretary read a paper by Mr. W. West, on the Fresh-water Algæ of the West of Ireland, and exhibited by way of illustration a number of preparations under the microscope, and a series of beautiful drawings by the author. The paper was criticized by Messrs. A. W. Bennett and E. M. Holmes, both of whom testified to the excellence of the work done and the value of the drawings.—The Zoological Secretary next read a paper by Dr. W. H. Strachan, on the tick pest of Jamaica, which was characterized as of so serious a nature as to demand investigation by entomologists, with a view to a remedy. An interesting discussion followed, in which Mr. D. Morris gave a variety of details from personal experience during a residence of some years in Jamaica, and Mr. A. D. Michael pointed out the generic characters of certain West Indian ticks which were likely to include those found in Jamaica by Dr. Strachan. The question of remedy for this plague was discussed by Dr. John Lowe, and Messrs. T. Christy, C. Breeze, and T. J. Briant.

CAMBRIDGE.

Philosophical Society, November 23.—The following communications were made:—The self-induction of two parallel conductors, by Mr. H. M. Macdonald. The well-known expression for the self-induction of two parallel wires (Maxwell, § 685) holds only for the case when neither of them is magnetic. For the case when both wires are magnetic, the value of the coefficient is found, in this paper, in the form of an infinite series. This series can be expressed in finite terms when only one of the wires is magnetic, and then gives

$$L = \tfrac{1}{2}(\mu + \mu_0) + 2\mu_0 \log \frac{b^2}{aa'} + 2\mu_0 \frac{\mu - \mu_0}{\mu + \mu_0} \log \frac{b^2}{b^2 - a^2},$$

where μ_0 is the permeability of the surrounding media (viz. usually unity), a the radius of the magnetic wire of permeability μ, a' the radius of the other wire, and b the distance between their lines of centres. The effect of the magnetic quality is exhibited by means of numerical tables.—The effect of flaws on the strength of materials, by Mr. J. Larmor. The effect of an air-bubble of spherical or cylindrical form in increasing the strains in its neighbourhood was examined; and it was suggested that the results might be of practical service in drawing general conclusions as to the influence of local relaxations of stiffness of other kinds. In particular, a cavity of the form of a narrow circular cylinder, lying parallel to the axis of a shaft under torsion, will double the shear at a certain point of its circumference; and the effect of a spherical cavity will not usually be very different. It is assumed in the analysis that the distance of the cavity from the surface of the shaft is considerable compared with its diameter, so that the influence of that boundary may be left out of account in an approximate solution.—The contacts of certain systems of circles, by Mr. W. McF. Orr.—On liquid jets, by Mr. H. J. Sharpe. The problem is treated by the method of Fourier series.

DUBLIN.

Royal Society, November 18.—Prof. A. C. Haddon, President of the Scientific Section, in the chair.—An analysis of the spectrum of sodium, by Dr. G. Johnstone Stoney, F.R.S. The position of the lines which present themselves in

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the spectrum of hydrogen are given, or approximately given, by Balmer's law, viz.

$$n = k\left(1 - \frac{4}{m^2}\right),$$

where $k = 274{\cdot}263$. In this formula n becomes the oscillation frequency of the successive lines, when for m we write the integer numbers 3, 4, 5, &c. Similarly, Profs. Kayser and Runge have found that A, B, and C can be determined so that the empirical formula,

$$n = A + B\frac{1}{m^2} + C\frac{1}{m^4},$$

shall approximately represent the positions of the lines in any one of the three series that present themselves in the spectra of the other light monad elements—Li, Na, K, Rb, Cs. These formulæ have an important physical meaning. They indicate that n is a function of $1/m$; in other words, that although the periodic times of the successive rays are not themselves a fundamental period with its harmonics, as is the case with the vibrations that give rise to musical sounds, they in some way depend on an event of this simple character which is going on in the molecules from which the spectrum emanates. Balmer's law may be represented by a very simple diagram which places this relationship in evidence. Draw the parabola

$$y^2 = \frac{1}{4k}(k - x),$$

and place its axis horizontal. Erect an ordinate at the distance k from the vertex. Double this out, and using its double length as unit, set off upon it the harmonics 1/3, 1/4, 1/5, &c. From each of the points so determined draw horizontal lines to the curve: these are the values of n for the successive lines of the hydrogen spectrum. Now, having regard to the fact that the light monad elements, H, Li, Na, K, Rb, Cs, have all of them series of lines which appear to belong to the same general type, we are justified in assuming that Balmer's law is the simplest case of a general law which prevails throughout all the light monads. Hence, if the oscillation-frequencies be plotted down as the horizontal lines of a diagram constructed as above with $x = n$ and $y = 1/m$, the curve passing through the ends of the lines in the other monads should be some curve of which the parabola is a particular case. This may happen in different ways, but the simplest hypothesis is that they are hyperbolas or ellipses. Accordingly, the author has tried this hypothesis in the case of the sodium spectrum, with the result that hyperbolas approximately represent series P (the principal series) and series S (the series of sharp lines), and that a parabola represents the third series, series D (the series of diffuse lines); and with the further interesting result that the only line in the sodium spectrum which has not hitherto fallen into its place as a member of one or other of the three series proves to be in reality the first term of series S, with a value for n which is negative instead of positive. The physical meaning of this is that the revolution going on within the molecules round that elliptic partial which gives rise to this double line is in the opposite direction to what it would have been if its n had been positive (see memoir by the author "On Double Lines in Spectra," recently published in the Transactions of the Royal Dublin Society). The equation of an hyperbola being

$$(a - x)^2 = P(b + 1000 \cdot y^2),$$

the values to be attributed to the constants for series P of the sodium spectrum are approximately—

$$\log P = 3{\cdot}7740300$$
$$a = 3337{\cdot}4120$$
$$b = 1438{\cdot}35$$

and their values for series S are—

$$\log P = 2{\cdot}5263843$$
$$a = 434{\cdot}0587$$
$$b = 108{\cdot}514.$$

The equation of a parabola being

$$x = a - 1000 \cdot by^2,$$

the values of the constants for series D are—

$$a = 244{\cdot}93,$$
$$\log b = 0{\cdot}04357.$$

The investigation shows that in series P and series S of the sodium spectrum, the curve of nature is not an exact hyperbola, but a curve slightly less curved in the neighbourhood of its vertex. It also indicates that there is probably a line in the sodium spectrum, belonging to series P, at or a little less than the wavelength 2130.—Mr. J. Joly exhibited and described a shutter for use in stellar photography. This shutter enables any bright star in the field of the telescope to be covered at will, so as to secure better definition. The shutter is a small watch-spring magnet, adjustable to any part of the field, and pivoted so that it can be rotated by the action of a current which circulates round the field in a narrow coil. In one position of the magnet the star is exposed, in the other covered. A modification for parallax work, suggested by Mr. A. A. Rambaut, and used at Dunsink Observatory, has the magnet and coil to one side of the field, and the shutter, which is carried on a needle attached to the magnet, fixed in the centre of the field. There is no vibration in these shutters, owing to the small mass of the moving parts. In the first form, the current in the one coil may control shutters placed in any part of the field of the telescope, so that, if desirable, more than one star may be covered.—Prof. T. Johnson described the structure and function of the peculiar swellings (callosities) of *Nitophyllum versicolor*, Harv., and pointed out the bearing of his observations on the specific character of *N. versicolor*, and Schmitz's views on the structure of the Floridean thallus.—Mr. E. W. L. Holt read a list of the rarer shore and deep-sea fishes obtained during the cruise of the s.s. *Harlequin* on the west coast of Ireland (1891). One fish, *Centrophorus squamosus* (Gm. L.), taken in deep water off the Mayo coast, is new to the British fauna. The following are new to the Irish fauna: *Raia oxyrhynchus* (Linn.), from 500 to 375 fathoms, and from shallow water; *Raia microcellata* (Mont.), from shallow water—coast of Mayo and Donegal; *Rhombus norvegicus* (Gthr.), from shallow water—Donegal Bay; *Arnoglossus grohmanni* (Bonap.) was again taken; *Crystallogobius nilssoni* (Düb. and Kor.) proved to be abundant everywhere, between 10 and 35 fathoms. The following were amongst the forms, usually inhabiting littoral water, which were taken at more than 100 fathoms: *Scyllium canicula, Acanthias vulgaris, Galeus vulgaris, Raia oxyrhynchus, Gadus æglifinus, Conger vulgaris.*

PARIS.

Academy of Sciences, December 7.—M. Duchartre in the chair.—Reply to a note by M. Besson on phosphides of boron, by M. Henri Moissan. The author points out that he remarked upon the reaction between boron and phosphorus in a paper presented on April 6, 1891, and more fully described its properties on July 6, 1891. He therefore claims priority over M. Besson, who first presented a note on the subject on July 13.—On the theory of linear differential equations, by M. André Markoff.—On modifications of the adiabatism of a contracted gaseous stream, by M. H. Parenty.—The vapour tensions of cobalt chloride solutions, by M. Georges Charpy. The graphic representation of the tensions at different temperatures of a solution saturated in the cold (containing 32 per cent. of $CoCl_2$) gives two right lines from 20° to 40°, and from 75° onwards respectively, joined by a curve. Each of these right lines corresponds to a definite state of hydration of the salt; the lower represents the tension of a red solution, the upper of a blue one. These results agree with those of M. Etard, but the interval of passage between the two states is from 40° to 75° instead of from 35° to 50°, as found by this observer, a difference explained by the use of saturated solutions in his experiments.—Action on some metals of sodammonium and potassammonium, by M. Joannis. (See Notes.)—Calculation of the temperature of ebullition of isomeric ethers of the fatty acids, by M. G. Hinrichs.—Thermal data concerning active malic acid and potassium and sodium malates, by M. G. Massol. The heat of solution of the anhydrous acid is (per mol. in 4 litres), − 3·31 Cal.; heats of neutralization—by K = + 26·23 Cal., by Na = + 24·86 Cal.; heats of solution of the anhydrous salts:

$C_4H_5O_5K$ in 6 litres = − 5·78 Cal.
$C_4H_4O_5K_2$ in 8 litres = + 1·55 Cal.
$C_4H_5O_5Na$ in 6 litres = − 1·66 Cal.
$C_4H_4O_5Na_2$ in 8 litres = + 1·78 Cal.

The heats of formation of the salts indicate that malic acid lies between succinic and oxalic acids in the energy of its action.—

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A similar expression, in which r_1 is written for r_2, and r_2 for r_1, gives the value of A_2. The phase angle between the currents is given by

$$\tan(\phi+\psi)=\frac{\left(r_1+\rho\frac{\pi}{2}\right)\left(r_2+\rho\frac{\pi}{2}\right)-\rho^2\left(\frac{\pi}{2}-\alpha\right)^2}{\left(r_1+\rho\frac{\pi}{2}\right)\left(r_2+\rho\frac{\pi}{2}\right)+\rho^2\left(\frac{\pi}{2}-\alpha\right)^2-\frac{\rho\left(\frac{\pi}{2}-\alpha\right)\left(r_1+\rho\frac{\pi}{2}\right)}{\cos\alpha}}\cdot\tan\alpha$$

The expression for ϕ shows that the phase of the current in circuit A_1 is independent of the resistance r_1. On the other hand, varying r_2 alters ϕ. It was also pointed out that $\tan(\phi+\psi)$ is generally greater than $\tan\alpha$.—Prof. J. Perry, F.R.S., read a paper on struts and tie-rods laterally loaded. He pointed out that, in the case of struts, a slight want of straightness may considerably reduce the breaking load. Even if a strut be originally straight and the thrust properly distributed, its weight usually produces lateral loading and consequent bending. Similarly, centrifugal force produces lateral loading in connecting rods. For some years the author has given his students practical examples of struts and tie-rods to work out, taking into account the effect of lateral loads. The chief results obtained, together with a general treatment of the whole subject, are embodied in the paper. Where the curves of bending moment and the deflections due to lateral loading can be easily developed by Fourier's series, solutions can readily be found. Simple cases of uniformly loaded struts and tie-bars have been fully worked out, and also the case of locomotive coupling rods. In one problem on the latter subject, a rectangular cross-section was chosen, and the proportions of depth to breadth determined so as to make the rod equally strong in the two directions when running at various given speeds. With cranks 12 inches long, the results show that, at a speed of 390 revolutions per minute, the ratio of depth to breadth must be infinite, so as to give equal strength, so great is the influence of the lateral loading due to centrifugal force, when combined with the thrust. Horizontal tie-rods loaded by their own weight have been investigated, and the tensions required to neutralize compression due to bending determined. A steel bar, 1 inch diameter and 48 inches long, was used as a strut, with a thrust of 1500 pounds. The maximum stress, due to bending by its own weight alone, was 810, and on applying the thrust the maximum stress was raised to 23,190, or about 26 times that due to lateral loading alone. More complex cases have also been treated, the results of which are given in the paper.

PARIS.

Academy of Sciences, December 14.—M. Duchartre in the chair.—On the distribution of prime numbers, by M. H. Poincaré.—On the fixation of nitrogen by arable soils, by MM. Arm. Gautier and R. Drouin. The conclusion is drawn that only soils containing organic matter fix the free or ammoniacal nitrogen of the atmosphere, even in the absence of plants, and that the organic matter existing in all arable soil is an indispensable intermediary in this fixation of nitrogen.—On the camphoric and isocamphoric esters, and the constitution of the camphoric acids, by M. C. Friedel.—Remarks on the history of supersaturation, by M. Lecoq de Boisbaudran. The author gives some notes, made by him in 1866, on the subject of supersaturation, which are in agreement with the phenomena of solution observed in recent years.—Observations of Borrelly's asteroid (Marseilles, November 27, 1891), made at Paris with the East Tower equatorial, by Mdlle. Klumpke. Observations for position were made on November 30, December 2 and 5.—On integrals of the second degree in problems of mechanics, by M. R. Liouville.—On a class of *congruences* of lines, by M. A. Petot.—On the actual state of geodetic and topographic works in Russia, by General Venukoff.—A brief note on the maps of Russia, prepared under the direction of General Kowersky.—On circular polarization, by M. E. Carvallo.—On a thermo-electric standard of electromotive force, by M. Henri Bagard. The author has experimented with thermo-electrolytic couples consisting of two liquids, one an amalgam of zinc, containing a known proportion of this metal, and the other a solution of sulphate of zinc. He finds that such a couple is absolutely constant between two given temperatures, its electromotive force between 0° and $t°$ being given by the formula—

$$E_0^t = 0{\cdot}001077t + 0{\cdot}00000090t^2.$$

And it is not necessary to exercise any great precision in the determination of the weight of zinc dissolved in the known weight of mercury to form the amalgam, for the variation of the electromotive force when the couple is at the temperatures 0° and 100° appears to be only 0·0001 volt when the proportion of zinc was varied from 0·00025 to 0·00075 the mass of the mercury.—The three basicities of phosphoric acid, by M. Daniel Berthelot. The basicities have been investigated by the author using a method of determining the electric conductibilities of phosphoric acid solution, and of the same with varying quantities of soda, potash, or ammonia respectively added. The conclusion is drawn that monobasic and bibasic phosphates are stable even in dilute solution, and that the tribasic alkaline phosphates are nearly completely dissociated in dilute solution. Phosphoric acid differs completely from the true tribasic acids as the monobasic and bibasic salts of the latter are partially dissociated by water, and the tribasic salts, on the contrary, are stable in solution.—Salts in solution, sodium sulphate and strontium chloride, by M. A. Etard.—A green solid chromic sulphate, by M. A. Recoura. It has the formula $Cr_2(SO_4)_3,11H_2O$. — Bismuthic acid, by M. G. André.—On the distillation of oil, by M. Pierre Mahler.—A new porcelain, asbestos porcelain, by M. F. Garros.—On the presence of reticulated tissue in the muscular walls of the intestines, by M. de Bruyne.—On the first phases in the development of *Crustacea edriophthalma*, by M. Louis Roule.—On *Gymnorhynchus reptans*, Rud., and its migration, by M. R. Moniez.—On the *rôle* of the foot as a prehensile organ in Hindoos, by M. Felix Regnault. Many travellers have remarked on the ability possessed by most Hindoos of using the foot as well as the hand in work of all descriptions. M. Regnault has made some measurements of the lengths of the feet and toes of a number of natives, and draws some conclusions therefrom as to the adaptation "of the organ to the function."—On the discovery of Tertiary shells in the volcanic tufa of Limburg (Grand Duchy of Baden), by M. Bleicher.—The circulation of winds on the surface of the earth: fundamental principles of the new theory, by M. Duponchel.

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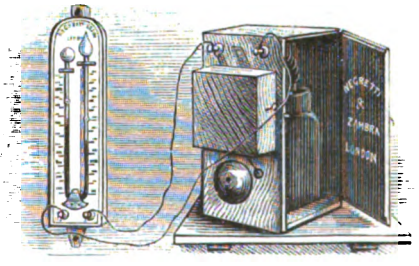

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THURSDAY, FEBRUARY 18, 1892.

A COLLECTION OF MEMOIRS ON PHYSICS.

Collection de Mémoires relatifs à la Physique. Publiés par La Société Française de Physique, tome 1-5. (Paris, Gauthier-Villars.)

LA Société Française de Physique in the volumes before us have initiated a movement which cannot fail to be of the greatest service to students of physics. They are publishing collections of memoirs on one subject written by several authors, instead of following the more usual plan of collecting the papers of one author on a variety of subjects. There can, we think, be no question as to which plan is of most service to the student. The collected papers of one author must from the nature of the case be chiefly used as a work of reference, while the study of a collection of the most important memoirs ought to form an essential part of the reading of every advanced student of the subject. Now that the memoirs in which the foundations of the sciences of electrostatic or electromagnetism, and of investigations with the pendulum, are by the enterprise of the French Society so readily accessible, it is to be hoped that there will be a much greater development of the systematic study of the original memoirs than we are afraid prevails at present.

Science, as Maxwell said, is most easily assimilated when it is in the "nascent" condition, and, moreover, it is to be expected that when a long paper by a master of his subject has been condensed in a text-book to a twentieth of its original length, something of importance must be lost. In a text-book there is as a rule but little room for anything beyond the description of the method which ultimately proved successful, all reference to the difficulties met with, and the way they were overcome, has to be omitted, though these are precisely the points most calculated to induce the student to endeavour to make investigations for himself.

The volumes before us are also of especial interest to the student of science, as they contain papers such as those by Coulomb on electrostatics and magnetism, and by Ampère on electromagnetism, which raised the subject with which they dealt from chaos to order.

The first volume of these collected memoirs is edited by M. Potier, and differs somewhat in character from those which follow, inasmuch as it is entirely devoted to papers by Coulomb. It contains the classical memoirs in which he established by the aid of the torsion-balance the fundamental laws of electrostatics, and of the action between permanent magnets. It also contains a memoir on the loss of electricity by an insulated charged body, in which he comes to the conclusion that there is a leakage of electricity through the air; subsequent experiments have, however, shown that this is erroneous, and that there is no such loss when the air is free from dust and the charged body not under the action of ultra-violet light.

It is remarkable, considering the importance of Coulomb's contributions to electricity and magnetism, that his most important memoirs on these subjects were all published within five years, 1785-1789, when he was between 49 and 53 years of age.

The editor gives as an appendix a very clear account of Poisson's and Sir W. Thomson's investigations of the problem of two electrified spheres. Poisson's papers on this subject are not very accessible, and we wish that they had been included in this volume, as his investigation of this problem is surely one of the most elegant pieces of analysis ever written. It is possible, however, that it was deemed to be too exclusively mathematical to be included in this collection of physical memoirs.

The second and third volumes, edited by M. Joubert, are devoted to electromagnetism. They include Œrsted's paper, published in France in 1820, announcing the discovery of the deflection of a magnet by a current, and the marvellous series of papers published almost weekly by Ampère, in which, in a few months after the publication of Œrsted's discovery, the "Newton of Electricity" established the action of a magnet on a current, and of one current on another, and of the identity of magnets and electric currents. In his earlier papers, those previous to 1822, Ampère seems to have been hampered by the erroneous idea that the force between two small elements of current at right angles to the line joining them was indefinitely small compared with that between the same element at the same distance when forming parts of the same straight line. Instead of being infinitely smaller, it is in reality, as he showed later, just twice as great. He soon, however, corrected this mistake, and in 1822 gave to the world a complete theory of the mechanical forces between currents in a memoir which is reprinted in these volumes, and which was described by Maxwell as "perfect in form and unassailable in accuracy."

In addition to the papers we have mentioned, there are many others dealing with points of the greatest interest: thus we find Œrsted, in his paper on the action of a current on a magnet, suggesting that light may be an electrical phenomenon, and Ampère writing in favour of this suggestion. In the second volume we have a paper by Faraday on electromagnetic solution; two papers by Davy, in one of which he describes a curious heaping up of a layer of mercury over the places where a strong current enters and leaves the mercury. This pretty effect is due to the mechanical force between the current in the leads and the current through the layer of mercury. This paper also contains Barlow's description of his electromagnetic wheel, and two papers, not hitherto published, by Fresnel, on Ampère's theory of magnets. Fresnel appears to have been the author of the suggestion that the currents by which Ampère explained the magnetic properties flowed round the molecules of the iron. In the third volume Weber's great paper on electrodynamic measurements appropriately completes the series of papers on electromagnetism, as the measurements made by the methods it develops afford a complete verification of the theory given in the preceding pages by Ampère.

The fourth and fifth volumes, edited by M. Wolf, contain memoirs on pendulum experiments. They commence with a well-written historical account of such experiments. Next we have a bibliography extending over 216 pages, and then follows a series of admirably selected papers of which the names speak for themselves. On

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pendulum experiments pure and simple, we have memoirs by De la Condamine, Borda and Cassini, Prony, Kater, Bessel, Sabine, and Baily, and in addition to these we have Stokes's paper on the effects of fluid friction on the motion of pendulums.

We must, in conclusion, express our gratitude to La Société Française de Physique for the publication of these volumes, and to MM. Potier, Joubert, and Wolf, for the masterly way in which they have edited them. We hope that the volumes before us are but the first terms in an infinite series. J. J. THOMSON.

THE FORMATION OF BEACHES.

Sul Regime delle Spiaggie, e sulla Regolazione dei Porti. ("On the Formation of Beaches, and the Rules for designing Harbour Works.") By Signor P. Cornaglia, late Inspector of the Royal Corps of Italian Civil Engineers. (Turin: R^a Tipografia Paravia, 1891.)

THE laws that govern the movements of sand along the sea-shores, the formation and corrosion of beaches, the shifting of bars and sand-banks at the mouth of rivers, and the silting up of harbours, have not yet been clearly explained, as the problem is a complex one. These effects result from a combination of various causes, such as the action of sea-waves, of tidal currents, of the natural discharge of rivers, and of the dimensions and specific gravity of the materials that form the beaches and give them a special angle of rest.

To examine this problem, Signor Cornaglia takes it up in its most simple form. He tries to ascertain the laws that regulate the propagation and effects of sea-waves acting in a tideless sea, or nearly so, as is the Mediterranean. These researches are made both theoretically and practically, by the help of mathematical analysis and by verifying the results of theory by direct observations of waves in accumulating the materials that form the beaches. These form the subject of two memoirs, in which are discussed the vertical propagation of waves in liquids, and the origin and action of bottom-waves (*flutti di fondo*) in liquids in a state of undulation.

The results of these researches are thus expressed:—(*a*) The undulatory movement of liquids generates near the bottom an oscillatory movement called *bottom-wave*, or under wave (*flutto di fondo*), which is alternately directed to or from the shore; (*b*) vertically under the crest of a superficial wave, the bottom-wave is directed toward the shore; under the hollow of a wave it is directed toward the sea; (*c*) the force of these bottom-waves increases with the force of the superficial waves, with the greater distance or "*fetch*" from which the waves arrive, and with the greater depth of the sea; (*d*) the energy of these bottom-waves may be great at great depths; (*e*) on a rising submarine slope the force of the bottom-waves directed toward the shore is greater than that of the reverse bottom-waves; (*f*) the bodies resting upon the bottom of the sea and exposed to these bottom-waves are struck in alternate directions; (*g*) the component of the weight of these bodies, parallel to the bottom slope, may counterbalance the effects of the direct bottom-waves, or, added to the effect of the return bottom-waves, these latter may prevail upon the direct ones; (*h*) the line along which the action of each of the two opposite bottom-waves, combined with the component of the weight of the bodies, counterbalances that of the other, is called the *neutral line*; (*i*) all conditions being equal, this *neutral line* is situated at a greater depth the stronger the waves are, and the smaller the slope of the bottom and the size and specific weight of the bodies resting upon the bottom; (*j*) in the Mediterranean this *neutral line* is situated at depths varying from 8 to 10 metres, or 27 to 33 feet; (*k*) on the land side of the neutral line the materials forming the bottom of the sea are pushed by the bottom-waves toward the shore, on the sea side they are drawn toward the greater depths; (*l*) parallel to the shore the materials travel always in a contrary direction to that from which the waves come, and they travel alternately in one way or the other according to the direction of the waves. However, the ultimate direction in which these materials move is that corresponding to the prevailing waves, which is also, more or less, that of the prevailing winds.

In a third memoir the author discusses the formation or corrosion of beaches in a tideless sea, and explains a long series of observations and experiments made principally by himself along the shores of the Riviera, with a view of ascertaining the position of the neutral line.

In a fourth memoir are examined the conditions of the estuary of a river opening into a tideless sea, and the effects due solely to the waves having been previously ascertained, the effects due to the outgoing water of the river are described. Then the author passes on to examine the case of a river opening into a sea subject to the influence of tide and tidal currents; and by separating the effects of the waves and of the outgoing water of the river proper, he tries to explain the effects due exclusively to the action of tides. From this he gives some hints about the probable position of the neutral line resulting from all these different causes, and by comparing the conditions of the Mersey with those of the Thames and other tidal rivers, the author concludes that in the former case the outlet of the river is on the land side of the neutral line, and thus the materials brought down by the river or washed away from the coast are pushed into the estuary, while in the case of the Thames its outlet is on the sea side of the corresponding neutral line, and the materials are drawn into deeper water. In the same way he explains why some harbours in the Mediterranean having their entrance in depths of water superior to 10 metres, and thus outside the neutral line, have kept good for centuries, while other harbours with their entrance inside the neutral line have gradually silted up.

Having thus explained his views on the formation of beaches, Signor Cornaglia makes use of them in laying down in several memoirs the principal rules for designing such maritime works as may be necessary to maintain or improve the navigable channel in the estuary of a river, or to protect the entrance of a harbour, or to prevent the sea from encroaching upon a beach. There are descriptions of works carried out in several Italian harbours during this century, and the results, good or bad, according to the way these works were designed.

A drawback to this book is that it is written in the form of separate memoirs, so that there are many repetitions and some unnecessary details, which rather diminish than

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add to the clearness of the work. Taken as a whole, however, the book will be read with interest and advantage both by the maritime engineer and by the geologist.

EGYPTIAN HISTORY.

Egypt under the Pharaohs. By H. Brugsch-Bey. A New Edition, condensed and thoroughly revised by M. Brodrick. (London : John Murray, 1891.)

THE science of Egyptology is scarcely seventy-five years old, and published formulated statements of the history of Egypt derived from the comparatively newly-acquired decipherment of hieroglyphics are not yet thirty years old ; books on Egyptian history are now so common that the general reader has yet difficulty in deciding which of those available is best for him to read or study. Dr. Birch's "Egypt from the Earliest Times to B.C. 300" printed, we believe, in 1875, is without doubt, the best of all the small histories of Egypt which have ever been written : the facts are to be depended upon, few alterations are necessary, difficulties are not slurred over, and the whole subject is there treated with the breadth of view and learning only to be found in such a scholar. More concise, but written in the same admirable style, is the "Aperçu" of Mariette, which aimed at presenting in a small compass the principal facts of Egyptian history to the visitors to the Exhibition in Paris in 1867; for the general history of Egypt and the relations of her people with foreign nations in the various epochs of her national life, the reader would naturally consult Maspero's "Histoire Ancienne des Peuples de L'Orient," and Lenormant's "Histoire Ancienne de l'Orient." The best examples of scientific histories of Egypt are those of Dümichen and Wiedemann. In the "Aegyptische Geschichte" of the latter scholar, published at Gotha in 1884, the author not only sets before the general reader or student the statements of certain facts, but gives in foot-notes the authorities for the statements, so that his work may easily be controlled.

This was an important step in advance, and has done more to convince people generally that the decipherment of the Egyptian inscriptions has been productive of important results than can be directly estimated. Long, however, before any of the above-mentioned works was written, or probably planned, Dr. Brugsch, the greatest of all living Egyptologists, so far back as 1859, published the first part of a history of Egypt which was to begin with the earliest monument, and to end with our own days; this part was entitled "L'Egypte sous les Rois Indigènes," and was published with 19 plates in quarto, but the two other parts, which were announced to contain the history of Egypt under the Greeks and Romans, and under the Arabs, seem never to have appeared. In 1876 Dr. Brugsch published his "Geschichte Aegyptens unter den Pharaonen," in two volumes, with maps and tables ; and in this work he dealt with the history of Egypt as comprised in the thirty dynasties, beginning with the first historical king, Menes, and ending with Nectanebus. The narrative was written in fine German, and nearly every important event in the history of Egypt, as then known, he described by translating the hieroglyphic inscription which referred to it. These translations read easily, and, on the whole, represented very well the sense of the Egyptian inscriptions in the words of a modern language. In 1877 Mr. John Murray published an English translation of this work entitled "A History of Egypt under the Pharaohs," with Maps, by H. Brugsch ; the translation was the joint work of Danby Seymour and Philip Smith, and although it was on the whole good, it was only too evident that on certain points of Egyptology the translators had no special knowledge, while the beauty of Dr. Brugsch's style was, of course, lost in the process of translation. In 1881, Mr. Murray issued a second edition of the work, which, in addition to a new preface by Dr. Brugsch, contained a number of notes by Philip Smith, which were helpful to the reader, and several alterations and corrections by the author himself ; the thirty-two pages of additions and notes were also most useful. During the past ten years great strides have been made in the science of Egyptology, notwithstanding the loss, by death, of Birch and Lepsius, the fathers of Egyptology ; and general investigations into the history and language of Egypt have resulted in the discovery of a host of new facts, many of which have an important bearing upon the received ideas on these subjects. That a new edition of Brugsch's "Aegypten," or of its English translation is called for, is not to be wondered at, and no one has shown himself more sensible of this need than Mr. Murray himself. We venture to submit, however, that any attempt to "condense" or "thoroughly revise" the work by anyone except Dr. Brugsch or some competent hand was a mistake. The "condensed" edition of "Egypt under the Pharaohs" now before us contains 450 pages of text 8vo., and three maps ; all Philip Smith's notes, which, as we before said, were useful to the general reader for whom this book is intended, are deleted, Brugsch's article on the Exodus, the additions and notes, the transliteration and translation of the stele of Usertsen III. have been omitted, and the originally full index has been cut down. Each page contains five lines more than the second English edition, hence we necessarily expect the "condensed" edition by Miss Brodrick to be a smaller book ; but seeing also that by cutting out clauses and important adjectives, &c., &c., she has succeeded in putting 49 pages of the second English edition into 18 of the condensed edition, we do not understand the statement made in the second paragraph of her preface that nothing has been omitted except the Essay on the Exodus and the transliteration, &c., of the Tablet of Usertsen.

We have no doubt that Macaulay's "History of England" could be condensed to one-fifth of its present size by cutting out all that is explanatory of facts ; but what should we gain by this mutilation ? Miss Brodrick's condensation has been so vigorous that Brugsch's explanation of the word "Hyksos," which occupies fifty-seven lines in the second English edition, occupies only eight in hers ; and where Brugsch gives two references to one event described in the Bible, Miss Brodrick omits the one which refers to the fuller narrative (p. 375, note 4). "The much-vexed question of the nationality of the Bubastites has, so far as possible, been accommodated to Brugsch-Bey's present views" by omitting whole paragraphs which occur in the second English edition.

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In the condensed edition the cartouches of Egyptian kings which stood at the head of the chapters in the second English edition have been placed at the beginning of the book, and Miss Brodrick has added five pages of matter on the Dêr el-Bahari mummies.

We have long hoped that Dr. Brugsch would issue a new edition of his "Aegypten unter den Pharaonen," revising his facts in some places, and correcting his statements in others, and also adding the new facts relating to the periods between the VII.-XIth and XIII.-XVIIth Dynasties, which have recently come to light; failing this, which is much to be desired, we hoped that one of his pupils would do the work under his guidance. That, however, the English translation made by Seymour and Smith, mutilated and robbed of its notes, and of the additions of the author, should be issued as a popular text-book of Egyptian history under Brugsch's name is a fact which we deplore.

OUR BOOK SHELF.

The Story of the Hills: a Popular Account of Mountains, and how they were made. By the Rev. H. N. Hutchinson, B.A., F.G.S. (London: Seeley and Co., 1892.)

THIS is a pleasant, chatty book, all the more welcome because wholly unpretentious; not too deep for "human nature's daily food" when roaming among the hills of which it treats. It will be read with pleasure and profit by the tourist, who likes to know just enough about the sundry points of interest connected with the scene of his wanderings to make the enjoyment of his outing intelligent, but who is not haunted by a feverish anxiety to be for ever, in season and out of season, improving his mind. Many who would shrink from a formal scientific treatise with horror or disgust will find themselves able to enjoy this book, and through its channel scraps of useful knowledge may insinuate themselves into their minds which would never have found their way there by any other road.

Part I. is multifarious, and touches on a vast variety of matters more or less connected with mountains, and principally of human interest—mountain races, mountain legends, the uses of mountains to mankind, mountain storms, avalanches, and the plants and animals of mountains. Scientific explanations of facts and phenomena are interspersed: the severe critic may detect a little vagueness and looseness here and there in these, but no very serious lapse. Well-chosen quotations from Ruskin and other authors give brilliancy to the narrative. There are landscape views reproduced from photographs, which have all the excellences and the artistic failings of this class of illustration.

Part II. is mainly taken up with a geological history of mountains. Here all the main geological truths that bear on the subject are expounded clearly, and with great fullness of detail. In fact, an epitome is given of a large number of the leading doctrines of geology, which will suffice for the needs of many a general reader. A separate chapter is devoted to volcanic mountains and volcanic activity. We may note that the three stages in the life of a volcano mentioned on p. 266 are not such as are usually defined by geologists. A. H. G.

The Optics of Photography and Photographic Lenses. By J. Traill Taylor. (London: Whittaker and Co., 1892.)

ALTHOUGH photography is so widely practised at the present day, it is surprising how little is known by amateurs about the principles that underlie the construction of photographic lenses.

The present work will serve as an excellent guide to those who wish to gain this information, and should be found to be of great practical use. The author has dealt with the subject in a very popular manner, and although the mathematics is reduced to a minimum, he has made his meaning very clear throughout.

In the first few chapters the nature and properties of light are discussed, together with explanations of photographic definition, single and achromatic lenses, cause of the inverted image, spherical aberration, nature and function of diaphragms, nature and cure of distortion, optical centres of single and combination lenses, &c. Chapters xi. to xv. treat solely of lenses, including accounts of the non-distorting, wide-angle, portrait, landscape, copying, and universal lenses. As there are thirty-nine chapters in all, we may mention that of those remaining there are many on subjects which may be of special interest to individual readers. Thus we have a chapter dealing with photo-telescopic lenses, a short one on the grinding of lenses, and another on enlarging and projecting in relation to lantern optics.

It will be seen that the author has dealt with a wide range of subjects in which the lens makes its appearance, and the reader will find that the explanations are lucid, while the illustrations bring out the points which they are intended to show with equal clearness.

W.

The Evolution of Life; or, Causes of Change in Animal Forms. A Study in Biology. By Hubbard Winslow Mitchell, M.D. (New York and London: G. P. Putnam's Sons, 1891.)

DR. MITCHELL says in the preface to this book that he has accomplished in it "all that can be reasonably expected from a medical man deeply immersed in the duties of his profession." What most people expect from medical men in this position is that they will not write books on vast and complicated subjects, for the proper treatment of which an author must have not only exceptional ability but ample opportunities for philosophic study. So far as we have examined the work, it has neither freshness of thought nor charm of style. Dr. Mitchell mentions that he has travelled in many different parts of the world. If he was determined to write a book, he would have been better employed in recording his reminiscences as a traveller than in tediously discussing questions which have occupied so many of the foremost intellects of the present age.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

A Magnetic Disturbance.

OUR attention having been directed for some days past towards a spot of unusual size upon the sun's disk, we were not by any means surprised to observe, as doubtless many of your readers elsewhere also did, an aurora of great beauty on Saturday night last; nor was our anticipation of seeing a magnetic disturbance portrayed upon the magnetograph records disappointed in the morning, for when the sheets were changed and the photographs developed, we saw that perturbations more violent than any which had been recorded at Kew for the past ten years had been in progress since about 5.45 a.m. of February 13.

The magnets were very quiet on Friday, but early on Saturday morning they became disturbed. The easterly declination

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slightly increased until about 5.40 p.m., whilst both horizontal and vertical forces similarly increased in intensity, more especially between 4 and 6 p.m. They further diminished in force after 10 p.m., and their changes became very rapid from 12 midnight to 2 a.m., whilst at the same time the declination proceeded to its extreme westerly position. Subsequently, the fluctuations in magnetism became much reduced in extent, and the whole disturbance gradually diminished and died out about 4 p.m. of Sunday.

The Kew magnetometers were not able to record the complete extent of the vibrations to which free needles were subjected, nor could the entire change of force be secured in the field of the instrument. The limits, however, clearly recorded were 2° of declination from ·1760 to ·1830 of horizontal force, and from ·4350 to ·4420 units of vertical force expressed in C.G.S. measure in absolute force.

G. M. WHIPPLE,
Superintendent.

Kew Observatory, Richmond, Surrey, February 16.

The New Star in Auriga.

PROF. COPELAND has suggested to me that as I am the writer of the anonymous postcard mentioned by you a fortnight ago (p. 325), I should tell your readers what I know about the Nova.

It was visible as a star of the fifth magnitude certainly for two or three days, very probably even for a week, before Prof. Copeland received my postcard. I am almost certain that at two o'clock on the morning of Sunday, the 24th ult., I saw a fifth magnitude star making a very large obtuse angle with β Tauri and χ Aurigæ, and I am positive that I saw it at least twice subsequently during that week. Unfortunately, I mistook it on each occasion for 26 Aurigæ, merely remarking to myself that 26 was a much brighter star than I used to think it. It was only on the morning of Sunday, the 31st ult., that I satisfied myself that it was a strange body. On each occasion of my seeing it, it was slightly brighter than χ. How long before the 24th ult. it was visible to the naked eye I cannot tell, as it was many months since I had looked minutely at that region of the heavens.

You might also allow me to state for the benefit of your readers that my case is one that can afford encouragement to even the humblest of amateurs. My knowledge of the technicalities of astronomy is, unfortunately, of the meagrest description; and all the means at my disposal on the morning of the 31st ult., when I made sure that a strange body was present in the sky, were Klein's "Star Atlas," and a small pocket telescope which magnifies ten times.

THOMAS D. ANDERSON.

21 East Claremont Street, Edinburgh, February 13.

Nacreous Clouds.

IN the morning of the 30th ult. there was a magnificent display of the nacreous (or iridescent, as they were first called) clouds, which formed such a striking feature of the sunset and sunrise sky for some days in succession in December 1884 and 1885 (vol. xxxi. pp. 148, 192, 316, 360, &c.). They were not exactly the same in appearance, but I should say they were of the same nature. I had not seen them in the interval of six years, and have only noticed them lately on the one day mentioned. They were confined to the southern part of the sky. As the sun rose higher their colours were less visible, and the clouds disappeared about noon; though in the afternoon some reappeared, but never became very striking. At 5h. 44m. G.M.T. there was only one group, which was too far from the sun to show any nacreous colours; its centre was about at hour-angle 1h. 2m. west, and declination 23½° south. Although conspicuous they were no longer very bright, and I should say the sun was evidently not shining on them, for they were the same bluish-green colour as the western sky, and I apprehend were illuminated by the sky.

T. W. BACKHOUSE.

Sunderland, February 9.

The Cause of an Ice Age.

IN his very kindly review Prof. Darwin thinks I might have stated my argument with more completeness if I had preserved its generality by the use of a symbol instead of taking a special case.

No doubt in many ways the treatment he suggests would have been better. It would, for instance, have enabled me to prove the case *a fortiori*. Perhaps, however, the reasons given in the chapter explaining "why the book has been written" may show that for the object I had in view the method actually used was appropriate.

I am also much obliged to the same friend for pointing out that the astronomical theorem proved in the appendix had been given by Wiener, "Über die Stärke der Bestrahlung" (*Zeitschrift der Oesterreichischen Gesellschaft für Meteorologie*, vol. xiv., 1879, p. 129).

ROBERT S. BALL.

Observatory, Co. Dublin, February 9.

Ice Crystals.

WITH reference to the letter on the subject of ice crystals which appeared in NATURE of the 4th inst. (p. 319), it is perhaps worth mentioning that a paper on the subject, entitled "Eine Eiskrystallgrotte," by C. A. Hering, appeared in Groth's *Zeitschrift für Krystallographie und Mineralogie*, Band xiv. (1888), pp. 250–253, and Plate vi.

The crystals occurred in an old mine on the Waschgang near Döllach in Carinthia. Large fans, as much as 300 mm. long × 200 mm. broad, of ice-crystals grow out horizontally from the vertical walls. The stalk, consisting of a series of hexagonal prisms, hollow, like thermometer-tubes, was in the middle 25 mm. thick and thickened towards the point of attachment to the rock. The fan surface was a large hexagonal plate with strong prismatic ribs running from the centre to the angles. The interspaces between the ribs were filled by prisms arranged with the greatest regularity. Upon the ribs of the fan either single crystals or funnel-shaped structures with step-like sides consisting of prisms were borne. The individual crystals were almost all thick tabular forms, with prism, basal pinacoid, and rhombohedral faces.

BERNARD HOBSON.

Owens College, Manchester, February 8.

A Rare British Earthworm.

IN the summer of 1890, during my researches into the Vermes of Cumberland, I discovered a species of earthworm which proved to be new to Britain (*Lumbricus Eiseni*, Levinsen). As I have recently had the good fortune to receive specimens of the same worm from another part of the country, it seems desirable to place the same on record. A correspondent writes from Gloucestershire as follows:—

"Last Saturday (January 30, 1892), I walked up to one of my favourite woods here on the Cotswolds, about 700 feet above the sea—a damp old beech wood, the Frith Wood of Withering's "Arrangement," seventh edition, 1830—and seeing a stump of some 10 inches diameter with a growth of the black 'Candle Snuff Fungus' on it, I examined the rotten wood, which gave way to the pokes of my stick. Among this rotten wood I saw some earthworms, two or three of which I inclose, hoping they may prove an addition to our worm fauna."

I have placed on record all the known earthworms of Gloucestershire in *The Field Club* for 1891, to which this may now be added. The worms were small, but in good form for identification, and prove to be specimens of Eisen's worm. I have, unfortunately, been unable hitherto to consult Levinsen's original description; nor have I been able to obtain Rosa's memoir published in the *Boll. Mus. Zool.*, Torino, 1889 (vol. iv., No. 71). I am therefore obliged to content myself with a description of the specimens in my possession.

Lumbricus Eiseni, Lev., as found in Britain, is a small species of earthworm, measuring about 1½ inches in length when adult. It has the usual colour of the allied species—the purple and red worms—being of a ruddy hue, with iridescence. The clitellum or girdle, which occupies segments (24) 25 to 31, is a reddish-brown, being lighter in colour than the anterior portion of the worm's body on the dorsal surface. Ventrally the worm is, as usual, of a lighter shade. No *tubercula pubertatis* have been seen under the girdle, but the first dorsal pore in every specimen examined is clearly detected behind the 5th segment. This may be indicated by the fractional sign $\frac{5}{6}$; and as the most recent researches tend to demonstrate the constancy of this character for each species of earthworm, it is important to note the same. The lip or prostomium has the complete mortise and

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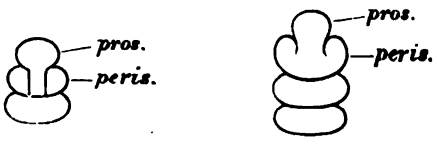
tenon arrangement which distinguishes Lumbricus from Allolobophora.



The male pore is situated normally on segment 15, but as the papillæ which carry the pores are large, they extend over the adjoining segments on either side. Earthworms vary greatly in this respect. Rosa says that spermathecæ are absent in this species, a peculiarity which has been noted in worms belonging to several other genera. I have not sufficient material to enable me to confirm or dispute this statement at present. I have counted the segments of three specimens, and found them to be in each instance 106. As the year advances I hope to be able to obtain mature adults for dissection, when it will be possible to give a detailed account of the internal anatomy. Meanwhile the external characters are amply sufficient for distinguishing the worm if the girdle is properly developed, as its nearest British ally (*Lumbricus purpureus*, Eisen) has the clitellum on segments 28 to 33.

HILDERIC FRIEND.

Idle, Bradford.

The Implications of Science.

WILL you allow me to say something in answer to Mr. Dixon's letter on this subject in NATURE of January 21 (p. 272)?

(1) I admit that there is a verbal or symbolic "convention" if two (or more) persons agree to understand any given words or symbols in a way arbitrarily chosen by themselves. But the scope of such convention is exceedingly limited: if people wish to be understood, or even to understand themselves, they must use the same words as others use, and use them in the same sense (except in an infinitesimal proportion of cases). If it is said that the common application and use of current words is a mere convention, the word *convention* is taken in an extremely strained and metaphorical sense, since nothing like an explicit agreement has ever been made. The "convention" as to the use of language is as fictitious as the *social contract* of Locke and Rousseau. But in the one case, as in the other, there is a solid basis of facts, to suit which the hypothesis has been produced. Language has been moulded by thought and feeling, which, in their turn, have been impressed by facts; and it is facts and relations of facts that language seeks to express. As Mill says (in the first chapter of his "Logic") names are a clue to things, and bring before us "all the distinctions which have been recognized not by a single inquirer but by all inquirers taken together." No one, I imagine, would say that a *particular* case of the impossibility of affirming and denying a given statement, depends "solely on the law of contradiction"; but in the case of any particular assertion, the impossibility, *in that case*, is seen, and to a mind that has reached the generalizing stage, the universal is discernible in the particular. As regards the question of "real propositions," I will not occupy space with quotations, but will only refer to Mr. Dixon's letter of December 10, in which the passages occur which led me to think that he regarded assertions (or denials) of the existence of particular objects as the only "real" propositions.

(2) As regards induction, I agree with Mr. Dixon that the starting point in induction is hypothesis or discovery. But with reference to the rest of the procedure, and its relation to so-called "formal" logic, I differ from him. For I think that an inductive generalization may be set out syllogistically; *e.g.*,

What has once produced X will always produce X;
A has once produced X;
∴ A will always produce X (= all A is X).

If space allowed, I should like to consider the justification for the major premiss, and also to say something about the grounds on which the minor (which indicates the hypothesis or discovery) asserts causation [or concomitance] in a given instance.

(3) Mr. Dixon says: "We do not, in mathematics, conclude a universal proposition from a single concrete instance." But it appears to me that, as far as my own experience goes, in every concrete mathematical proposition which I understand this is exactly what happens; and I do not see how, on Mr. Dixon's view, mathematical formulæ could ever have been constructed. "A mathematical formula," Mr. Dixon remarks, "does not imply the existence of any instance whatever of its application, any more than a definition implies the reality of the thing defined." But if a definition is always of a *thing*, what more is wanted? The definition is admitted to be of *something*; and what is *something* must, I suppose, exist *somehow*.

(4) I still think that in the passage in Mr. Dixon's letter which I referred to under (4) he is not consistent. For if, as he asserts, the definition of *four* as = 1+1+1, makes it false to say that *Twice two are four*, this is surely because the *facts* referred to by *four* are no longer what they were when the statement in question was true. If definitions were purely arbitrary, as Mr. Dixon holds, what would prevent my saying that *Four* (1+1+1) *means twice two* (1 + 1) + (1 + 1)? It is surely only the reference to *things* which makes it absurd—(and, however *four* (4) may be defined, how is *one* (1) to be understood, except by reference to things?).

That words and symbols used intelligibly do, and must, refer to something beyond themselves, seems to me indisputable. If they did not, no assertion of the form *S is P* could ever be made, for the symbol S is certainly *not* the symbol P. And for *any* statement, of the form *S is P*, to be possible and significant, it is further necessary that S and P should have *identical* application, but *diverse* signification. If application *and* signification were the same, we should get *S is S* and *P is P*; if application were *not* the same, we must say, S is *not* P. Hence, no term can ever be taken in *mere* denotation (or application), nor in *mere* connotation (signification); but both *momenta* of each term have to be taken into account in every assertion. If to take a case given by Mr. Dixon in his "Essay on Reasoning," p. 8) we "define" *metal* as "the list of *denotation*, iron, copper, tin, zinc, lead, gold, and silver," then *iron*, &c., can only be pointed out by taking some specimen of iron, and saying, *This and all other things which are* LIKE *it in certain respects*. An absolutely arbitrary denotation can be given only if the *whole* of the objects denoted are severally pointed out; and even then, unless they are labelled, they can only be remembered and identified by means of their *characteristics*; if labelled, by *that* characteristic.

Mr. Dixon objects to my attributing to him the view that "mathematical truths in as far as 'real' are obtained by induction, and are therefore not necessary." But in his letter of December 10 he says:—"For example, the assertion 'Two straight lines cannot inclose a space' is certainly not a 'necessary truth.' Either its terms are defined by connotation, so that its truth depends solely on those definitions, or else its terms are defined by denotation, as representing real things in space; *and the truth of the assertion can only be proved by induction from actual experience with those things*. In the first case, the truth is arbitrary, not necessary; and in the second case it might conceivably be false, as was shown by Helmholtz." It was this passage which led me to the opinion which I expressed.

Cambridge, January 31. E. E. C. JONES.

Vacuum Tubes and Electric Oscillations.

I HAVE not had the advantage of hearing the lecture of M. Nikola Tesla nor of seeing his experiments, but it does not seem out of place to recall the attention of your readers to an article by Dr. Dragoumis in your issue for April 4, 1889, in vol. xxxix. p. 548.

OLIVER J. LODGE.

THE NEW STAR IN AURIGA.

SINCE our last article was written the weather has continued very bad for astronomical observations. The only new results obtained which have reached us consist of a paper read by Mr. Norman Lockyer at the Royal Society on Thursday last, and an important telegram from Prof. Pickering, which appeared in Wednesday's *Standard*.

We will take these in order. Mr. Lockyer's communication to the Royal Society was dated February 8; it stated that two more photographs, containing many more lines than the former ones, were taken on Sunday night, February 7, and it went on to make the important announcement that "The bright lines K, H, *h*, and *G* are

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accompanied by dark lines on their more refrangible sides."

This was substantially the substance of the telegram which appeared in the *Standard* on the following Wednesday (February 10), with the additional remark that the Harvard astronomers thought it possible that the phenomena presented by the new star had been caused by the collision of two celestial bodies.

On the next day the detailed observations made on Sunday night at Kensington, together with the approximate wave-lengths of the lines measured on the photographs, were sent on by Mr. Lockyer to the Royal Society. From these we learn that the Nova on Sunday appeared to be slightly brighter than on February 3.

With the 10-inch refractor and Maclean spectroscope, C was seen to be very brilliant, and there were four very conspicuous lines in the green. Several fainter lines were also seen, and a dark line was suspected in the orange. Mr. Lockyer noticed that some of the lines, especially the bright one near F, on the less refrangible side, appeared to change rapidly in relative brightness, and this was confirmed by Mr. Fowler.

Observations of the spectrum were made by Mr. Fowler with the 3-foot reflector and the Hilger 3-prism spectroscope. These call for no special remark.

Twenty bright lines have been measured on the photographs, and their wave-lengths are given in the accompanying table:—

Lines in the spectrum of Nova Aurigæ.

1st Photo. Date, February 3. λ obtained by means of a curve.	2nd Photo. Date, February 3. λ obtained by means of a curve.	3rd Photo. Date, February 7. λ obtained by means of a curve.	3rd Photo. λ obtained by means of a direct comparison with lines in α Cygni.	Bright-line stars.	Orion stars (dark lines).	Nebula in Orion (bright line).
K 3933	3933	3933	3933	—	3933	3933
H 3968	3968	3968	3968	3970	3968	3968
h 4101	4101	4101	4101	4101	4101	4101
4128	4130	4127	4128	—	4130	4130
4172	4172	4172	4172	—	4172	—
—	—	—	4202	4200	—	4200
4226	4227	4228	4226	—	—	4226
4268	4268	—	4264	—	4268	4268
—	—	4294	4291	—	—	—
4312	4310	4310	4310	—	—	—
G 4340	4340	4340	4340	4340	4340	4340
—	—	—	4383	—	—	4383
—	—	—	4412	—	4415	4410
—	—	—	4434	—	—	—
—	—	—	4469	4472	4472	4472
4516	4516	4522	4518	4510	—	—
4552	4552	4554	4555	4550	—	—
4587	4587	4584	4587	—	—	—
4618	4618	4625	4625	4620	—	—
—	—	4860	4860 F	4860	4860	4860

The table also shows probable coincidences with the lines in the spectra of the Wolf-Rayet stars as photographed by Prof. Pickering, dark lines in Orion stars photographed at Kensington, and bright lines in the Orion nebula photographed at Mr. Lockyer's observatory at Westgate.

In addition to the lines recorded in the table, the photographs in the spectrum of the Nova show several lines more refrangible than K. They probably include some of the ultra-violet hydrogen lines.

All the lines in the spectrum of the Nova are broad, although in a photograph of the spectrum of Arcturus, taken with the same instrumental conditions, the lines were perfectly sharp. It is also important to note that the broadening of the lines is not accompanied by any falling off of intensity at the edges, as in the case of the hydrogen lines in such a star as Sirius. With the method employed in taking the photographs, long exposures are liable to result in a thickening of all the lines on account of atmospheric tremors. The lines would also be thick if the Nova be hazy. In the photograph, however, all the lines are not equally thick.

If the lines are similarly broadened when a slit spectroscope is employed, the effect must be due to internal agitations, for if different regions of the Nova are moving with varying velocity, or with the same velocity in different directions, a normally fine line might be widened in the manner observed in the photographs.

With regard to the bright and dark lines the paper states as follows:—

"A somewhat similar phenomenon has already been recorded by Prof. Pickering in the case of β Lyræ, and this has been confirmed by a series of photographs taken at Kensington. In this case the bright lines are alternately more or less refrangible than the dark ones, with a period probably corresponding to the known period of variation in the light of the star. The maximum relative velocity indicated is stated by Prof. Pickering as approximately 300 English miles per second.

"In the case of Nova Aurigæ, the dark lines in all four photographs taken at Kensington are more refrangible than the bright ones, so that as yet there is no evidence of revolution.

"The relative velocity indicated by the displacement of the dark lines with respect to the bright ones appears to be over rather than under 500 miles per second. The reduction is not yet complete.

"Should the photographs which may be obtained in the future continue to show the dark lines displaced to the more refrangible side of the bright ones, it will be a valuable confirmation of my hypothesis as to the causes which produce a new star—namely, the collision of two meteor-swarms On this supposition the spectrum of Nova Aurigæ would suggest that a moderately dense swarm is now moving towards the earth with a great velocity and is disturbed by a sparser one which is receding. The great agitations set up in the dense swarm would produce the dark-line spectrum, while the sparser swarm would give the bright lines."

ELECTRODYNAMIC THEORIES AND THE ELECTROMAGNETIC THEORY OF LIGHT.[1]

IN a former article we endeavoured to give an account of the first part of M. Poincaré's "Électricité et Optique," in which he dealt with the electric and magnetic theories expounded in Maxwell's treatise. In Part II. he now compares the theory of electromagnetic action given by Maxwell with the somewhat more general theory put forward by Helmholtz in his celebrated paper on the equations of motion of electricity (*Pogg. Ann.*, cii. p. 529, or *Wissensch. Abhand.*, vol. i.); discusses the condition which must hold in order that the two theories may coincide; and, after a masterly exposition of the various consequences which flow from Maxwell's theory, finishes with a very valuable analysis of the theoretical and experimental work of Hertz.

In the first chapter M. Poincaré deals with the formula of Ampère for the mutual action of two current elements. The method adopted is founded on the following three principles assumed from Ampère's experiments:—

(1) That a current in a conductor may be replaced by an equal current in a sinuous conductor nowhere deviating from the first by a distance comparable with the distance of the latter from any element of the other conductor acted upon.

1 "Électricité et Optique." II. Les Théories de Helmholtz et les Expériences de Hertz. Par H. Poincaré, Membre de l'Institut. (Paris: Georges Carré, 1891.)

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(2) The action of a closed circuit carrying a current upon any current element is normal to the element.

(3) The action of a closed (non-varying) solenoid upon a current element is zero.

It is besides assumed that the action of a circuit upon a current element is the sum, in the dynamical sense, of the individual actions of the elements of the circuit; and that the action between two elements is a force in the straight line joining their centres.

The process used for the deduction of Ampère's formula from these premisses is very elegant. If ds, ds' be the lengths of the two elements, γ, γ' the currents in them, ϵ the angle between the elements, θ, θ' the angles they make with the line joining their centres, the action of ds on ds' may be represented by $f(r, \theta, \theta', \epsilon)\gamma\gamma' ds ds'$. But the action of ds may, by the first principle stated above, be replaced by the actions, of its components dx, dy, dz; so that

$$f = A\frac{dx}{ds} + B\frac{dy}{ds} + C\frac{dz}{ds},$$

where A, B, C are coefficients. Now, f depends upon $r, \theta, \theta', \epsilon$; r and θ' do not depend on the direction cosines of ds; $\cos\theta$ and $\cos\epsilon$ are linear and homogeneous with respect to these direction-cosines. Hence f must be linear and homogeneous with respect to $\cos\theta$ and $\cos\epsilon$, that is with respect to dr/ds, and $d^2r/ds ds'$. Similarly, f is linear and homogeneous with respect to dr/ds', $d^2r/ds ds'$. Hence we have

$$f = \psi(r)\frac{dr}{ds}\frac{dr}{ds'} + 2\phi(r)\frac{d^2r}{ds ds'},$$

where $\psi(r)$ and $\phi(r)$ are functions of r.

These functions are determined by the second and third fundamental principles. The second gives $\psi(r) = \phi'(r)$, so that the problem is reduced to the determination of $\phi(r)$. This value of $\psi(r)$, however, permits f to be written in the form

$$2d\mathrm{U}/dr \,.\, d^2\mathrm{U}/ds ds',$$

where U is a function of r only, and

$$d\mathrm{U}/dr = \sqrt{\phi(r)}.$$

From this it is then shown that, if T be the so-called electrodynamic potential (electrokinetic energy) of the circuits—that is, the function the space variation of which, for any direction, is the force in that direction between the circuits—

$$\mathrm{T} = \int\int \frac{d\mathrm{U}}{ds}\frac{d\mathrm{U}}{ds'} ds ds',$$

the currents being each unity, and the integrals being taken round the circuits.

The determination of U is then effected by means of the third principle. It is first shown that T may be written as the integral of $Fdx + Gdy + Hdz$ round the circuit to which ds belongs, F denoting the integral round the other circuit of $\mathrm{U}'^2 ds' dr/ds' \,.\, (x - x')/r$, and G, H similar expressions. F, G, H are, in this theory, what Maxwell has called the components of vector potential. These values of F, G, H, it is to be remarked, fulfil the relation

$$d\mathrm{F}/dx + d\mathrm{G}/dy + d\mathrm{H}/dz \,(= \mathrm{J}) = 0.$$

By applying the third principle it is proved that, if ∇^2 have its ordinary signification, and $f'(r) = \mathrm{U}'^2/r$, $(\mathrm{U}' = d\mathrm{U}/dr)$, $\nabla^2 f(r)$ must be a constant, in order that the action of a closed non-varying solenoid on a complete circuit may be zero. Since $f(r)$ must be zero at infinity, this gives $f(r) = k/r$; and if the ordinary electromagnetic definition of unit current be taken, k must be unity, so that $\mathrm{U}' = \pm 1/\sqrt{r}$. Hence the attraction between the elements is

$$2\gamma\gamma' ds ds' \frac{1}{r^2}(\cos\epsilon - \tfrac{3}{2}\cos\theta\cos\theta'),$$

Ampère's well-known expression.

The above expressions for F, G, H reduce easily to $\int f(r)dx'$, $\int f(r)dy'$, $\int f(r)dz'$, so that, putting in the value of $f(r)$, we get the well-known value of the mutual energy of the two circuits—

$$\mathrm{T} = \int\int \frac{\cos\epsilon}{r} ds ds'.$$

The theory of induction is next taken up. After a short discussion of some objections made by M. Bertrand to the received method of deducing the laws of induction from the observed facts of electromagnetism, M. Poincaré proceeds to show that the electrokinetic energy of two currents is equal to the electrodynamic potential, and recalls Maxwell's application of Lagrange's dynamical equations to the theory of inductive action. He then deals at some length with the celebrated theory put forward by Weber for the action between two quantities, e, e', of electricity, as depending on their distance apart and their motion.

This we pass over, with the remark that Poincaré here discusses certain difficulties to which the theory leads in connection with the value it gives for the action between two current elements, and concludes with a short analysis of Maxwell's examination of the theory of induction as deduced from Weber's law. According to Maxwell ("El. and Mag.," vol. ii. p. 445, second edition), Weber's theory gives, for the inductive electromotive force exerted by the circuit in which the current γ flows on the other, the equation—

$$\mathrm{E} = \frac{d}{dt}\int\int \gamma\frac{1}{r}\frac{dr}{ds}\frac{dr}{ds'} ds ds',$$

which, for a closed circuit, agrees with the well-known equation—

$$\mathrm{E} = -\frac{d}{dt}\int\int \gamma\frac{\cos\epsilon}{r} ds ds.$$

M. Poincaré points out that this apparent agreement of the two theories is due to the fact that Maxwell has overlooked certain terms which contribute to the value of E, and which do not give a zero result when integrated round a closed circuit.

The expressions given by Weber and Neumann for the mutual potential of two current elements are next considered, and shown to be included in the general expression given for the same potential by Helmholtz. By means of this expression Helmholtz's general electrodynamic theory is introduced, and then follows an elaborate comparison of the theories of Helmholtz and Maxwell. It is shown that Helmholtz's theory leads to the value of T for conduction in three dimensions given by the equation—

$$\mathrm{T} = \tfrac{1}{2}\int(\mathrm{F}u + \mathrm{G}v + \mathrm{H}w)d\varpi,$$

where $d\varpi$ is an element of volume, u, v, w the components of currents, and the integral is extended throughout all space. F, G, H, are, of course, the components of vector potential, and in this theory are given by equations—

$$\mathrm{F} = \int \gamma' dx'/r + \tfrac{1}{2}(1 - k)d\psi/dx, \text{ \&c.},$$

where

$$\psi = \int \gamma' ds' \,.\, dr/ds'.$$

If ρ be the density of free electricity at any point,

$$du/dx + dv/dy + dw/dz = -d\rho/dt,$$

and this, when instead of $\gamma' dr/ds'$ is substituted its value in terms of u', v', w', gives, by an application of Green's theorem, the result—

$$\psi = \int r d\rho'/dt \,.\, d\varpi',$$

where $d\varpi'$ is an element of the space in which the current

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γ' is flowing. Now, if in the insulating dielectric two like quantities of electricity, e, e', would produce a repulsive force of magnitude $ee'/\lambda r^2$, the electrostatic potential ϕ, due to the free electricity of density ρ', is given by the equation—

$$\lambda\phi = \int \rho' d\varpi'/r.$$

Thus, from the foregoing values of F, G, H, three equations are obtained, viz.,

$$\nabla^2 F = -4\pi u + (1 - k)\lambda d^2\phi/dxdt,$$

with two similar equations for $\nabla^2 G$ and $\nabla^2 H$. These give

$$dF/dx + dG/dy + dH/dz = -k\lambda d\phi/dt,$$

which is zero if k or λ be zero.

If, now, the magnetic inductive capacity be taken for the moment as unity, and the magnetic force (α, β, γ) be so defined that T is γ times the magnetic induction through the circuit in which the current whose numerical magnitude is γ flows, we deduce easily the equations—

$$\alpha = dH/dy - dG/dz, \text{ \&c.};$$

so that there follow

$$\frac{d\gamma}{dy} - \frac{d\beta}{dz} = 4\pi u - \lambda \frac{d^2\phi}{dxdt},$$

&c., &c.

These coincide with the equations of currents given by Maxwell when the last terms are omitted. We must therefore either put $\lambda = 0$, or (since $d\phi/dt$ is not in general zero) $\phi = 0$ to reduce to Maxwell's theory.

Poincaré next proves that unless the k in Helmholtz's theory be not less than zero, the sum of the electrostatic and electrokinetic energies may diminish indefinitely from an infinitely small value, so that there would be unstable equilibrium. This affords another reason for rejecting the theory of Weber, in which $k = -1$.

In chapter v. Poincaré passes from the theory of Helmholtz to that of Maxwell. He first considers magnetic and dielectric polarization according to a modified and corrected version of the theory of Poisson. He supposes the dielectric space filled with conducting particles separated by other material, the dielectric proper, which completely insulates these bodies from one another. These conducting bodies are supposed to be electrically polarized, so that electric displacement (f, g, h) is set up in the medium. A parallel theory of magnetic polarization is considered, and the electric displacement is simply the electric analogue of the intensity of magnetization of the medium—that is, the magnetic moment at each point per unit of volume. If ϵ be the ratio of the volume occupied by the conducting particles to the whole volume of the dielectric space in which they are embedded, the specific inductive capacity, K, of the medium is found to be $\lambda\phi(\epsilon)$, where $\phi(\epsilon)$ is a function analogous to $(1 + 2\epsilon)/(1 - \epsilon)$ (the corresponding function for the case in which the conducting particles are spheres) in that it becomes infinite when $\epsilon = 1$.

According to Poincaré, λ is the specific inductive capacity of the dielectric medium proper, or insulator between these conducting bodies, and is very small. In order, therefore, that K may be finite, it is necessary that ϵ may be very nearly equal to unity.

The electrostatic potential, ϕ, at any point is that due to the free electricity present on conductors, and to the electricity developed throughout the medium by its polarization. The electrification, in fact, consists of two parts—a volume density on the dielectric depending on the electric displacement, and of amount $-(df/dx + dg/dy + dh/dz)$, and a resultant surface density, $\sigma' = \sigma - (lf + mg + nh)$, where σ is the surface density of the electricity present in the form of charges on conductors, and $lf + mg + nh$ (in which l, m, n are the direction cosines of the normal to the surface directed inwards towards the dielectric) is the surface distribution due to the polarization. By speaking of this as the electrification, what is meant is that this electrification existing in the dielectric medium proper would give the observed potential, ϕ, at each point. Thus

$$\phi = \frac{1}{\lambda}\left\{\int \frac{d\varpi}{r} + \int \sigma' \frac{dS}{r}\right\}.$$

The value of the electrostatic energy, U, according to this dielectric theory, is given by

$$U = \frac{\lambda}{8\pi}\int \Sigma\left(\frac{d\phi}{dx}\right)^2 d\varpi + \frac{2\pi}{K - \lambda}\int \Sigma f^2 d\varpi.$$

If there are applied electromotive forces, X, Y, Z, their values are given by

$$X = -d\phi/dx - 4\pi f/(K - \lambda),$$

with similar equations for Y and Z. Hence, if X, Y, Z=0—that is, if there is nothing but electrostatic action—the electrostatic energy is given by

$$U = \int \frac{K}{8\pi} \Sigma\left(\frac{d\phi}{dx}\right)^2 d\varpi.$$

If X, Y, Z are not zero, the electrostatic energy becomes $2\pi/K.\int \Sigma(f^2) d\varpi$ (which is Maxwell's expression), provided $\lambda = 0$. We have to inquire what reasons can be adduced for putting $\lambda = 0$ in this theory.

M. Poincaré shows that the velocity of propagation of a wave of longitudinal displacement in Helmholtz's theory is $\sqrt{K}/(K - \lambda)k\lambda$. On the other hand, the velocity of propagation of a wave of transverse displacement is $\sqrt{1/\mu(K - \lambda)}$. Thus there will be no longitudinal wave if one of the conditions, $k = 0$, $\lambda = 0$, $K = \lambda$, hold. The last condition would make the velocity of propagation of waves of transverse displacement infinite, and must be rejected for every medium, even so-called vacuum, in which light is propagated. Poincaré adopts the second hypothesis.

Further, if to λ a positive value sensibly greater than zero be assigned, a wave of transverse displacement will be given a velocity sensibly greater than that of light. Thus, to pass to Maxwell's theory, it is necessary to make λ insensible, as this gives for the velocity of waves of transverse displacement his value $1/\sqrt{\mu K}$, which is known by experiment to be the velocity of light.

The adoption of this value of λ leads to all the electromagnetic equations of Maxwell, and to the conditions $J = 0$, $du/dx + dv/dy + dw/dz = 0$, the last of which expresses that electricity considered as the analogue of a fluid is incompressible—that is, that all currents flowing are closed currents.

It may be pointed out here that this conclusion agrees with that arrived at by Mr. R. T. Glazebrook in his comparison of Maxwell's electromagnetic equations with those of Helmholtz and Lorentz, and in his further paper on the general equations of the electromagnetic field (Proc. Camb. Phil. Soc., vol. v., Part ii., 1884). Mr. Glazebrook's result is that Φ, the electrostatic potential of Helmholtz's paper must be zero everywhere in order to pass from Helmholtz's theory to that of Maxwell. But Φ is what Poincaré has denoted by $\lambda\phi$, so that the conditions are the same.

It has been attempted to make the transition to Maxwell's theory by putting $k = 0$. This would not suffice alone, as Poincaré points out. For, while giving at once $J = 0$, it would fail to give Maxwell's velocity for transverse waves unless further $\lambda = 0$, which by itself would suffice to effect the transition irrespective of the value of k.

Poincaré thus supposes, in opposition to the older dielectric theories, that even vacuum, or the ether of the inter-planetary spaces, consists of polarizable conducting matter embedded in an insulating medium of infinitely small inductive capacity, λ. According to

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Mossotti's theory, which is the starting-point of all mathematical theories of polarization, the conducting particles are spherical, and therefore, if ϵ be the ratio of the volume of the spheres to the total volume of the medium, the value of K is $(1 + 2\epsilon)/(1 - \epsilon)$. It is here assumed that the specific inductive capacity of the insulating dielectric is unity. Poincaré, however, sees no reason for making this particular assumption, and takes it as λ, a quantity which, if Maxwell's theory be the true one, must be exceedingly small. This involves, as already stated, $K = \lambda\phi(\epsilon)$, where $\phi(\epsilon)$ is a quantity which becomes very great when $\epsilon = 1$. Thus, according to Maxwell's theory, the conducting particles are separated by infinitely thin insulating partitions, so that they practically fill the whole space. Of course, the physical fact may be very different from that here supposed: the theory only furnishes a picture, not perhaps altogether clear and intelligible, of the structure of the medium and its functions.

It may be said that the infinitely small inductive capacity, λ, of the medium, itself requires physical explanation. This is quite true; but so also does the specific inductive capacity equal to unity assumed for vacuum or air in the ordinary theories. In fact, such dielectric theories as have been put forward, involving merely polarization of the medium, only give an explanation of the difference between the electric behaviour of one medium and another, and furnish none whatever of the real *rationale* of the propagation of electric action.

That the value of ϕ may be finite, it is necessary that the values of the volume density, ρ, and the surface density, σ', may be infinitely small, since

$$\phi = \int \frac{\rho}{\lambda r}\, d\varpi + \int \frac{\sigma}{\lambda r}\, dS.$$

Here ρ is the volume density due to the surface distributions on the opposite faces of the partitions between the conducting particles, and this, it is easy to see, will be infinitely small. Also, σ' is the sum of the actual density (surface density of charge) on the surface of the conductors, and the density, which is the surface manifestation of the polarization of the conducting particles, or $\sigma' = \sigma - (lf + mg + nh)$. This also can be conceived as exceedingly small, so that ϕ may have a finite value.

Further reasons for preferring the theory of Maxwell are discussed in chapter vi., which is entitled "The Unity of Electric Force." This chapter consists of an exposition of Hertz's modification of Maxwell's electromagnetic theory—a modification, it is to be remarked, practically given also, but in vector form, by Mr Oliver Heaviside, in various papers in the *Philosophical Magazine*. When made, it exhibits a striking parallelism between the equations of electric and magnetic force, and leads to some remarkable theorems. Using Maxwell's equations, and deviating slightly from Poincaré's mode of presenting the equations, we have, if k now denote electric conductivity of the medium, and P, Q, R components of electric force—

$$\left(k + \frac{K}{4\pi}\frac{d}{dt}\right)P = \frac{1}{4\pi}\left(\frac{d\gamma}{dy} - \frac{d\beta}{dz}\right),$$

with two similar equations for Q and R. But also we have—

$$\frac{\mu}{4\pi}\frac{d\alpha}{dt} = -\frac{1}{4\pi}\left(\frac{dR}{dy} - \frac{dQ}{dz}\right),$$

with two similar equations for β and γ. These last may, by the introduction of a non-existent quantity, g, be written—

$$\left(g + \frac{\mu}{4\pi}\frac{d}{dt}\right)\alpha = -\frac{1}{4\pi}\left(\frac{dR}{dy} - \frac{dQ}{dz}\right),$$

&c., &c.

The quantity g, Heaviside points out, is the proper magnetic analogue to k, and may therefore be called the magnetic conductivity. Its reciprocal would be the true magnetic resistivity of the medium. Of course, in an insulating medium k is also zero.

According to Maxwell's theory, P, Q, R, α, β, γ fulfil the equations—

$$\frac{dP}{dx} + \frac{dQ}{dy} + \frac{dR}{dz} = 0,$$

$$\frac{d\alpha}{dx} + \frac{d\beta}{dy} + \frac{d\gamma}{dz} = 0;$$

the first, since $du/dx + dv/dy + dw/dz = 0$, and the second because the magnetic force in the medium, being supposed purely inductive, must fulfil the solenoidal condition, except at the (vortex) origin of the disturbance.

There is therefore, in Maxwell's theory, a perfect reciprocity of relation between the electric and magnetic quantities. Hence we might infer, from the magnetic phenomena following from electric currents or flow of electricity, an analogous set of electric phenomena following from the flow of magnetism. Now we know that if a magnet varies in strength it produces an electromotive force of components P, Q, R, at every point of the surrounding space. This we may suppose due to a current of magnetism flowing from one end of the magnet to the other, and thus producing the variation in the magnet's strength. The directions of the components of electric force at any point are in fact coincident with those of the components of vector potential produced by the magnet at that point, and are equal to the time-rates of variation of these components. But why should this not be regarded as an electrostatic field in the ordinary sense of the term? For example, a current of electricity, flowing round a closed circuit, produces a magnetic field equivalent to that which would be produced by a magnetic shell of proper strength, and having its edge coincident with the circuit. Of this current a closed solenoid varying continuously in magnetic strength (for example, a closed solenoid in which the magnetizing current is varying in strength) is the magnetic analogue, and ought in the same way to be equivalent to an *electric shell*, in the sense of producing an identical electric field. Such a shell ought to be subject in an electric field to dynamical action; and further, two such varying solenoids ought to exert the same mutual dynamical action, as would the two equivalent electric shells if placed in the same configuration. The second of these conclusions asserts that the dynamical action on such a shell depends only on the electric field in which it is placed, and that its action on the other varying solenoid is due to its producing exactly the same electric field as the equivalent electric shell would produce. This is what Poincaré gives as Hertz's principle of the unity of electric force.

Of course, it is to be noticed that the second conclusion does not follow from the first. We cannot reason that because the mutual action of an electric shell and a varying solenoid is the same as that of two electric shells, therefore the mutual action of two solenoids is the same as that of two electric shells.

If, however, we assume that the dynamical action on a closed varying solenoid depends only on the electric field in which it is placed, we can say that the mutual action of two varying solenoids is the same as that of their equivalent electric shells.

M. Poincaré calculates the work done in effecting a relative displacement of two such varying solenoids, and finds that it is equal to the change in the electrostatic energy of the system, as the change in the electrokinetic energy is all accounted for otherwise. Now, the electrostatic energy of a system is given as we have seen by the equation—

$$U = \int \left\{ \frac{\lambda}{8\pi} \Sigma\left(\frac{d\phi}{dx}\right)^2 + \frac{2\pi}{K - \lambda} \Sigma(f^2) \right\} d\varpi,$$

where

$$f = -(K - \lambda)/4\pi \cdot dF/dt + d\phi/dx.$$

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This may be written

$$U = \int \left\{ \frac{\lambda}{8\pi} \Sigma \left(\frac{d\phi}{dx} \right)^2 + \frac{K - \lambda}{8\pi} \Sigma \left(\frac{d\phi}{dx} + \frac{dF}{dt} \right)^2 \right\} d\varpi.$$

Now, if there be no inductive action—that is, if the field be wholly electrostatic—

$$dF/dt,\ dG/dt,\ dH/dt = 0,$$

and hence, for two electric shells—

$$U = \int \frac{K}{8\pi} \Sigma \left(\frac{d\phi}{dx} \right)^2 d\varpi.$$

On the other hand, if the action be wholly inductive—that is, if we have no so-called electrostatic action—

$$d\phi/dx,\ d\phi/dy,\ d\phi/dz = 0,$$

and for two varying solenoids we have—

$$U' = \int \frac{K - \lambda}{8\pi} \Sigma \left(\frac{dF}{dt} \right)^2 d\varpi.$$

If, then, there be the same mutual action between the two varying solenoids as between their equivalent electric shells, we must have $U = U'$. But because of the equivalence the value of dF/dt, &c., produced at any point by either solenoid, must be the same as those of $d\phi/dx$, &c., produced at the same point when the solenoid is replaced by its equivalent electric shell. Thus we get $U/U' = K/(K - \lambda)$, and therefore $\lambda = 0$. Thus, if the principle of the unity of electric force is true, $\lambda = 0$, and we have Maxwell's theory.

Dr. Oliver Lodge has, as is well known, endeavoured to detect the existence of an electrostatic field produced by varying magnetic action (NATURE, May 23, 1889, and *Electrician*, May 17, 1889), and has reason to believe that he has been successful. It is also possible, as Poincaré suggests, that this kind of electrostatic action may be developed when iron rings, &c., are placed in the field of an alternating electromagnet, as in the experiments of Elihu Thomson.

In a note, which forms a supplement to the comparison of the theories of Helmholtz and Maxwell, M. Poincaré points out that when the mutual action of a varying solenoid and an electric shell is considered, contradictory results are obtained according as the solenoid is regarded as fixed and the shell movable, or the shell fixed and the solenoid movable. Thus the theory of Helmholtz in this application does not give fulfilment of the third law of motion.

Possibly, some such theory as this may throw some light on the electric phenomena of voltaic cells, with their finite steps of potential across the surfaces of separation of dissimilar substances, and help to refer the production of all currents to the single cause—electromagnetic action.

We come now to the discussion which the book contains of the experiments of Hertz. This fills considerably more than one-half of the work, and we cannot, in the space left at our disposal, give an adequate account of it. Of the experiments themselves it is not necessary to say anything, as they have been fully and ably discussed in NATURE by Mr. Trouton (February 21, 1889). Hertz's own theory of the radiation of electric and magnetic energy has also been given in these pages by Dr. Lodge (February 21, 1889, *et seq.*). Poincaré's presentment of the theory is, however, marked by many points of originality, and abounds in acute and interesting remarks.

The theory of the dumb-bell exciter used by Hertz is first considered, then the field produced is discussed, and, last of all, the action of the resonator or receiver is dealt with. Taking the exciter as a couple of spheres, 15 cms. in radius, placed with their centres 150 cms. apart, and joined by a wire ½ cm. in diameter, Poincaré calculates (1) the capacity, (2) the self-induction of the arrangement. The value of the capacity of the arrangement of two spheres used by Hertz in the calculation of the period of the exciter was that of each of the spheres by itself, viz. 15 cms. Now, if one of the spheres were alone in its own field with a charge q and at a potential V, we should have $q/V = 15$. But at any instant when the charge of one sphere is q, that of the other sphere is $-q$; and since the spheres may be taken as nearly without mutual influence, the difference of potential between them is 2V. The capacity is, then, $q/2V$, or 7·5 cms.—half the value used by Hertz. That this is the proper value to use for the capacity is easily verified by a reference to the mode of establishing the equation of oscillation, when it is seen that the capacity is really defined by that equation as the charge on one of the spheres divided by their difference of potential.

The calculation of the self-induction given by Poincaré is interesting. Regarding, as an approximation, the currents in the spheres, and the influence of the spark-interval, as negligible, and taking the wire as of length l (equal to the distance between the centres of the bulbs), and of diameter d, and assuming that the current is wholly on the surface of the wire (which it is approximately, when in rapid alternation) he finds—

$$L = 2l \left\{ \log \frac{4l}{d} - 1 + \frac{k - 1}{2} \right\},$$

where k is the quantity which appears in Helmholtz's theory.

This differs from the value given by Hertz in having -1 for the middle term within the brackets instead of -0.75, and $(k - 1)/2$ instead of $(1 - k)/2$ for the third term. The first discrepancy arises through the currents having been taken by Hertz as uniform over the cross-section of the wire, and the second probably through an error in sign. The self-induction, L, is 1902 cms. if the term involving k is not taken into account, and (1902 + 150) cms. if k be put equal to zero.

Calculating the period $T (= 2\pi\sqrt{LC}$, where C is the capacity in electromagnetic units), we find it to be $2{\cdot}51 \times 10^{-8}$ seconds, and multiplying by the ratio of the electromagnetic unit of quantity to the electrostatic unit, or v, we get for the wave-length 7·53 metres. Hertz gives 1·77 for the calculated half period, and 5·31 for the corresponding half wave-length. On account of the error in the estimation of the capacity, it is clear that this value of the half period and half wave-length must be divided by $\sqrt{2}$, and this brings them into agreement with the values first stated. There is, however, a serious discrepancy between the results of theory and experiment, which we shall notice presently.

The calculation of period, &c., of course proceeds on the assumption that the resistance is negligible, and this is no doubt the case to a sufficient degree of approximation. In the theory itself, also, no account is taken of the induction coil or of the displacement currents in the dielectric; further, the energy is dissipated, not merely by the production of heat, but by radiation into the dielectric. That the influence of the induction coil is indeed negligible Poincaré gives reasons for supposing; in fact, on account of the enormous self-induction of the induction bobbin, and the small mutual induction of the exciter and the bobbin, the corrected differential equation is the one formerly found for the oscillation, with the addition of an exceedingly small term, so that the solution is practically the same as before. (Here, p. 162, the expression "a étant très grand" should be "a étant très petit"). As Poincaré states, the vibration of the exciter is like that of a very small pendulum attached to a massive pendulum of long period; the period of the former is very little affected by its mode of support. For a similar reason the period is very little affected by the very considerable capacity of the bobbin.

Experimenting on the velocity of propagation of electro-

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magnetic waves, Hertz found that the half wave-length in air was about 4·5 metres, the corrected period of the vibrator used being about 2×10^{-8} seconds. This gives a velocity of propagation of $900/(2 \times 10^{-8})$ (or $4{\cdot}5 \times 10^{10}$) cms. per second, exceeding the velocity of light by about 50 per cent. For the wave-length in wires, however, he found a value which gives a velocity of propagation nearly equal to the velocity of light, when the correction of the period for error in capacity is taken into account. (Later, M. Poincaré gives the half wave-length in air for these experiments as 4·8 metres.)

Herr Lechner, experimenting at Vienna, has also found a velocity of propagation in wires very approximately equal to the velocity of light, which thus confirms Hertz's result. On the other hand, MM. Sarasin and de la Rive, experimenting at Geneva in 1890, found that the wave-length observed depends very much on the dimensions of the resonator. But using an exciter exactly similar to that of Hertz, and of the same dimensions, and a resonator 75 cms. in diameter, and therefore nearly an exact copy of that employed by Hertz, they found a half wave-length of 3 metres, instead of 4·8 metres as found by Hertz. Thus there is a discrepancy between the two results which it is difficult to explain. Hertz himself gives a possible explanation, in a letter to M. Poincaré which is quoted in a note on some recent experiments which is printed as an appendix. On account of its interest, we take the liberty here of translating the extract quoted. It is to be noted that what is called the wave-length here is the distance from node to node, or half the complete wave-length.

"It is difficult for me to believe that I have been misled in the second method into finding 4·8 metres instead of 3 metres; but since the result of Messrs. Sarasin and de la Rive has every theoretical appearance of truth, I have endeavoured to find out the cause of the difference. Here are two ways of explaining it. The waves were produced between two parallel walls of a room, and I have taken account of the reflective action of only one of them. Let us suppose, to begin with, that the length of the room is an exact multiple of the wave-length, say three wave-lengths. We shall have two well-marked nodes at the exact distance. If the length of the room is four wave-lengths, we shall have three well-marked nodes. But if the length of the room is intermediate between these, and nearer the former than the latter, we shall have two less distinct nodes at a distance apart greater than a wave-length. This explanation would appear to me satisfactory, if the difference were not too great.

"The other way of explaining the difference is this. My reflecting plate of zinc was fixed in a niche in the wall, and it is possible that the projecting parts of the wall may have had the effect of carrying off the nodes to a greater distance from the wall, and thus of giving too great an apparent length as measured. But it is also true that the niche was from 5 to 6 metres in width, and it does not seem to me very probable that it can have had any great effect.[1]

"I therefore cannot tell precisely the cause of my error; but I believe there must be some way of explaining it For a long time I have sought in vain to find a probable cause for the difference of velocity in air and in wires; and I had myself found, before Messrs. Sarasin and de la Rive, that there is no difference for short waves of 30 cms. in length. The results of these gentlemen, however, give the same velocity for long waves, and contradict my experiments"

Connected with this point M. Poincaré has some instructive remarks on what Messrs. Sarasin and de la Rive have observed and called multiple resonance, and which has also been observed by Fitzgerald and Trouton. Their supposition is that the exciter gives rise neither to a single vibration of distinct period, nor to a limited number of distinct vibrations, but rather to such a complex of vibrations as would give a wide band of continuous spectrum. Thus all vibrations, agreeing with possible modes of vibration of the resonator would be reinforced. That this explanation is not borne out by the theory is true, but on account of the incompleteness of the theory it is not possible to attach much weight to this fact. It is hard to believe that the vibrations can be perfectly simple.

Poincaré proposes, however, the following explanation. For various reasons, he thinks the logarithmic decrement of the vibrations of the exciter is probably much greater than that of the resonator, and so the vibrations of the exciter diminish in amplitude more quickly than those which by any cause are set up in the resonator. Thus the resonator, being started by the exciter, would continue its vibrations after those of the exciter had become insensible, but would then vibrate in its own proper period, thus giving vibrations of longer period and of greater wave-length than those which excited it. The wave-length, being determined by interference, and used with the too short period of the exciter, would of course give too great a velocity of propagation. With this explanation Hertz has expressed himself as practically in accord, and so a possible way out of the difficulty seems opened up. As Hertz remarks, the oscillations of the exciter, represented graphically, do not give a curve of sines pure and simple, but a curve of sines the amplitude of which gradually diminishes. Such an oscillation will cause all resonators receiving it to vibrate, but those in tune with the exciter more violently than the others. A mathematical investigation of the point is given by M. Poincaré, which explains the result, shown by experiment, that the apparent spectrum found by Sarasin and de la Rive seems more extended when wires are connected to the vibrator, than when the propagation takes place freely in air.

Whether this explanation be satisfactory or not, there can be no doubt, on the whole, that the electromagnetic theory of light is substantially true. The theory is far from complete, and there are many outstanding points which require further theoretical and experimental elucidation. Some of these are touched on by Poincaré in his discussion of the field produced by exciters of different forms, and the theory of the resonator, but especially in a valuable series of notes which he has added to his lectures. These deal with special topics, which are there treated with more detail than was possible in the body of the work. Such, for example, are his notes on multiple resonance, the calculation of the period, and the propagation of waves in sinuous wires.

This article has run to too great a length, and must here close. M. Poincaré's work ought to be read by everyone interested in Maxwell's great scientific generalization—the greatest, perhaps, ever made by a natural philosopher since the days of Newton—and in its remarkable experimental verification by Hertz. There never was, perhaps, a time of greater mathematical and physical activity than the present, but withal it is marked by a care for the scientific student which no previous age ever displayed. It is no small encouragement to humbler scientific workers when masters of analysis like M. Poincaré take the trouble to publish, in a connected form, their lectures and researches on the current scientific questions of the day. Besides earning the gratitude of those who are thus admitted within the circle of their pupils, by immediately communicating their discoveries and expositions in this manner to the general scientific public, they multiply many-fold the direct effect of their work on scientific progress.

A. GRAY.

[1] For an interesting discussion of the effects of reflecting plates of different dimensions, see a paper by Mr. Trouton in the *Phil. Mag.*, July 1891.

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ON SOME POINTS IN ANCIENT EGYPTIAN ASTRONOMY.[1]

II.

4.—*The Probable Date of the Founding of Denderah as derived from the Account of the Building Ceremonial.*

SO much having been stated relating to the inscriptions recording building ceremonials, I will now return to the statement regarding Denderah, to see what can be made of it on the view that either the middle or the chief point—that is, the brightest star of the constellation of the Great Bear as we now know it—was the part referred to, and that the cord was stretched to the star on the horizon.

The first question which arises is, Was there any reason why δ Ursæ Majoris at the centre, or α the brightest, should have been used as the orientation point at any time? Was there any reason why any special sanctity should have been associated with either? Certainly not in the case of δ on account of its magnitude, because Dubhe, not far from it, is much brighter And certainly not in the case both of δ and α on account of the time of their heliacal rising. We seem therefore in an *impasse* along this line of inquiry, but a further consideration of the question brings out the remarkable fact that at two different points of time the North Polar distance of α Ursæ Majoris was nearly the same as that of α Lyræ and γ Draconis, so that α Lyræ would be visible at one of the dates and γ Draconis with α Ursæ Majoris at the other—all rising in the same amplitude.

The stars rising at the same amplitude, a temple directed to one would be directed to the other, but the stars would rise at different times. This, it may be suggested, may have been the reason why α was used by the king: the most convenient hour of the night was chosen. But there may have been another reason.

We know enough of the Egyptian priests to imagine it might be to their interest that even the king himself should not know everything, and the question arises whether, knowing the equal amplitudes of the risings of these stars, the secret was retained while α Ursæ Majoris was used.

This, of course, can only be put forward as a suggestion, and to many, no doubt, it will seem to be far-fetched; but from the account given by Herodotus of some of the ceremonials and mysteries connected with the temple of Tyre, it is suggested that the priests used starlight at night for some of their operations very much in the same way as they might have used sunlight during the day. According to Herodotus, in the temple in question there were pillars of gold and emerald which shone at night. Now, there can be little doubt that in the darkened sanctuary of an Egyptian temple the light of α Lyræ, one of the brightest stars in the northern heavens, rising in the clear air of Egypt would be quite strong enough to throw into an apparent glow such highly reflecting surfaces as those to which Herodotus refers.

Supposing such a ceremonial as this used at Denderah, the less the worshippers—who, reasoning from the analogy of the ceremonial termed the manifestation of Rā,[2] would stand facing the sanctuary with their backs to the chief door of the temple—knew about the question of a bright star which might probably produce the mystery the better; so that it may be almost said that the priests had a reason, and a very good one, for using a comparatively faint star for the orientation of a temple, some of the uses of which were to utilize for their own purposes some of the phenomena presented by a bright one rising at the same amplitude. These considerations have, however, only full force as between α Ursæ Majoris and α Lyræ.

Of course, so far, nothing can be said with certainty with regard to either of the stars in question having been chosen for the original orientation, but in an inquiry of this kind no line of evidence is to be neglected.

We next come to possible dates. Taking the amplitude of Denderah as 73°, the dates of foundation given by either α Lyræ or γ Draconis will be those at which these stars had an equal amplitude.

Roughly, and only roughly, these dates would have been as follows:—

	Horizon 2° high
α Lyræ	7200 B.C.
γ Draconis	4400 B.C.

Now, what are the records concerning this temple? We know that the structure now visible to us was built in the time of the last Ptolemies and the first Roman Emperors, and I have already shown that at those dates the Great Bear (the old Thigh) did not rise at all, as it was circumpolar.

But it is also known that there was a temple here in the time of Thotmes III., and even earlier, going back to the earliest times of Egyptian history. King Pepi, of the 6th dynasty (*circ.* 3233 B.C.), is portrayed over and over again in the crypts.

Even this is not all the evidence in favour of a high antiquity. In one of the crypts (No. 9), according to Ebers and Dümichen there are two references to the earliest plans of the temple. One inscription states that the great ground-plan (*Senti*) of Ant (Denderah) was found in old writing on parchments of the time of the followers of Horus (sun-worshippers) preserved in the walls of the palace during the reign of King Pepi. Another inscription goes further, referring to the restoration by Thotmes III. (*circ.* 1600 B.C.) of the temple to the state in which it was found described in old writings of the time of the King Chufu (Cheops) of the 4th dynasty (*circ.* 3733 B.C.). If any faith is to be placed in this inscription, it seems to me to suggest a still higher antiquity. There would have been more reason for describing an antique shrine than a brand new one, and the date 4400 is well within the historical period, according to Mariette.

If, then, I am right in my suggestion as to the word *ak* referring to α Ursæ Majoris, and as to a star rising at an equal amplitude to γ Draconis being for some reason made use of in the building ceremony, we find the closest agreement between the astronomical orientation, the definite statement as to a certain star being used in the building ceremonies, and the inscription in the crypts referring to Cheops as the earliest historical personage who describes the building. I must confess that this complete justification of the double record strikes me as very remarkable, and I think it will be generally conceded that further local observations should be made in order to attempt to carry the matter a stage further.

If the above results be confirmed we have a most important indication of the fact that in the rebuilding in the time of Thotmes III. and of the Ptolemies, the original orientation of the building was not disturbed, and that in the account of the building ceremonies we are dealing as surely with the laying of the first foundation stone as we are dealing with the original plan.[1]

[1] Continued from p. 299.

[2] One of the inscriptions relating to the manifestation of Rā has been translated by de Rougé as follows:—

"Il vint en passant vers le temple de Rā; il entra dans le temple en adorant (deux fois). Le ḫer-heb [celebrant] invoqua (celui qui) repousse les plaies du roi; il remplit les rites de la porte; il prit le seteb, il se purifia par l'encens: il fit une libation; il apporta les fleurs de *Habenben* [a part of the temple]; il apporta le parfum (?). Il monta les degrés vers l'adytum grand, pour voir Rā dans Habenben; lui-même se tint seul; il poussa le verrou; il ouvrit les portes; il vit son père Rā dans Habenben; il vénéra la barque de Rā et la barque de Tum. Il tira les portes, et posa la terre sigillaire (qu'il) scella avec le sceau du roi. Lui-même ordonne aux prêtres, 'J'ai placé le sceau; que n'entre pas quelqu'un dedans de tout roi qui se tiendra (là).'"—"Chrestomathie Egyptienne," de Rougé, iii. p. 60

[1] On this point I am permitted by Prof. Maspéro to print the following extract from a letter I received from him:—

"Tous les temples ptolémaïques et la plus grande partie des temples pharaoniques sont des *reconstructions*. Ce que vous avez observé de

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Here, however, it is necessary to proceed with caution, for the last word may not yet have been said when we accept γ Draconis.

I have elsewhere pointed out that it is not impossible that a temple once oriented to a certain star, and long out of use on account of the precessional movement, may be utilized for another, and be rehabilitated in consequence, when that same movement brings another conspicuous star into the proper rising amplitude.

In the present case, the orientation fits γ Draconis in the historic period, but it also fits α Lyræ in the times of the Hor-schesu, the dimly seen followers of Horus, or sun-worshippers, before the dawn of the historic period. If we assume that the record is absolutely true (and I for one believe in these old records more and more), and that Cheops only described a shrine founded by the Hor-schesu, then we are carried back to *circ.* 7000 B.C. I am indebted to my friend Dr. Wallis Budge, for the suggestion that the position of Denderah on the highway from the Red Sea—which may soon be reached by a railway from Keneh to Kosseir !—would make it one of the most important places in Ancient Egypt.

In any case the consideration has to be borne in mind that the series of temples with high northern and southern amplitudes at Denderah, Abydos, Thebes, Philœ, Edfu, were nearly certainly founded before the time at which the heliacal rising of Sirius, near the time of the summer solstice, was the chief event of the year, watched by priests, astronomers—if the astronomers were not the only priests—and agriculturalists alike. Now we know from Biot's calculations that this first took place *circ.* 3285 B.C., and that Sirius—though, as I am informed by Prof. Maspéro, *not* its heliacal rising—is referred to on inscriptions in pyramid times. The Sirius temples at Thebes, Philœ, and Denderah, are in all probability of much later foundation than those to which I have referred, and that at Denderah seems from its orientation to be the latest of all.[1]

I will, however, leave for a future occasion the question of the original Hor-schesu shrine, and consider at present an important relation between the chief temple at Denderah and one of the chief temples at Karnak. I am not aware that the relation has been pointed out before.

The amplitude of the temple at Denderah, dedicated to *Hathor*, is 73° N. of E. The amplitude of the temple dedicated to Mut or Maut at Karnak is 71° N. of E., which, assuming the same star to have been used, corresponds to a date (according to the height of the horizon) of *circ.* 3000 to 3500 B.C. This is therefore later than the Hathor temple of Denderah.

Now, we have it from Plutarch (*Isis* and *Osiris*, Parthey, cap. 56) that Isis = Maut = Hathor = Methuer, and this is sufficiently clear from the symbols of these goddesses, without his authority.

It is fundamental for the orientation theory that the cult shall follow the star. But we have here the same cult. Hathor and Maut are merely *local* names associated with local totems. Isis is a *generic* name simply, meaning an accompaniment of sun-rise, whether that light be the dawn, or a heliacally rising star, or even the moon. The generic symbol for Isis is the sun's disk and horns, which I think may not impossibly be a poetic development of the sign for sunrise. The local totem of the special warning star in use at any time or place may be anything: hippopotamus, crocodile, hawk, vulture, lion, or even some other common living thing into which the totem degraded when the supply of the original fell short.[1] Hence, as the number of warning stars was certainly very restricted, they, or rather the goddesses which typified them, had different names in almost every nome. Hence Egyptian mythology should be, as it is in fact, full of synonyms; each local name being liable to be brought into prominence at some time or another owing to adventitious circumstances, relating either to dynasties or the popularity of some particular shrine.

Let us concede, then, that we had the same cult. Now about the star. For Denderah we have already found γ Draconis. What about Thebes ? As I have elsewhere pointed out, the temple at Karnak, the date of the building of which is the most certain, undoubtedly pointed to γ Draconis.[2] Its amplitude is 62° N. of E. This was in 1200 B.C. Nor is this all; as I have also shown, it forms the second of a series with the following amplitudes :—

61°
62°
68°
71° } N. of E.

Now the last in this series, directed on this view to γ Draconis at Karnak, is precisely the temple of Maut !

So that here we have a very concrete case of the cult following the star, not only in the same place, but at different places, and we are driven to the conclusion that Hathor at Denderah and Maut at Thebes, exoterically different goddesses, were esoterically the same star γ Draconis.

Although it carries us still further into the region of mythology, there is more evidence to be gathered from a consideration of the old constellations. I do not think it saying too much to remark that among these the attention of the early Egyptians was almost exclusively confined to the circumpolar ones. Further, the mean latitude being, say, 25°, the circumpolar region was a restricted one, 50° in diameter, instead of over 100°, as with us. But not quite exclusively, for to them then, as to us now, the Great Bear and *Orion* were the two most prominent constellations in the heavens; for them, as for us they typified the northern and southern regions of the sky

There can be no question that the chief ancient constellation in the north was the Great Bear, or, as it was then pictured, the Thigh (Meskhet). After this came the Hippopotamus. I had come to the conclusion that this has been replaced on our maps by part of Draco before I found that Brugsch and Parthey were of the same opinion.

The female hippopotamus typified Taurt, the wife of Set (represented by a jackal with erected tail, or hippopotamus), and one of the most ordinary forms of Hathor presents us with the horns and disk surmounting a hippopotamus. There is evidence that the star we are considering, γ Draconis, occupied the place of the head or the mythical headgear.

So far, then, mythology is with me; but there is a difficulty. According to the theory the cult must follow the star: this must be held to as far as possible. But suppose the precessional movement causes the initial function of a star to become inoperative, must not the cult—which, as we assume, had chiefly to do with the heralding of sunrise at one time of the year or other—change? And if the same cult is conducted in connection with another star, will not the old name probably be retained ?

There is another temple of Hathor at Thebes—the

Dendérah, est vrai d'Esnéh, d'Ombos, d'Assouan, de Philæ, &c. Or, si les premiers constructeurs d'un temple—ou chez nous d'une église—peuvent choisir presque à leur gré l'emplacement et par suite l'orientation, la plus convenable, il en est bien rarement de même des *reconstructeurs*. Les maisons accumulées autour du temple les gênaient, d'ailleurs les habitudes du culte et de la population étaient prises: on rebâtissait le temple—comme d'ordinaire chez nous on rebâtit l'église—sur la même orientation et sur les mêmes fondations. J'ai constaté le fait à Kom-Ombo, où les débris du temple décoré par Amenhotpou I. et Thoutmosis III. sont orientés exactement comme ceux du temple ptolémaïque actuel, bâti *sur* les ruines du précédent. Vous avez donc le droit de dire, non seulement pour Dendérah, mais pour beaucoup d'autres temples, qu'ils ont été reconstruits sur l'orientation du temple qu'ils remplaçaient, quand même cette orientation ne répondait plus à la réalité des choses."

[1] I have not yet reduced my own observations of this temple, but Nissen refers to previous measures, *Rheinisches Museum für Philologie*, 1885, p. 44.

[1] Have we such instances of degradation in the cat replacing the lion and the black pig the hippopotamus, to give two instances ?

[2] I refer to the Temple M of Lepsius, built by Ramses III.

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temple of Dêr el-Bahari, founded or embellished by Queen Hatshepset (*circ.* 1600 B.C.). This temple, instead of being oriented 73° N. of E., lies 26° S. of E., it can never, therefore, have faced the star observed at Denderah.

Now, are there any possible explanations? Two have suggested themselves to me.

At Denderah the image of the goddess was taken on a certain day in the year on to the terrace, so that the light of the sun—her father Rā—might fall upon her.

Building a temple so that the sunlight might enter it once or twice a year (which could not happen in the Denderah temple in consequence of its northern outlook) would enable the aforesaid operation to be performed in the sanctuary itself. The Thebes temple on this hypothesis assured this—and at the winter solstice.

The next explanation I submit to Egyptologists with much fear and trembling. It is, briefly, that about 3200 B.C. observations of the star Sirius replaced, or were added to, those made of γ Draconis. Mythologically a new Isis would be born.

I base this suggestion on the following considerations:—

(1) While the Denderah Hathor was represented by the disk and horns on a hippopotamus; at Thebes, the city of the "Bull" Amen, Hathor is represented by a cow with a like headdress.

(2) Sirius, represented originally as a goddess with the two feathers of Amen standing in a boat, is now changed to a cow with the disk and horns.

(3) Hathor was the cow of the western hills of Thebes. It is in these hills that the temple Dêr el-Bahari lies, and this temple if oriented originally to Sirius would have been founded about 3000 B.C., when Sirius would have an amplitude of 26°.

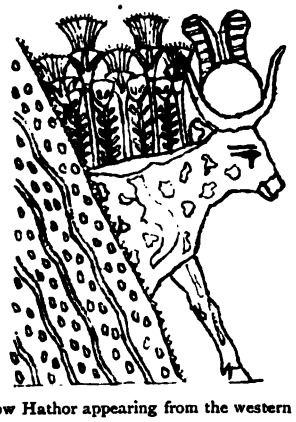

FIG. 4.—The cow Hathor appearing from the western hills of Thebes.

(4) A temple was built in later times at Denderah oriented to Sirius, and Sirius with the cow's horns and disk became the great goddess there, and when her supremacy all over Egypt became undoubted, her birthplace was declared—at Denderah—to have been Denderah.[1]

(5) In the month list at the Ramesseum the first month is dedicated to Sirius, the third to Hathor. This is not, however, a final argument, because *local* cults may have been in question.

(6) Set seems to have been a generic name applied to the northern (? circumpolar) constellations, perhaps because *set*=darkness, and these stars, being *always visible* in the night, may have in time typified it. Taurt, the hippopotamus, was the wife of Set. The Thigh was the thigh of Set, &c. γ Draconis was associated therefore with Set, and the symbolism for Set-Hathor was the hippopotamus with horns and disk. Now, if, as is suggested, Sirius replaced γ Draconis, and the cow replaced the hippopotamus, the cult of Set might be expected to have declined; and as a matter of fact the decline of the worship of Set, which was generally paramount under the earlier dynasties, and even the obliteration of the emblems on the monuments, are among the best-marked cases of the kind found in the inscriptions.[1]

(7) The *Isis* temples of Denderah were certainly oriented to Sirius; the *Hathor* temple was as certainly *not* so oriented. And yet, in the restorations in later times (say Thotmes III.—Ptolemies), the cult has been made Sirian, and the references are to the star which rises at the rising of the Nile.

I do not see why the Egyptians should have hesitated to continue the same cult under a different star when they apparently quite naturally changed Orion from a form of Osiris (Sah-Osiris) and a mummy (as he was represented when the light of his stars was quenched at dawn at the rising of Sirius) to that of Sah-Horus (when in later times the constellation itself rose heliacally).

J. NORMAN LOCKYER.

[1] Brugsch thus translates one of the inscriptions:—"Horus in weiblicher Gestalt ist die Fürstin, die Mächtige, die Tronfolgerin und Tochter eines Thronfolger. Ein fliegender Käfer wird (sie?) geboren am Himmel in der uranfanglichen Stadt (Denderah) zur Zeit der Nacht des Kindes in seiner Wiege. Es strahlt die Sonne am Himmel in der Dämmerung, wann ihre Geburt vollbracht wird."—Brugsch, "Astron. Inscription.," p. 97.

SUPERHEATED STEAM.

I HAVE noticed a curious misapprehension, even on the part of high authorities, with respect to the application of Carnot's law to an engine in which the steam is superheated after leaving the boiler. Thus, in his generally excellent work on the steam-engine,[2] Prof. Cotterill, after explaining that in the ordinary engine the superior temperature is that of the boiler, and the inferior temperature that of the condenser, proceeds (p. 141): "When a superheater is used, the superior temperature will of course be that of the superheater, which will not then correspond to the boiler pressure."

This statement appears to me to involve two errors, one of great importance. When the question is raised, it must surely be evident that, in consideration of the high latent heat of water, by far the greater part of the heat is received at the temperature of the boiler, and not at that of the superheater, and that, of the relatively small part received in the latter stage, the effective temperature is not that of the superheater, but rather the mean between this temperature and that of the boiler. An estimate of the possible efficiency founded upon the temperature of the superheater is thus immensely too favourable. Superheating does not seem to meet with much favour in practice; and I suppose that the advantages which might attend its judicious use would be connected rather with the prevention of cylinder condensation than with an extension of the range of temperature contemplated in Carnot's rule.

If we wish effectively to raise the superior limit of temperature in a vapour-engine, we must make the boiler hotter. In a steam-engine this means pressures that would soon become excessive. The only escape lies in the substitution for water of another and less volatile fluid. But, of liquids capable of distillation without change, it is not easy to find one suitable for the purpose. There is, however, another direction in which we may look. The volatility of water may be restrained by the addition of saline matters, such as chloride of calcium or acetate of soda. In this way the boiling temperature may be raised without encountering excessive pressures, and the possible efficiency, according to Carnot, may be increased.

The complete elaboration of this method would involve the condensation of the steam at a high tempera-

[1] Rawlinson, vol. i., p. 317; vol. ii., p. 347 *et seq.*

[2] Second edition (Spon: London, 1890).

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ture by reunion with the desiccating agent, and the communication of the heat evolved to pure water boiling at nearly the same temperature, but at a much higher pressure. But it is possible that, even without a duplication of this kind, advantage might arise from the use of a restraining agent. The steam, superheated in a regular manner, would be less liable to premature condensation in the cylinder, and the possibility of obtaining a good vacuum at a higher temperature than usual might be of service where the supply of water is short, or where it is desired to effect the condensation by air.

RAYLEIGH.

TWO AFRICAN EXPLORERS.

WE regret to have to record the death of two of the best-known African explorers—Colonel J. A. Grant and Dr. Wilhelm Junker; the latter comparatively a young man, and the former by no means old. They belonged to two distinct types of African explorers: Grant was the true pioneer, who went out to force his way through an unknown region; Junker was the scientific student, content to spend years in one limited region in order to work out its geography, natural history, and ethnology.

The announcement of the death of Colonel James Grant, Speke's companion in the expedition for the discovery of the source of the Nile, has been received with widespread regret. The stalwart figure and genial, good-natured, boyish face of Colonel Grant has been familiar in London society and in geographical and scientific circles for more than twenty years. In African exploration and in the younger generation of African explorers he took a keen interest to the last. He was a man of chivalrous loyalty to his friends. Speke's memory he almost worshipped, and it need scarcely be said that his feelings to Burton were of a very different stamp. Mr. Stanley had no more staunch supporter than Colonel Grant. Born in the parish manse of Nairn, in Scotland, in 1827, he was educated, like so many other Scots that have distinguished themselves in the service of the country, at the Grammar School of Aberdeen, and at Marischal College. At the age of nineteen he obtained a commission in the East India Company's service, and between that and the end of the Mutiny saw much active service, and won honours for bravery and devotion to duty. It is, however, as an African explorer that he claims notice in these pages. It will be remembered that in 1857 the only great lakes, of which we knew anything, in the map of Africa were Chad and Nyassa, the latter then quite recently plotted for certain by Livingstone. But rumours of other lakes had been filtering down to the coast for years. In 1857, Burton and Speke started from Zanzibar in search of "the Great Lake," as it was vaguely called; and, after a painful march of eight months, found Tanganyika, the first discovered of those great sheets of water which form so marked a feature of the centre of the continent. On the return journey, Speke took a run north, to find another great lake said to exist in that direction. He reached the southern shore of Lake Ukerewe, which has since become so well known as Victoria Nyanza. Though Speke only caught a glimpse of the southern waters of the lake, and had no adequate idea of its amplitude, he conjectured rightly that it must be the source of the Nile. Into the unhappy quarrel of Speke and Burton it is unnecessary to enter. But Speke, and not Burton, was selected by the Royal Geographical Society, in 1860, to lead an Expedition to the lake for the purpose of confirming his conjecture. This Expedition Government subsidized to the amount of £2500. Captain Grant, as he was then, was chosen to accompany Speke. The latter was, no doubt, the leader of the Expedition; but Grant, though he suffered much, and had to be carried a great part of the way, did much to render the expedition of scientific value. The unknown countries to the west and north-west of Victoria Nyanza were explored, though the contour of the lake was very inadequately laid down. Uganda was reached in 1862, and in July of that year the Nile was seen issuing full-born from the lake, and dancing its way north-west over Ripon falls. The two travellers followed the river for 120 miles, but were compelled to quit it, and so missed the discovery of its connection with the Albert Nyanza. They came upon it again 70 miles further down, and reached Gondokoro in February 1863, where they were met and succoured by Samuel Baker.

As might have been expected, the discoverers of the Nile sources received a great ovation on their arrival in England. Grant, like Speke, received the gold medal of the Royal Geographical Society, and was made a C.B. In 1864, under the title of "A Walk across Africa," he published a narrative of the expedition. In 1872 he published "A Summary of the Speke and Grant Expedition" in the Journal of the Royal Geographical Society. Colonel Grant was a careful observer, and his "Walk across Africa" abounds in interesting facts and suggestions on country and people, especially on the latter.

It was he who did the greater part of the scientific work on the Nile Expedition, and among other things he made a considerable collection of dried plants now in the Kew Herbarium. A rough list of these formed an appendix to Speke's "Journal of the Discovery of the Source of the Nile"; and most of the illustrations of this work were from drawings made by Grant. The publication of the first volume of Prof. D. Oliver's "Flora of Tropical Africa" fired Grant with the desire to have a special volume prepared on the flora and fauna of the Expedition. The result was that the whole of the twenty-ninth volume of the Transactions of the Linnean Society was devoted to the flora, and it is one of the most interesting of the series. The purely botanical part was contributed by Prof. Oliver and Mr. J. G. Baker; and the 136 plates (prepared at Colonel Grant's expense) illustrating the new or otherwise specially interesting plants, are some of the best work of the late W. H. Fitch.

In 1871 Grant was elected a Fellow of the Linnean Society, and in 1873 of the Royal Society; and many other distinctions were conferred upon him, including a Companionship of the Star of India for his military services in India and Abyssinia.

Dr. Johann Wilhelm Junker was a different type of explorer. He was born in Moscow, of German parents, on April 6, 1840. He spent his boyhood in Göttingen, attended the German Gymnasium at St. Petersburg, and studied medicine at Göttingen, Berlin, and Prague. After a visit to Iceland, Junker went to Tunis in 1874, and to Egypt in 1875. In that year he began those explorations which, with one or two interruptions, he carried on continuously for twelve years. He visited Lake Mareotis, the Natrön Lakes, and the Fayum. In 1876 he went from Suakin, through the Khor Baraka, to Kassala and Khartum; he explored the lower Sobat, and made successive journeys among the western tributaries of the Nile. In 1876 he proceeded as far westwards as Makaraka, and in 1877 crossed the Tangi River and visited the Wau, thus overlapping the route of Schweinfurth (1869–70). Indeed, Junker, during his many years' journeying, did a great deal to supplement the work of his distinguished predecessor. After a brief visit to Europe (1878), Junker was back in Africa in 1879, this time accompanied by a photographer (Bohndorff), who also lent a helping hand in preparing the numerous natural history collections. He accompanied several of the expeditions sent out by the Egyptian authorities from Khartum. But Junker often wandered almost alone, with very few native companions, and as a

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rule he was everywhere well received. To him we are indebted for much of our information on the Monbuttus and their country, the A-Sandeh and the various other tribes that inhabit the wide region watered by the Western Nile tributaries and some of the northern feeders of the Congo. Indeed, much of Junker's work lay in that extremely interesting country which forms the water-parting between the basin of the Nile and the Congo, and his hydrographical observations form some of the most important results of his many years' travels. To him may be said to be due the first steps in the solution of what was long known as the Wellé problem. The Wellé, which Schweinfurth came upon near its sources, was, even up to 1886, conjectured by many to be the upper course of the Sharé, which runs into Lake Chad. Junker struck the river at various places, one as far west as 22° 40′ E.; but it was not until Grenfell and Van Gèle followed the Mobangi up from the Congo that the Wellé was proved to be one of the chief northern feeders of the Congo. But this is only one of many services rendered by Junker to the scientific geography of Africa. His investigations into the ethnology of the whole of this intensely interesting region are of the first importance, and his collections both in ethnology and in natural history now form some of the most prominent exhibits in the great Museum of St. Petersburg. Junker was an admirable specimen of the scientific explorer, and his twelve years' researches in the Soudan entitle him to be classed with Schweinfurth and Nachtigal, Wallace and Bates. Junker was in the heart of the Soudan when the Mahdist revolt reached a crisis. He had the greatest difficulty in escaping, and it was only after long detention that he reached Europe *vid* Lake Victoria, Zanzibar, and Egypt. He made many friends in London when he came here to receive the gold medal of the Royal Geographical Society, which he well deserved.

NOTES.

MRS. ADAMS wishes us to say that she would be very grateful if former friends and scientific correspondents of the late Prof. J. C. Adams would be so kind as to send to her care any of his letters still in their possession. Their doing so would much assist in the preparation of a memoir. All letters so intrusted will be carefully returned.

THE Electrical Committee of the Royal Commission for the Chicago Exhibition are anxious that the electrical department of the Exhibition should contain a good representation from this country. They have accordingly issued a circular letter on the subject, and it may be hoped that this will receive the careful attention of all who are in a position to facilitate the Committee's arrangements. The Committee especially desire that the Exhibition may display the very large share which English electricians have had in the development of electrical science and its practical applications. It is hoped that the fine collection of historical apparatus in the possession of the Post Office may be shown, and this will be supplemented by contributions which will be sought from many other sources. Although practical electric lighting has made greater progress in America than in this country, the Committee think there is much that England can show electricians on the other side of the Atlantic; and firms who devote special attention to the domestic uses of electricity and its artistic application are reminded that it may be to their interest to send to the Exhibition good specimens of their work. There will also be excellent opportunities for the manufacturers of electric railway signals, and for electro-metallurgists.

ON January 1, no fewer than 2082 applications for space at the Chicago Exhibition had been received from intending exhibitors in the United States alone. The number at the Philadelphia Centennial on the corresponding date was 864. Many applications have come from foreign countries, and it is expected that the exhibitors will be more numerous than at any previous "World's Fair." The allotment of space is to be made about June. The reception of exhibits will begin on November 1 next, and continue until April 10, 1893.

WE regret to have to record the death of Mr. H. W. Bates, F.R.S. He was sixty-seven years of age. Of his well-known book, "The Naturalist on the River Amazons," Darwin said that it was "the best work of natural history travels ever published in England." His "Contributions to an Insect Fauna of the Amazons Valley," printed in the Transactions of the Linnean Society, were described by the same high authority as "one of the most remarkable and admirable papers he had ever read in his life." Mr. Bates is widely known as the discoverer of the principle of mimicry in the animal world. For twenty-seven years he was the acting secretary of the Royal Geographical Society.

WE have received an intimation of the death of Dr. Pieter Willem Korthals, at the advanced age of eighty-four years. He was for many years in the Dutch East Indian Service, and published a considerable number of papers on the botany of Sumatra, Borneo, and Java, the most noteworthy of which form a folio volume entitled "Verhandelingen over de Natuurlijke Geschiedenis der Nederlandsche overzeesche Bezittingen," edited by C. J. Temminck, 1839–42. This work contains seventy coloured plates of excellent execution. Dr. Korthals's first botanical paper appeared in 1830, and dealt with the genus Nepenthes; and his last, so far as we are aware, appeared in 1854. He was a Knight of the Dutch Order of the Lion.

THE Council of the Royal Society of Edinburgh have awarded the Keith Prize for 1889–91 to Mr. R. T. Omond, for his contributions to meteorological science; and the Makdougall-Brisbane Prize for 1888–90 to Dr. Ludwig Becker, for his paper on "The Solar Spectrum at Medium and Low Altitudes."

THE Grand Gold Medal of the Paris Geographical Society has been awarded to M. Reclus. A gold medal has also been awarded to the Prince of Monaco for his researches on marine currents.

SIR JOHN COODE, Past-President of the Institution of Civil Engineers, was last Friday elected by the Committee of the Athenæum Club a member, under Rule II., which provides for the annual introduction of a certain number of persons of distinguished eminence in science, literature, or the arts, or for public services.

THE third Congress on anthropology in relation to crime will be held at Brussels from August 23 to September 3 next.

ON Monday Lord Cowper called attention in the House of Lords to the subject of technical instruction, and to "the difficulties in which County Councils were placed by not knowing whether or not they could rely upon a permanent annual Government grant for its promotion." Lord Cranbrook, in the course of his reply, said he did not believe any Government would repeal the Act in accordance with which the Councils had received the money which was being devoted to technical education. So far as the present Government were concerned, they had not the smallest intention of repealing the Act or of taking the money for any other purpose.

AT a Conference on technical education, held in Edinburgh on October 29, 1891, a general committee was appointed to consider the subject. This general committee in turn appointed a sub-committee to report on the amendments necessary in the laws relating to technical education in Scotland. The report of the sub-committee has now been issued; and appended to it is an official statement to the effect that the general committee,

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while approving of the suggestions made in the report, is of opinion that the Government ought to take an early opportunity of dealing with the question of technical and secondary education in a comprehensive measure, and that, for the efficient supervision of technical education, wider administration areas than the parish are required.

THE ceremonies in connection with the tercentenary celebration of Trinity College, Dublin, will begin on the morning of July 5, and conclude on the evening of July 8 next. Invitations were sent to most of the Universities and learned Societies in Europe, America, and the colonies, during last term; and replies have been received from many sending representatives, including Aberdeen, Athens, Bern, Cambridge, Christiania, Edinburgh, Erlangen, Giessen, Heidelberg, Johns Hopkins, Lausanne, Leyden, Madras, Naples, Smithsonian Institute, Sydney, Toronto, &c., and acceptances are daily arriving. A large number of acceptances have also been received to the numerous personal invitations. Among the men of science who intend to be present are Sir F. Bramwell, Geo. Darwin, Thiselton Dyer, J. Evans, D. Ferrier, M. Foster, Sir A. Geikie, H. Hertz, J. W. Judd, Ray Lankester, Sir J. Lister, Sir J. Lubbock, Baron Nordenskiöld, Sir J. Paget, Lord Rayleigh, Sir G. Stokes, and Sir W. Turner. While the programme of the celebration has not yet been finally approved of, it will probably include the following:—On the Tuesday morning there will be a reception by the Provost of Trinity College of all the invited guests; then a short full choral service in St. Patrick's Cathedral; in the afternoon a garden party; and in the evening a civic ball. On Wednesday morning there will be the presentation of addresses of congratulation from the various Universities, and in the evening a grand banquet, at which it is expected that over five hundred guests or members of the University will be present. On Thursday morning there will be a special Commencements for the conferring of a number of honorary degrees; after which there will be an adjournment from the Senate House to the College Park to witness the College races, held under the auspices of the College Athletic Club; in the evening there will be a special amateur dramatic performance. On the Friday morning there will be a gathering of the students to hear short addresses from some of the distinguished visitors; in the afternoon, a concert given by the members of the University Choral Society; and in the evening, the students' ball. Numerous committees are daily engaged working out the multifarious details. One of these has been charged with the superintendence of the publication of an illustrated volume, which is to give the past and present history of the College, the publication of which volume has been undertaken by the firm of Messrs. Marcus Ward and Co.

ON April 1, 1841, Sir William Hooker began his duties as Director of Kew. In the year 1891, therefore, the establishment might have celebrated its jubilee as a national institution, and it seemed to Mr. Thiselton Dyer that he might fitly mark the occasion by giving in the *Kew Bulletin* some account of the origin and development of the Royal Gardens as a place of botanical study. This intention he has now partly carried out, the December number of the *Bulletin*—which has just been issued—being entirely devoted to the subject. The narrative is one of great interest, and has evidently cost the author much hard work, as scarcely any authentic records exist of the period before 1840, when the Gardens were a purely private possession of the Crown. He has thus had "to fall back on local traditions, on local histories, the statements in which are often confusing and inaccurate, and on such scattered notices as could be gathered from contemporary literature." In the present instalment, the story is brought down to the year 1841. The history of the last half-century will be given in another number.

IN pursuance of his botanical expedition to Persia, Herr J. Bornmüller arrived at Batoum on December 24. Thence he intended proceeding to Teheran by way of Tiflis and Baku, and then, as rapidly as possible, to the south of Persia. The expedition is intended to extend over two years, and Herr Bornmüller does not intend to collect more than from fifteen to twenty sets of the plants obtained. Orders for sets should be sent to Herr R. Huter, Sterzing, Tirol.

MR. JAMES BRITTEN AND MR. G. S. BOULGER intend to issue (to subscribers) in June next, their "Biographical Index of British and Irish Botanists," reprinted, with additions and corrections, from the *Journal of Botany*, and brought down to the end of 1891. As the promises of support at present received will not cover expenses, they will be glad to receive the names of additional subscribers, addressed to the care of the publishers of the *Journal of Botany*.

ACCORDING to a telegram from New York, one of the finest displays of the aurora borealis ever known in that latitude was observed on the evening of February 13. The phenomenon stretched over a great belt of territory from Iowa to the Atlantic. A peculiar effect was produced on the telegraph system, and for intervals of three or four minutes at a time the wires were so surcharged with atmospheric electricity that between New York and Albany it was possible to send messages without the aid of the regular batteries. The current, however, was intermittent, and the effect unsatisfactory. For nearly two hours ordinary business could not be transacted with any degree of exactness. The aurora seemed to occupy the whole of the northern heavens, and was beautifully marked, the colouring being clear and distinct. People at first feared that a great fire was raging.

DR. A. WOEIKOF, of St. Petersburg, who is engaged on an investigation into the cause of the famine in Russia, writes to the *Meteorologische Zeitschrift* that it is chiefly due to drought from August to October 1890, which injured the winter crops; to partial and insufficient snow, which melted early in the spring, and was followed by frost in April; and lastly to droughts and hot winds from May to July 1891. In the southern portion of the Government of Samara the prospects up to June 10 were excellent, but the harvest was destroyed by two days of hot winds, on June 14 and 15. And in the southern central provinces also, where the winter crops had greatly suffered, a moderate harvest was hoped for after the middle of July, but four hot days, from July 13 to 16, quite destroyed the crops.

THE Journal of the Scottish Meteorological Society (third series, No. 8) contains a very interesting paper on silver thaw at Ben Nevis Observatory, by R. C. Mossman. The phenomenon is somewhat common at that Observatory, and occurs during an inversion of the ordinary temperature conditions, the temperature being considerably lower at the surface than at higher altitudes, causing the rain to congeal as it falls. In the six years 1885–90, 198 cases of silver thaw were observed, with a mean duration of 4½ hours in each case, and they nearly all occurred between November and March, during times of perfectly developed cyclones and anticyclones. An examination of the weather charts of the Meteorological Office showed that for the 198 days on which the phenomenon was observed, the distribution of pressure was cyclonic on 137 days, and anticyclonic on 61 days. In anticyclonic conditions there was a cyclonic area central off the north-west coast of Norway, while the centre of the anticyclone was over the south of the British Isles. In cyclonic cases, an anticyclone lay to the south, over the Iberian Peninsula. The lowest temperature at which the phenomenon took place was 18°, and was rarely below 27°. Fully 90 per cent. of the cases occurred when the thermometer was between 28° and 31°·9, so that the

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greater number of cases occurred just before a thaw. The most common type of cloud which preceded both cyclonic and anti-cyclonic cases of silver thaw was cirro-cumulus, frequently accompanied by cirrus and cirro-stratus; and the changes showed that the higher strata of the atmosphere came first under the influence of the moist current, which took from three to eight hours to descend to the height at which cumulo-stratus forms. An examination of a series of storm charts prepared by Dr. Buchan disclosed the somewhat remarkable fact that 73 per cent. of the cyclonic, and 63 per cent. of the anticyclonic cases of silver thaw on Ben Nevis were followed or preceded by gales on our northern and north-western coasts; and it would appear from the wind conditions that the barometric gradient at the height of Ben Nevis (4407 feet) must be totally different from what obtained at sea-level during the occurrence of silver thaw on the hill-top. Many of our readers will remember the remarkable case of silver thaw which occurred in London last Christmas Day.

THERE has been much talk in Germany about Dr. Peters's discovery of saltpetre in the Kilima Njaro district. This discovery accords with statements which were already well known. Dr. Fischer, after an examination of the Donjongai volcano, reported that in the neighbourhood of the crater there were a series of curiously shaped veins of a white substance which he took to be either saltpetre or soda. In 1879, Herr Jarler asserted that large quantities of sulphur would probably be found in the crater. The Berlin correspondent of the *Times*, by whom these facts are noted, adds that not far from the volcano there lie great swamps from which soda is obtained. It is expected that an expedition for the exploration of the district will soon be sent out by the German East Africa Company.

PROF. A. GIARD calls the attention of naturalists to a new case of mimicry between two very different insects (the one Hymenopterous, the other Dipterous)—*Athalia annulata* and *Beris vallata*. When both insects are quiet, the resemblance of colour and patterns is perfect, and as the *Athalia*, like most Tenthredinæ, is protected against birds and other foes by its unpleasant smell, it is probable that the resemblance is of considerable service to *Beris*. M. Giard also refers to the larva of *Allantus tricinctus* which is commonly found on the leaves of plants, vividly coloured, and quite conspicuous, but resembling in form and colour birds' droppings, as is the case with a spider described by H. O. Forbes.

THE number of persons who approve of cremation seems to be steadily increasing. From the Report of the Cremation Society of England for 1891, we learn that in 1885, the first year the crematorium at Woking was used, only three bodies were sent there; in 1886 the number was 10; in 1887, 13; in 1888, 28; in 1889, 46; in 1890, 54; and during the past year, 99. Crematoria are being built in various parts of the country. At Manchester a crematorium is in course of erection, and will, it is thought, be completed and opened for use during the coming spring. A company has also been formed, and is making rapid progress, with the same object at Liverpool; and the City of London Commission of Sewers is taking steps to obtain powers to erect a crematorium at their cemetery at Ilford. The Cremation Society at Darlington, and other associations, are moving in the same direction.

A SMALL axe of nephrite, found at Ohlau, in Silesia, and now in the Roman-German Museum at Mainz, has lately been carefully examined by Dr. O. Schoetensack, with a view to the discovery of the source from which the material must have been obtained. From a thorough determination of its specific gravity, microscopic structure, and chemical composition, Dr. Schoetensack concludes that the nephrite is exactly the same as a mineral which has been found by Dr. Traube near Jordansmühl, in Silesia. There is no reason, therefore, why the axe should not be pronounced to be of Silesian origin. This is the only prehistoric object of nephrite, found in Europe, the source of which has been definitely decided.

PROBABLY few authorities responsible for the making and maintenance of roads are aware that one of the things against which they should be on their guard is the use of rotten flints for macadamising purposes. Mr. C. Carus-Wilson has been giving some attention to this subject at Bournemouth, and, in a letter to a local journal, states the conclusions at which he has arrived. He was led to consider the matter by the abnormal quantity of slush on the surface of Poole Road. This he attributes to the fact that the road has again and again been mended with rotten flints, by which he means flints that have become decomposed to such an extent that there is very little true flint left. These flints are surrounded by a thick zone of soft white material. This is rapidly removed from the flinty nucleus by the grinding and pressure to which it is subjected; while the true flinty nucleus, being thus denuded of its outer covering, becomes too small to bear much crushing weight, so that it quickly breaks up, and forms a fine flint sand. Mr. Carus-Wilson has found by repeated microscopic analysis that the Poole Road mud is formed principally of these two substances.

THE Journal of the Society of Arts prints this week an interesting lecture on burning oils for lighthouses and lightships, by E. Price Edwards. It was delivered at a meeting of the Society on February 10. After the lecture Sir Lyon Playfair, who was in the chair, said it was quite clear that mineral oils must in time beat the vegetable oils, on account of their chemical composition, the ingredients in the latter not being all combustible, but consisting of fatty acids and glycerine. Mineral oils, on the other hand, were nearly of the same composition as olefiant gas, the illuminating constituent of coal gas, with the addition of a little more hydrogen. They were therefore sure to win in the end; it was merely a question of manufacturing them safely.

AT the meeting of the Field Naturalists' Club of Victoria on December 14 last, Mr. A. J. Campbell read a paper on a nest and egg of a bird of paradise (*Ptiloris victoriæ*). Eastern Australia possesses a genus (three species) of these beautiful birds, but they are very seldom seen. The nest was found on November 19, by Mr. Dudley Le Souëf and Mr. H. Barnard during a visit to the North Barnard Islands, about forty miles off the coast from Cardwell, Queensland. They watched the hen bird for some time, and saw her fly into the crown of a Pandanus tree growing close to the open beach. Although they could not distinguish the nest itself, they could see the head of the bird as she sat on it. The nest was about 10 feet from the ground, and the bird sat quietly, although they were camped about 5 feet away from the tree. There was a single egg, the incubation of which was probably about seven days old. The nest was somewhat loosely constructed of broad dead leaves and green branchlets of climbing plants and fibrous material. Inside were two large concave-shaped dead leaves underneath pieces of dry tendrils which formed a springy lining for the egg or young to rest upon. The following is the measurement in centimetres: over all, 19 broad by 9 deep; egg cavity, 9 across the mouth by 4 deep.

DOVE'S observation that when a tuning-fork of proper pitch is held to each ear beats can be heard, where there is no possibility of interference of the sound-waves in the air, is confirmed by Dr. W. Scripture (Wundt's *Philosophische Studien*), who further gives experimental ground for rejecting the hypothesis of transference of the sound through the bones of the head of

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the Eustachian tubes. Difference-tones, on the other hand, seem to be only perceptible when the tones of both forks affect the same ear. The same number of the *Studien* contains the first instalment of an elaborate article by Dr. J. Merkel upon the psychophysical error-methods.

IF we may judge from the progress of the Photographic Society of India, photography is rapidly becoming more popular among Anglo-Indians. In January 1891, there were 277 members on the Society's books; now there are over 310.

THE *Oesterreichische Botanische Zeitschrift* for February contains a very full account of the results of Porta and Rigo's visit to Spain in 1891, and of the species gathered by them on their journey.

WE have received Parts 10 and 11 of the "Illustrations of the Flora of Japan," published at Tokyo. The drawings continue excellent; the diagnoses are unfortunately in Japanese.

MESSRS. DULAU AND CO. have issued a catalogue of botanical works which they offer for sale. It contains the titles of about 3000 writings relating to geographical botany.

THE "Electrical Trades Directory," issued by Mr. George Tucker on behalf of the *Electrician*, has made its tenth annual appearance; and no effort has been spared to bring it up to date. The biographical division of the work contains sketches of the careers of 260 men who are well known in the electrical world. These sketches have all been revised by the subjects of them. No fewer than 28 of these notices are accompanied by portraits, among which is a portrait of Prof. Ayrton from a specially engraved steel plate.

THE Royal University of Ireland has issued its Calender for the year 1892.

AN experiment, illustrating the remarkable power possessed by palladium of occluding hydrogen, is described by Prof. Wilm, of St. Petersburg, in the current number of the *Berichte* of the German Chemical Society. The experiment is so simple, and requires so short a time to exhibit, that it would appear to be eminently suitable for lecture demonstration. The metallic palladium is employed in the finely divided state obtained by heating the easily prepared yellow crystals of the compound $PdCl_2 . 2NH_3$, first in the open air, and subsequently for a short time in an atmosphere of hydrogen. A small quantity, about four grams in weight, of the palladium so obtained is placed in a bulb blown at the bend of a U-shaped tube. The extremity of one limb of the U-tube is bent round at right angles, and connected with a wash-bottle containing sulphuric acid, which in its turn is connected with a Kipp's apparatus generating hydrogen from zinc and dilute sulphuric acid. The wash-bottle serves not only to dry the hydrogen, but also to indicate the speed of the current of gas. The extremity of the other limb of the U-tube is narrowed to a capillary, and terminates with a tightly-fitting stopcock and jet. In commencing the experiment, the hydrogen current is started, and then, first the metal, and afterwards the whole U-tube, is carefully heated with a Bunsen flame in order to remove the moisture formed by the action of the hydrogen, under the influence of the palladium, upon the oxygen of the air contained in the apparatus. When all the air and moisture are thus driven out of the apparatus, an attempt may be made to ignite the issuing hydrogen at the jet above the open stopcock. It will be found, however, that even while the metal is hot and the stream of hydrogen very rapid, a constantly burning flame cannot be maintained at the jet with the stopcock fully open; instead, a series of somewhat explosive ignitions and sudden extinctions occurs. It is only when the stopcock is turned so as to reduce the exit of the gas to a minimum that a constantly burning jet can be obtained, the hydrogen in contact with the palladium being then subjected to a certain amount of compression. The palladium is now heated a little more strongly, just above bright redness, when it is no longer capable of occluding hydrogen, and then the lamp is withdrawn, and after a few seconds the stopcock closed. The occlusion is then demonstrated in a most striking manner, for the stream of hydrogen continues to bubble through the sulphuric acid bottle and into the U-tube for several minutes with its original rapidity, although all exit is prevented by the closing of the stopcock. At length, however, the occlusion diminishes, and the stream of hydrogen gradually becomes slower and slower, until it entirely ceases, the palladium having regained the temperature of the room, and become saturated with hydrogen at this temperature. If now the stopcock is opened, and the metal again heated, upon applying a flame to the jet, the issuing hydrogen evolved from the palladium takes fire, and burns with a tall flame which remains constant for some minutes, then, as the hydrogen stored in the palladium becomes exhausted, diminishes in size, and finally disappears. The moment the flame is removed occlusion instantly commences again, and the experiment may be repeated any number of times with undiminished effect.

THE additions to the Zoological Society's Gardens during the past week include a Herring Gull (*Larus argentatus*), six Common Gulls (*Larus canus*), five Black-headed Gulls (*Larus ridibundus*), British, presented by Mr. T. A. Cotton, F.Z.S.; a Roseate Cockatoo (*Cacatua roseicapilla*) from Australia, a Grey Parrot (*Psittacus erithacus*) from West Africa, presented by Mrs. Hennah; a Red-winged Parrakeet (*Aprosmictus erythropterus*) from Australia, presented by Lieut.-Colonel R. J. H. Parker, R.E.; a Cape Dove (*Œna capensis*) from South Africa, presented by the Rev. George Smith.

OUR ASTRONOMICAL COLUMN.

PHOTOGRAPHIC MAGNITUDES OF STARS.—The Astronomer-Royal communicated a paper "On the Relation between Diameter of Image, Duration of Exposure, and Brightness of Objects in Photographs of Stars taken at the Royal Observatory, Greenwich," to the Royal Astronomical Society in January. First, with regard to the relation between the diameter of the image of a star on the photographic plate and the time of exposure. Measures of a large number of stars have led to the development of the following empirical formula for the same star with different exposures—

$$\sqrt{d} = 1{\cdot}03 \log t + \text{const.},$$

where d = the diameter of a stellar image in seconds of arc, and t the exposure in seconds of time, the magnitude, m, being expressed on Pogson's scale. This relation represents the observations through a range of eight magnitudes, with a mean apparent error of less than one-twentieth of a magnitude. By using the measures from which the above formula was deduced, and photometric determinations of magnitude made at Bonn and Oxford, data were obtained for determining whether there was a constant relation between duration of exposure and brightness of star photographed, whether, in fact, for equal diameters of images—

$$\text{Exposure} \times \text{brightness} = \text{const.}$$

The relation found from the comparison was

$$0{\cdot}4 \times m = 0{\cdot}97 \log t + \text{const.}$$

This agrees very well with

$$0{\cdot}4 \times m = \log t + \text{const.},$$

in which 0·4 is the logarithm of the number expressing the magnitude-ratio. By combining the two relations developed, it follows that, for the same exposure—

$$\text{Magnitude of star} = \text{const.} - 2{\cdot}43 \times \sqrt{\text{diameter}}.$$

And finally the three formulæ are expressed by the following:—

$$m = 2{\cdot}5(\log t - 0{\cdot}97\sqrt{d}) + \text{const.},$$

connecting magnitude, diameter of image, and time of exposure.

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THE ZODIACAL LIGHT.—"The Zodiacal Light as related to the Terrestrial Temperature Variations" is the subject of a reprint from the *American Meteorological Journal* for November 1891, recently received from Mr. O. T. Sherman. In it the author endeavours to show that, when the temperature of the whole earth is considered, "the principal cause of variation is the difference in the sum of the local densities of the zodiacal light-forming matter which lies between us and the sun." Curves have been constructed to indicate five-yearly means of temperature variation from 1790 to 1884, five-yearly means for the zodiacal light, and the yearly auroral numbers for Europe south of the polar circle; but there are not sufficient data to permit the deduction of any very definite conclusion from them.

THE ANCIENT TOMBS AND BURIAL MOUNDS OF JAPAN.

AT a recent meeting of the China branch of the Royal Asiatic Society at Shanghai, Prof. Hitchcock, of the Smithsonian Institute, read a paper on the ancient tombs and burial mounds of Japan, in the course of which he said that, while the form and structure of the Japanese mounds were now known, thanks to the as yet unpublished researches of his companion in many journeys in Japan, Mr. W. Gowland, their early origin was yet to be traced. It was surmised that a few at least of the Japanese burial customs were derived from China. In the course of his own travels in the north of China he had failed to discover any indications of the existence of mounds like those in Japan; but he still expected to hear of them from some experienced traveller in the interior of that vast empire. Referring to the origin of the tombs, the lecturer said the first Emperor, who lived in the seventh century B.C., is supposed to be buried in Yamato, and the tombs of his successors are pointed out by the Imperial Household Department. The identity of the sepulchres may be questioned, but it is a fact that we can distinguish consecutive modifications of form apparently corresponding to successive periods of time.

Several distinct methods of interment have prevailed at different periods in Japan. They may be conveniently distinguished as follows: (1) burial in artificial rock caves; (2) in simple earth mounds, with or without coffins; (3) in rock chambers, or dolmens; (4) in double or Imperial mounds. The lecturer then proceeded to illustrate the appearance of these different kinds of mounds by the aid of photograph slides thrown on to a screen. He showed that the double mounds were invariably protected by a wide and deep moat, sometimes by two, and consisted of two distinct mounds with a depression between them. One of these double mounds, near Sakai, according to Japanese reckoning dates from about the fourth century. The height is about 100 feet, and the circuit of the base 1526 yards. The Emperor Kei Tai, who is reported to have lived in the sixth century, was one of the last emperors known to have been buried in a double mound. Some mounds have terraced sides, and this form is said to date from about the seventh century. Large quantities of clay cylinders were used for the purpose of preserving the terraces against the effects of the weather. When the covering of earth is removed, it is found that the stone chamber beneath, which contained the coffin, opens through passages often 40 feet and sometimes 60 feet long. The earth has in many cases been washed away from the mounds, exposing the rocks which are piled over the central chamber. According to a Japanese authority, in all the sepulchres the first order of performing the burials was the piling up of the earthen mound, leaving an underground tunnel leading from the outside to the very centre of the mound. This mound completed, the coffin, usually carved and made of stone, in which the corpse was placed, and sealed, was then introduced through the tunnel and placed in the centre of the mound, and the tunnel was then filled up with stones. The lecturer, however, said the coffins were not always introduced through the galleries, and the tunnels were certainly not filled up with stones, although their ends were probably closed with stones. He inferred from his own observations that the chambers were frequently, if not usually, built round the coffins. Stone and clay coffins had been found together in one cave, showing them to have been contemporaneous.

After showing a number of photographs of the pottery discovered in the mounds, he drew attention to a number of small clay figures representing human beings. He said it was a very ancient custom in Japan to bury the retainers of a prince standing upright around his grave. Like many other customs, this also came from China. In the time of the Japanese Emperor Suinin (97-30 B.C.), his younger brother died, and they buried all who had been in his immediate service around his tomb alive. "For many days they died not, but wept and cried aloud. At last they died. Dogs and crows assembled and ate them. The Emperor's compassion was aroused, and he desired to change the custom. When the Empress Hibatsuhime-no-Mikoto died, the Mikado inquired of his officers, saying: 'We know that the practice of following the dead is not good. What shall be done?' Nomi-no-Sukune then said: 'It is not good to bury living men standing, at the sepulchre of a prince, and this cannot be handed down to posterity.' He then proposed to make clay figures of men and horses, and to bury them as substitutes. The Mikado was well pleased with the plan, and ordered that henceforth the old custom should not be followed, but that clay images should be set round the sepulchre instead." Even as late as the year 646 an edict was published, forbidding the burial of living persons, and also the burial of "gold, silver brocade, diaper, or any kind of variegated thing." From this it might be inferred that the old custom of living burial was kept up, to some extent, even to the seventh century. The edict reads: "Let there be complete cessation of all such ancient practices as strangling oneself to follow the dead, or strangling others to make them follow the dead, or killing the dead man's horse, or burying treasures in the tomb for the dead man's sake, or cutting the hair, or stabbing the thigh, or wailing for the dead man's sake." The figures of clay thus introduced as substitutes for human sacrifices, and also to take the place of horses, are known as *tsuchi ningio*. Specimens of them are now very rare, and this fact leads to the supposition that the figures were not buried, but left exposed on the surface of the ground.

In the discussion which followed, Dr. Edkins pointed out the resemblance which existed between the stone relics found in Japan and China and in Europe, as indicating the existence of communication between distant lands in those days. It was also very interesting to note that, in the very earliest ages, men had been possessed with the idea of a future life for the soul.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—E. W. Hobson, M.A., Fellow and Lecturer of Christ's College, late Deputy Lowndean Professor, has been approved for the degree of Doctor in Science.

A decision on the subject of appointing lecturers in agricultural science will be taken by the Senate on February 25.

A meeting for the purpose of considering the propriety of erecting in Westminster Abbey a memorial to Prof. Adams has been summoned by the Master of St. John's, and will be held in the Combination room of that College on Saturday, February 20, at 3.30 p.m.

ST. ANDREWS.—At a meeting on Saturday, the 13th inst., of the Senatus Academicus of St. Andrews University, consisting of the Principals and Professors of the United College and St. Mary's College, St. Andrews, and University College, Dundee, it was unanimously resolved to confer the honorary degree of LL.D. upon Prof. Michael Foster, F.R.S., and Dr. Hugo Müller, F.R.S. The conferring of the degrees will take place in April.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, January 28.—"On certain Ternary Alloys. Part V. Determination of various Critical Curves, and their Tie-lines and Limiting Points." By C. R. Alder Wright, D.Sc., F.R.S., Lecturer on Chemistry and Physics in St. Mary's Hospital Medical School.

The author describes a number of "critical curves" obtained in accordance with the triangular system of representation proposed by Sir G. G. Stokes, whereby the composition of a given ternary mixture is represented by the position of the centre of gravity of three weights placed at the respective corners of an equilateral triangle, and respectively in the proportions of the three components of the mixture. With certain pairs of metals, *e.g.* lead and zinc, each will only dissolve the other to a limited extent when molten, so that a mixture of the two separates into two binary alloys, one containing chiefly lead with a little zinc, the other mainly zinc with a little lead, the exact proportion depending on the temperature of the mass. If a third metal, *e.g.* tin, be added to the mixture, such that this third, or

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"solvent," metal will mix in all proportions with either of the other "immiscible metals" separately, the ternary mixture produced either forms a "real" ternary alloy, not separating into two different mixtures on standing (molten), or else an "ideal" alloy incapable of existing, and immediately separating into two different ternary mixtures; *e.g.* one chiefly containing lead with some of the tin and a little zinc, the other mainly consisting of zinc with the rest of the tin and a little lead. The two points representing these two mixtures lie on opposite sides of the point indicating the total mass of the three metals originally used, this third point lying somewhere on the straight line connecting the other two "conjugate points," or "tie-line." By employing a series of mixtures containing gradually increasing proportions of the third "solvent" metal, two branches of a curve are gradually traced out representing the respective loci of the pairs of conjugate points; these two branches of the "critical curve" tend to meet at some point, termed the "limiting point," where the tie-lines vanish.

A large number of experiments were made to determine the conditions necessary to obtain the nearest possible approximations to the positions of truly conjugate pairs of points; former experiments having indicated that the compositions of the two alloys formed might be influenced by the relative proportions of the two immiscible metals, in such fashion that points slightly varying in position along one branch might be obtained as conjugates to a given point on the other branch under different conditions. It was found that *such variations entirely disappeared with thorough admixture*, which is more readily effected when the proportions used are such as to give rise to approximately equal quantities of the two alloys than when they are such that one alloy is formed to a much larger extent than the other. Observations with non-metallic analogous fluids (chloroform, water, glacial acetic acid) always gave sharply concordant values, thorough intermixture (by shaking vigorously in a stoppered bottle) being much more easy than with metals melted in a crucible and simply stirred vigorously.

Plotting the curves with the heavier immiscible fluid (lead, chloroform, bismuth) at the left hand corner of the base of the triangle, the lighter one (zinc, water) at the right hand corner, and the solvent fluid (tin, acetic acid) at the apex of the triangle, it was found that the tie-lines with chloroform-water-acetic acid, always sloped downwards to the left, the angle of slope continually increasing. The right hand branch of the critical curve rises to a maximum elevation, and then descends again to the limiting point, whilst the left hand branch continually ascends to that point. At the limiting point the chloroform and water are in the proportions $2CHCl_3$, $5H_2O$; the acetic acid present being less the higher the temperature, *i.e.* the critical curve for a higher temperature lying *inside* that for a lower one. At positions near the limiting point the mixture is extremely sensitive to temperature-variation; a few tenths of a degree will often make all the difference as to whether a single homogeneous fluid results (a "real" mixture), or two different ones, the point indicating the mixture in the first case lying just outside the critical curve, and in the second case inside it.

Analogous results were obtained with the metallic mixtures; with lead-zinc-tin mixtures, the lower ties slope to the left, the upper ones to the right, the limiting points also lying on the right and below the highest point of the critical curve; at this point the lead and zinc are nearly in the proportion $PbZn_8$. Two points, one in the left hand branch, the other in the right, are indicated by the configurations of the tie-lines, corresponding approximately with the definite atomic compounds $SnPb_3$ and $SnZn_4$ (when aluminium is used instead of zinc, similar configurations are developed, corresponding with $SnPb_3$ and $SnAl_4$). When silver is employed instead of tin, all the ties slope to the left; irregularities of outline (bulges) are noticed, caused by the formation of definite atomic compounds, $AgZn_5$ and Ag_4Zn_5: these bulges are still more pronounced when bismuth is used instead of lead (bismuth-zinc-silver curve). With both silver-lead-zinc and silver-bismuth-zinc curves, the limiting points lie to the *left* of and below the highest point of the critical curve; with both tin-lead-zinc and tin-bismuth-zinc curves, to the *right* and below the highest points.

In any given curve, substitution of bismuth for zinc *depresses* the curve, so that the curve with lead uniformly lies *outside* that with bismuth. The curve for a higher temperature always lies *inside* that for a lower temperature, the effect of temperature-variation being very much less marked at the lower parts of the curve, than at points lying anywhere near the limiting point.

February 4.—"On the Thermal Conductivities of Crystals and other Bad Conductors." By Charles H. Lees, M.Sc., the Owens College, Manchester.

The author commences by referring to Kundt's discovery, that the metals stand in the same order as conductors and as to the velocity of propagation of light through them, and mentions that his experiments were originally intended to furnish data for a similar comparison for crystals, but that their object has been extended.

After some preliminary experiments, he adopted the "divided bar" method, which consists in placing a disk of the material the conductivity of which is required, between the ends of two bars of metal placed coaxially, heating one end of the combination, and observing, by means of thermo-junctions applied to the bars, the distribution of temperature along them—first, with the disk in position; second, with the bars in contact without the disk. When the conductivity of the bar is known, these observations suffice to determine that of the disk.

The ends of the bars which came in contact with the disks were amalgamated, as this was found to be the best method of securing good contacts These bars were suspended horizontally in a frame, and, by means of screws, set accurately in the required position.

The conductivity of the bar was determined by the method due to Forbes of determining the loss of heat from the surface by allowing the bar to cool and observing the rate of change of temperature, and then observing the steady distribution of temperature along the bar when heated at one end.

The author finds it to be 0·27 C.G.S. unit, and to increase slightly with temperature.

The disks used were of the same diameter as the bar, and were of various thicknesses, in order to make the distribution of temperature throughout the bars nearly the same in each case.

The following are the results obtained. No relation of the kind found by Kundt for metals seems to hold for the crystals experimented on :—

	C.G.S. units.
Crown glass	0·0024
Flint glass	0·0020
Rock salt	0·014
Quartz along axis	0·030
,, perpendicular to axis	0·016
Iceland spar along axis	0·010
,, ,, perpendicular to axis ...	0·0084
Mica perpendicular to cleavage	0·0016
White marble	0·0071
Slate	0·0047
Shellac	0·00060
Paraffin	0·00061
Pure rubber	0·00038
Sulphur	0·00045
Ebonite	0·00040
Gutta-percha	0·00046
Paper	0·00031
Asbestos paper	0·00057
Mahogany	0·00047
Walnut	0·00036
Cork	0·00013
Silk	0·0002
Cotton	0·0006
Flannel	0·0002

Chemical Society, January 21.—Prof. W. A. Tilden F.R.S., Vice-President, in the chair.—The following papers were read:—The estimation of oxygen dissolved in water, by M. A Adams. The author describes an apparatus in which the estimation of dissolved oxygen in water by Schützenberger's method may be carried on so as entirely to avoid loss of oxygen by diffusion. The results obtained by this method are liable to differ according to the rate at which the determinations are effected, higher results being obtained when the operations are quickly performed.—The luminosity of coal-gas flames, by V. B. Lewes. The author has quantitatively studied the actions which occur in luminous gas flames, and also those which lead to loss of luminosity in the flame of a Bunsen burner. He considers that the most accurate method of dividing a luminous hydrocarbon flame into zones, is to regard it as made up of three:—(1) The inner zone, in which the temperature rises from a

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comparatively low point at the mouth of the burner to about 1000° C. at the apex of the zone; in this portion of the flame various decompositions and interactions occur, which culminate in the conversion of the heavier hydrocarbons into acetylene, carbon monoxide being also produced. (2) The luminous zone, in which the temperature varies from 1000° to a little over 1300°. The acetylene is here decomposed with liberation of carbon, which imparts luminosity to the flame. (3) The extreme outer zone, in which the combustion is practically complete. The various actions which tend to cause the loss of luminosity in a Bunsen flame may be summarized as follows:—(1) The chemical action of the atmospheric oxygen, which causes loss of luminosity by burning up the hydrocarbons before they, in their diluted condition, can afford acetylene. (2) The diluting action of the atmospheric nitrogen, which, by increasing the temperature necessary to bring about the partial decomposition of the hydrocarbons present, prevents the formation of acetylene, and so causes non-luminosity. (3) The cooling influence of the air introduced, which is able to add to the general result, although the cooling is less than the increase in temperature brought about by the oxidation due to the oxygen in the air. (4) In a normal Bunsen flame the nitrogen and oxygen are of about equal importance in bringing about non-luminosity, but if the quantity of air be increased, oxidation becomes the principal factor, and the nitrogen practically ceases to exert any influence.—The origin of flame coloration, by A. Smithells. This paper was reported in NATURE of January 28, (p. 306).—Note on the action of dilute nitric acid on coal, by R. J. Friswell. If bituminous coal be treated with 49 per cent. nitric acid, a black substance is obtained which is almost completely soluble in sodium carbonate solution and behaves as a nitro-acid; on treating this substance further with nitric acid, a brown acid is obtained which has not yet been examined.—A pure fermentation of mannitol and dulcitol, by P. F. Frankland and W. Frew. The authors have obtained an organism which sets up a fermentative decomposition, not only of mannitol, but also of dulcitol, a substance which has hitherto resisted fermenting bacteria. The products of the activity of this organism are essentially the same in the case of both sugars, consisting of ethyl alcohol, acetic acid, succinic acid, carbon dioxide, hydrogen, and varying quantities of formic acid. The decomposition may be represented by two sets of changes, viz. (*a*) $C_6H_{14}O_6 = 2C_2H_6O + CO_2 + CH_2O_2$, and (*b*) $C_6H_{14}O_6 = C_4H_6O_4 + C_2H_4O_2 + 2H_2$. It appears that two molecules of the sugar are resolved in accordance with equation (*a*) for every one decomposed according to (*b*). The organism is termed *Bacillus ethaceto-succinicus*.—Synthesis of hexahydroterephthalic acid, by J. E. Mackenzie and W. H. Perkin, Jun. The sparingly soluble hexahydroterephthalic acid of Von Baeyer is obtained by eliminating two molecules of carbon dioxide from hexamethylene-tetracarboxylic acid.—The magnetic rotation of dissolved salts, by W. Ostwald. The author considers that the values obtained by Perkin for the magnetic rotatory power of the ammonium salts of fatty acids in solution are in accord with the electrolytic dissociation theory. This theory does not lead to any numerical value or sign of the difference between the observed and calculated values of the magnetic rotation of electrolytes. In the case of haloid acids and salts the variation is positive; in the case of the oxy-acids negative. It is, therefore, not surprising that molecules with nearly zero variations are capable of existence.—The dissociation of nitrogen peroxide, by W. Ostwald. Using the data given by Cundall, the author has compared the extent to which nitrogen peroxide undergoes dissociation when vaporized, with that which it suffers when dissolved in chloroform. From the results he concludes that it behaves in accordance with Van 't Hoff's generalization that dissolved substances obey the same laws as gases. It appears, however, that in the gaseous state dissociation is far more advanced than in a chloroform solution.—Corydaline, by J. J. Dobbie and A. Lauder. The authors have obtained corydaline in colourless crystals melting at 134° 5. The results obtained on analysis do not agree with Wicke's formula, $C_{18}H_{19}NO_4$, but rather with $C_{22}H_{23}NO_4$. Analyses of the hydriodide, platinochloride, and methiodide of the alkaloid also give numbers agreeing with this formula.—Silver compounds of thiourea, by J. E. Reynolds. The author has obained a series of crystalline compounds of thiourea with silver nitrate, bromide, chloride, iodide, and cyanide. These substances contain one, two, or three molecules of thiourea to one of the silver salt, and readily afford silver sulphide when heated a few degrees above their melting-points.

Zoological Society, February 2.—Mr. W. T. Blanford, F.R.S., in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of January 1892.—Mr. W. Bateson, F.R.S., exhibited some Crabs' claws bearing supernumerary prongs. It was shown that these extra parts are really complementary (right and left) pairs of indices or pollices, according to their position of origin, and not repetitions of the two pincers of the claw, as was commonly believed.—Mr. Sclater made some remarks on the breeding of the Ground-Pigeons (*Geophapes*) in the Society's Gardens, and showed that the young of these Pigeons, when first hatched, were not materially different in point of development from those of the typical Pigeons, and that there was consequently no ground for separating the *Geophapes* from the order Columbæ on this accouut, as it had been recently proposed to do.—A letter was read from Prof. R. Ramsay Wright, inclosing some photographs of the heaps of skulls of the American Bison which are collected on the plains of the Saskatchewan, and piled up at the sidings on the Canadian Pacific Railway, awaiting transport, and which testify to the enormous number of these animals recently exterminated.—Mr. W. Bateson gave a summary of his recent observations on numerical variation in teeth. The facts given related chiefly to specimens of Quadrumana, Carnivora, Marsupials, and other orders of Mammals in the British and other Museums. The author pointed out that the ordinarily received view of homologies between teeth is based on the hypothesis that the series is composed of members each of which is either present or absent. In the light of the facts of variation, this hypothesis was shown to be untenable, and an attempt was made to arrive at a more just conception of the nature of the homology of multiple parts.—Mr. R. Lydekker described part of an upper jaw of a Sirenian Mammal from the Tertiaries of Northern Italy, containing milk-teeth. As these teeth showed a masked Selenodont structure, it was urged that the specimen indicated the descent of the Sirenia from Selenodont Artiodactyle Ungulates. It was incidentally shown that *Halitherium veronense*, Zigno, from the same deposits, belongs to *Prorastomus*, Owen.—A communication was read from the Rev. H. S. Gorham, containing descriptions of and notes on the Coleoptera collected by Mr. John Whitehead on Kina Balu, Borneo. The present communication related to the families Hispidæ, Erotylidæ, Endomychidæ, Lycidæ, Lampyridæ, and others.—Another communication from the Rev. H. S. Gorham and Mr. C. J. Gahan gave an account of some of the Coleoptera collected by Mr. W. Bonny in the Aruwimi Valley, Central Africa.—Mr. P. L. Sclater, F.R.S., read some notes on a small collection of Mammals brought by Mr. Alfred Sharpe from Nyassaland, amongst which was a flat skin of Angas's Bush-bok (*Tragelaphus angasi*), a species of Antelope not hitherto recorded to occur in this district.—Mr. Sclater also gave the description of a new Antelope from Somali-land, proposed to be called *Bubalis swaynii*, after Captain H. G. C. Swayne, R.E., who had furnished him with the specimens on which it was based. He likewise exhibited and remarked on some other examples of Antelopes from the same country contained in Captain Swayne's collection.

Linnean Society, February 4.—Prof. Stewart, President, in the chair.—Mr. J. E. Harting exhibited Gould's coloured plate of a humming-bird (*Phæthornis longuemareus*), of which species a pair had made their nest in the drawing-room of Mr. Hamilton, of Queen's Park, Trinidad. The nest was built in a palm about five feet high, standing in a tub within the room. The first egg was laid on December 27 last, the second egg on December 29, and a young bird was hatched on January 12. The circumstance was regarded as quite unprecedented, though Mr. D. Morris was able to quote a case which came under his own observation in Jamaica, wherein a humming-bird had built its nest on the extremity of a saddle-bar in a verandah.—Mr. Harting also exhibited some life-sized photographs of the egg-cases of two species of dog-fish (*Scyllium*), and made some remarks on the mode of deposition and period of incubation as observed in different aquaria.—Mr. F. N. Williams read a paper on the genus *Dianthus*. He pointed out that *Velesia*, *Dianthus*, and *Funica* form a natural group of genera distinguishable from the Silene group by their seeds, which have a facial hilum and straight embryo. *Velesia* may be distinguished from *Dianthus* and *Funica* by having half the number of stamens. There are, however, three characters to be relied on in distinguishing these two genera; (1) the presence of a

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spicalyx of bracts; (2) the number of nerves to the calyx; (3) the junction of the claw with the blade of the petal. This last character was regarded as distinguishing very clearly *Dianthus* from *Funica*. In *Dianthus* the blade of the petal is abruptly narrowed into the claw, so that the two are distinct; in *Funica* the transition is gradual. Mr. Williams was of opinion that the species of *Dianthus* might be arranged in three natural groups (sub-genera): (1) in which the flowers are numerous and clustered, as in "Sweet William"; (2) the largest group, in which the flowers are few and usually solitary on the branches of the stems, as in carnations; and (3) a small group intermediate between *Funica* and the true pinks, and corresponding with the genus *Kohlrauschia* of Kunth. The number of species recognized by Mr. Williams in this monograph amount in round numbers to 250.—A paper by Messrs. G. J. Hinde and W. M. Holmes was then read, on the Sponge remains in the Lower Tertiary strata near Oamaru, Otago, New Zealand. Near Oamaru there are beds of white, friable siliceous rock of Upper Eocene age, almost entirely composed of Sponge spicules, Diatoms, and Radiolaria, thus resembling in character the Diatom and Radiolarian ooze of the present deep seas. The Sponge remains are all detached; they belong largely to the *Monactinellidæ*, though *Tetractinellid*, *Lithistid*, and *Hexactinellid* spicules are also present. The smaller flesh spicules of these different groups are perfectly preserved, and thus enable a comparison to be made with existing Sponges, to which generically they mostly belong. In all 43 genera and 113 species have been recognized by their characteristic spicules. Many of the forms have not hitherto been known as fossil. The existing relatives of many of them now inhabit the Indian and Southern Oceans, but some are at present only known from the North Atlantic. The remains of deep-water Sponges are intermingled in the deposit with others hitherto supposed to belong to moderate depths only, but in recent dredgings by H.M.S. *Egeria* off the south-west coast of Australia, at a depth of 3000 fathoms, there is a corresponding admixture of similar spicules.

PARIS.

Academy of Sciences, February 8.—M. d'Abbadie in the chair.—Observations on a note by M. H. Le Chatelier, on the optical measurement of high temperatures, by M. Henri Becquerel.—Silica in plants, by MM. Berthelot and G. André. An examination into the occurrence, distribution, and state, of silica in the spring-wheat plant in various stages of its growth, and into the quantity and state of the silica in the soil in which the plants were grown.—A new chart of the currents of the North Atlantic, by Prince Albert I. of Monaco.—Determination of the freezing-point of very dilute solutions: application to cane-sugar, by M. Raoult. The author describes a new method of taking cryoscopic observations, giving readings to within 1/500 of a degree. The molecular lowering of the freezing-point of water by cane-sugar is represented by a curve, which demonstrates that the author was correct in asserting that sugar gave, like other bodies, a gradual increase in its molecular lowering beyond a certain stage of dilution.—New measurement of the Perpignan base, by General Derrécagaix. The results are as follows: (1) the measured line, reduced to Delambre's base, gives for the length of this base 11,706·69 m.; (2) this length is verified by the geodesic accordance between the segments and the entire base, to an approximation of 1/250,000. Hence (1) the modern measurement of the Perpignan base makes it longer by 0·29 m. than found by Delambre using Borda's measures; (2) this new length is less than that calculated from the Paris base by only 5 cm.—New researches on the solar atmosphere, by M. H. Deslandres.—A new interpretation of Abel's theorem, by M. Sophus Lie.—On the integrals of equations of the first order which admit of only a finite number of values, by M. Paul Painlevé.—On a new process for the transmission of electric undulations along metallic wires, and a new arrangement of the receiver, by M R. Blondlot.—Refraction of liquefied gases, by M. James Chappuis. The indices of refraction at zero and under their maximum vapour pressure have been found for sulphurous acid and methyl chloride. For D they are respectively 1·3518 and 1·3533.—The rotatory power of quartz for the infra-red rays, by M. E. Carvallo.—The action of chlorine upon ruthenium: ruthenium sesquichloride and oxychloride, by M. A. Joly.—On a nitro-silicate of silver, and on the existence of a nitro-silicic acid, by MM. G. Rousseau and G. Tite.—On the decomposition of sulphurous acid by carbon at very high temperatures, by M. Scheurer-Kestner. Sulphurous acid passed over charcoal at a white heat is decomposed in accordance with the equation—

$$2SO_2 + 3C = 2CO + CO_2 + 2S.$$

—Chlorosulphide and bromosulphide of lead, by M. F. Parmentier. The chlorosulphide, obtained by a method described, is found to be $PbS.PbCl_2$. Its colour, when suspended in water, is cinnabar-red; when collected, it appears darker. The properties of the bromosulphide, $PbS.PbBr_2$, are similar, but it is more stable than the chlorosulphide. The existence of an iodosulphide is indicated.—Researches on sodium isopropylate, by M. de Forcrand.—On a nitro-derivative of antipyrin, by M. Edm. Jandrier.—On the rotatory power of diacetyltartric derivatives; reply to a note by M. Colson, by M. J. A. le Bel.—On the minimum perceptible amount of some odours, by M. Jaques Passy.—The law of absorption of carbon monoxide by the blood of a living mammal, by M. N. Gréhant.—On the fauna of the fresh waters of Iceland, by MM. Jules de Guerne and Jules Richard.—On the structure of the ovule and the development of the embryonic sac of *Dompte-venin* (*Vincetoxicum*), by M. Gustave Chauveaud.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Stanford's Handy Atlas of Modern Geography (Stanford).—The London Matriculation Directory, No. xi. January 1892 (Clive).—Synopsis of Non-metallic Chemistry: W. Briggs (Clive).—Pocket-book of Electrical Rules and Tables, 8th edition: Munro and Jamieson (Griffin).—Philosophical Notes on Botanical Subjects: Dr. E. Bonavia (Eyre and Spottiswoode).—The First Book of Euclid's Elements: Rev. J. B. Lock (Macmillan).—Hourly Means, 1888 (Eyre and Spottiswoode).—Recollections of a Happy Life, being the Autobiography of Marianne North, 2 vols.: edited by Mrs. J. A. Symonds (Macmillan).—Observations made at the Magnetical and Meteorological Observatory at Batavia, vol. xiii., 1890 (Batavia).

PAMPHLETS.—Aboriginal Skin-dressing: O. T. Mason (Washington).—Ten Years' Sunshine in the British Isles, 1881-90 (Eyre and Spottiswoode).—Harmonic Analysis of Hourly Observations of Air Temperature and Pressure at British Observatories (Eyre and Spottiswoode).

SERIALS.—Proceedings of the Royal Society of Edinburgh, vol. xviii., pp. 261-374 (Edinburgh).—Transactions of the Burton-on-Trent Natural History and Archæological Society, vol. ii. (Bemrose).—The Transactions of the Yorkshire Naturalists' Union, Parts 10 to 16 (Leeds, Taylor).—The Engineering Magazine, February (New York).—The Geological Magazine, February (K. Paul). Quarterly Journal of Microscopical Science, new series, No. 130 (Churchill)

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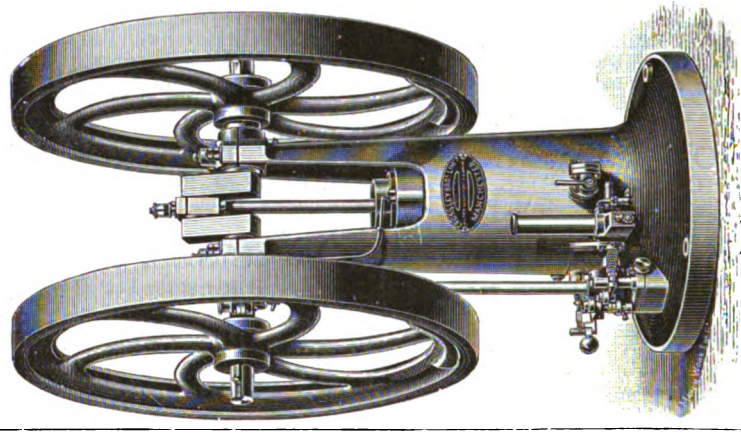

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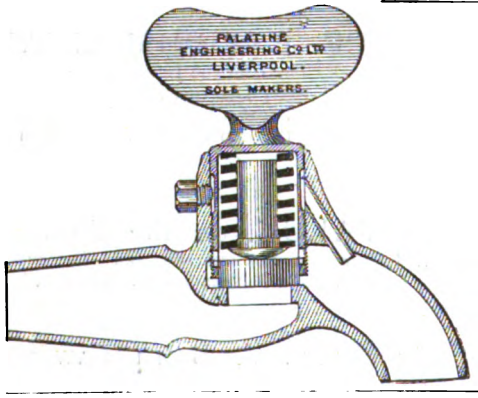

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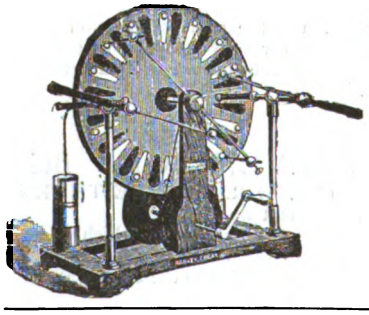

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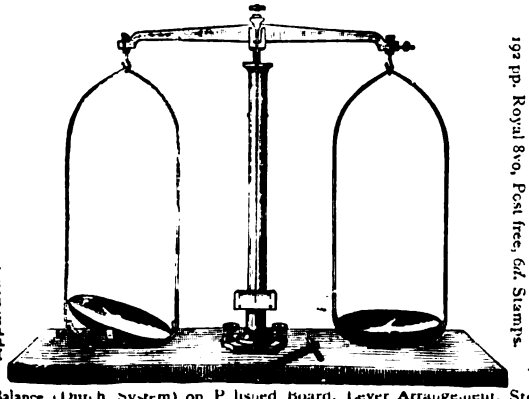

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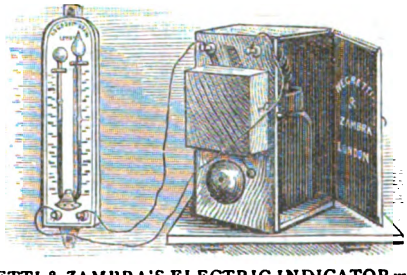

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THURSDAY, FEBRUARY 25, 1892.

THE SCIENCE MUSEUM AND THE TATE GALLERY.

WE are informed that various Government Departments are now considering the many questions raised by the Tate Gallery controversy in the real spirit of scientific inquiry—that is, they are at last attempting to deal with the facts, and they are consulting some people who ought to know something about them.

As the President and Officers of the Royal Society, and many other representative men throughout the country, have stated their opinion on the matter to Lord Salisbury in a memorial last year; and as they were pretty certain to have known the facts before they committed themselves to any statement involving them in great responsibility if they were wrong; the friends of science can only rejoice that at last the Government Departments are dealing with the matter in a scientific way.

The facts are as follows. In our Museum arrangements, while books and pictures—or, in other words, literature and art—are thoroughly represented, from the Museum point of view, on a scale worthy of the nation, only three branches of science—natural history, geology, and mineralogy—have so far been provided for, and by the Natural History Museum. This was pointed out about twenty years ago by the Duke of Devonshire's Commission; and the many advantages which would be certain to accrue to an industrial nation like ours, from collections representing the various physical, chemical, mechanical, and metallurgical sciences and their applications to industry, were very clearly referred to.

The Government, one may almost say, of course, took no notice of this important recommendation, or next to none. In 1876 the Royal Commissioners for the Exhibition of 1851—a body thoroughly well capable of judging of the importance of the Duke of Devonshire's recommendation—showed their opinion of its importance by offering a plot of ground stretching from Queen's Gate to Prince's Gate, and £100,000 to build this Science Museum. This offer, the Government of the day—most of the Governments of the day knowing and caring little about science, although they are so glad to receive donations for art—declined. So matters went on, Committee after Committee being appointed, reporting, and having their deliberations ignored, until at last, as a result of a Committee appointed by the Treasury itself, the plot of ground which the Government might years ago have had for nothing, was purchased, and purchased, of course, in strict relation to the discussions which, as we have shown, had been going on for the last eighteen years. At least, if that be not so, why was it purchased at all?

We now come to the plot of ground. It contains 350,000 square feet. What facts have we before us to aid us in answering the question as to whether this plot of ground is sufficient to do for all other sciences what the Natural History Museum does for three? Here, of course, if possible, we must appeal to facts, if facts exist. Let us assume that the new Museum is to be built on the precedent afforded by the Natural History Museum. The exhibiting space in the Natural History Museum consists of 152,000 square feet. To provide this exhibiting space in a building in which, of course, many other things besides exhibiting space have to be considered, the ground plan of the existing building contains an area of 162,000 square feet. The building itself, however, stands in a plot of ground covering about 500,000 square feet, and in this way ample possibilities of future extension are provided.

We are first, then, face to face with the fact that, assuming the Museum which is to do for all the other sciences what the Natural History Museum does for the three named, can, in a few years, be restricted to the same exhibiting area; we begin with a space of, roughly, 150,000 square feet *less* for that which must eventually be the *larger* Museum. Anyone will see that, in the nature of things, it is unlikely that the Museum which has to look after the interests of all the inorganic sciences, and their infinite applications to the arts (by which, of course, the industrial arts are meant), will always remain smaller than that Museum which has to provide for biology chiefly. But the matter, unfortunately, does not end here. It is practically certain that the Royal College of Science will require considerable additions to meet the demands made upon it for science teachers. It is perfectly well known already to all educationists that it has already entirely overflowed the small and ill-contrived building in which it is located; and the Professors of Physics and Geology are already camping out in temporary buildings of the most inconvenient description. We believe it is thoroughly conceded by the Government that new laboratories for physics, astronomical physics, and chemistry must be at once erected. Now, it is perfectly certain that, at this time of day, such laboratories as those, with their various appendages, cannot be erected on a space much less than 100,000 square feet; so that it comes to this, a site already too small for the purposes of a Science Museum and its future extension—if it is to be treated like the Natural History Museum—must have filched from it, for other scientific needs, another slice of 100,000 square feet.

This may be, perhaps, permitted, because, although the Science Museum may lose in area, it would gain enormously in advantage, for the reason that the teaching in the future may be given in close relation with the instruments which are used in it.

The suggestion that Mr. Tate should be allowed still another 100,000 square feet from this plot—from a plot containing less than 300,000 square feet available for building purposes—of course was preposterous from the beginning, and would have been at once shown to be impossible if the problem had been considered at all from the region of hard, dry fact. Unfortunately, it was not so considered, and hence an apparent antagonism between the interests of science and art, which has been going on now nearly a year. We have attempted to show that the question is not a question of sympathy—it is a question of fact; and now that the Chancellor of the Exchequer is dealing with the facts, as they ought to have been dealt with in the first instance, there can be no longer any doubt as to which way the issue will be decided.

We believe it is contended by some who are not acquainted with the exact terms of reference to the last

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Treasury Committee that their very indefinite suggestion as to the space to be provided to meet present needs really referred to their view as to the maximum amount of space which a Science Museum should be allowed to cover. We are certain that the scientific members of that Committee would repudiate any such opinion, and they would quote, as we have quoted above, the actual facts concerning the Natural History Museum as negativing any such idea.

Finally, it is advisable to point out the terrible want of any sense of perspective on the part of the Legislature on matters relating to science and art. We do not object to any expenditure the Government may choose to make upon art, but it is our clear duty to point out that the interests of science must not be neglected in order that art may be encouraged. The £70,000 paid for the land which has given rise to all this discussion is the same sum as that given not long ago for a single picture. The "British Luxembourg"—whatever that may be—which the Government is supposed to be now fostering, was intended to contain most of the pictures now in the South Kensington Museum; so that the edifying spectacle was to have been, or indeed may yet be, seen, of emptying all the picture galleries at one end of the Museum while £400,000 of public money is being expended—this has been agreed to by the Government with alacrity—in building new picture galleries at the other. Nor is this all. The Tate Gallery, if built, is to be maintained by the Government: we are informed this will cost at least £4000 annually. Here, then, is another endowment of, say, £160,000 for art. We do not object to this if the nation so wills it; but is it wise that all this time the training of our science teachers—our great requisite just now—is to be carried on in sheds, and that the only concern that the Government shows in relation to the Science Museum, which is to include the Patent Museum, is to still absorb year after year the patent fees, which ought in justice to be used for the improvement of our national industries?

CHEMICAL TECHNOLOGY.

Manual of Chemical Technology. By Rudolf von Wagner. Translated and edited by William Crookes, F.R.S., from the thirteenth enlarged German edition, as remodelled by Dr. Ferdinand Fischer. With 596 Illustrations, 968 pages. (London: J. and A. Churchill, 1892.)

WHEN a book has reached a thirteenth edition, it may usually be assumed that its form and contents are so familiar to all who are likely to be interested in the subject of which it treats as to require no description, and that it has attained so substantial a reputation as to be independent alike of praise or blame flowing from the pen of the critic. In the case of the book before us, however, the author is no longer living; and having completed in 1880 the eleventh edition of his work, the care of future issues devolves upon others. There is probably no subject which changes so rapidly, owing to the advances of science and the incessant activity of inventors, as chemical technology, or the application of chemistry to useful purposes in the industrial arts; and a glance at the table of contents of such a book as this is sufficient to remind one of the vast range of subjects with which chemistry has to do. It is the extent and diversity of these subjects which suggests at once the idea of the difficulties which must attend the compilation of such a treatise even when the work of so distinguished a technologist as von Wagner is carried forward by editors so eminent as Dr. F. Fischer and Mr. Crookes. That they have discharged their duty in no perfunctory spirit is evident from the fact that many sections are entirely new, and that, as compared with the eleventh German edition, more than half the text and the illustrations have been replaced by new matter. And further, as pointed out by the English editor, a manual such as this must be in many respects adapted to the conditions of the country where it is written, and, if translated for use elsewhere, it requires modification. The prices of raw materials, of fuel, and of labour, and the laws in different countries have in each case their influence upon the conditions under which chemical industries are carried on. As already remarked, a book in its thirteenth edition must have been found useful by a good many people, but the first thought that passes through the mind in turning over the pages of Wagner's "Technology" is—For whom can this book have been intended? It treats of everything: of fuel and metallurgy, of water, acids, and alkalies, of pigments and dyes, of glass and cements, of food and fibres, of leather, soap, wood, matches, and many smaller subjects. In the limits of 950 pages it is not possible to provide all the details essential to each process which would be sought by a manufacturer, and an examination of the volume shows that the treatment of the successive subjects is very unequal. The impression derived from its perusal is that, on the whole, it is most likely to be useful to senior students or to chemists who wish for general information relating to chemical manufactures, but that the efforts which have evidently been made to incorporate into this new edition an account of modern processes are spasmodic, and not always successful.

The articles on soda and on chlorine, for example, are among the best in the book. The several processes for the recovery of sulphur from alkali waste, including the Schaffner-Helbig and the Chance patent processes, are described with some detail; and the Weldon-Péchiney process for obtaining chlorine from magnesium chloride is described at length with the aid of numerous illustrations. But even in this latter case, in view of the importance of the process, which is still on its trial, information more recent than December 1887, might have been expected. There is no reference to more recent processes for electrolyzing alkaline chlorides; but this, perhaps, is expecting too much in the way of the "latest intelligence"; and doubtless the same remark applies to the absence of any description of liquid chlorine as a commercial article. This, however, does not account for the curious error by which the number formerly given as the density of liquid chlorine, 1·33, is assigned to the gas. The admirable work of Knietsch (*Annalen*, 259) on the properties of liquid chlorine deserves to be had in remembrance, more especially as it was done in the laboratory of an alkali-works, with industrial objects in view.

A really unsatisfactory article is that which relates to

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iron and steel. The ores are stated to be roasted in heaps and in special kilns, but no description is given of the process. The blast-furnace to which the longest description is devoted is an open-top furnace of forty years ago. No figure is given of a modern English blast-furnace; the form shown in the cut, and dismissed in eight lines of text, is not to be seen in this country. The cup and cone charger, general in Great Britain, is not even mentioned. There is no straightforward account of the successive chemical changes which are supposed to take place as the charge descends in the furnace, but, without introduction, the reader is plunged into a disproportionately long discussion of the "heat conditions of the blast-furnace." There is no description of the *tuyères*, though it is mentioned that they are cooled by water, nor of the sand casting-bed, nor of the resulting pigs. In the account of "crude" iron, as it is called, it is surely not enough to say that, "according to the nature of the carbon, it is distinguished as white and grey crude iron," and then to give just six lines to each of these two varieties, without so much as mentioning the various grades distinguished by the numbers familiar in every ironworks, or the purposes in the forge or the foundry to which they are severally applicable. Following the description of "crude" iron is a section occupying about three-quarters of a page on the examination of iron and steel, which might as well have been omitted, as it is quite useless. The same inadequate treatment is noticeable throughout the article, and to an English reader it must appear strange that English metallurgists, with the exception of Bessemer, Thomas and Gilchrist, and Bell, whose name appears once, are generally ignored. This seems to be one of the articles in which the English editor has not been sufficiently careful to supply the modifications indicated in the preface as requisite in order to adapt the book to the conditions prevailing in this country.

Turning now to subjects of a different character, there is the vast field of coal-tar and the colours derived from the tar hydrocarbons. The colours are dealt with in sixty-seven pages. In comparison, therefore, with other subjects in the same volume, this important and interesting department of applied chemistry receives perhaps its fair share of space; but, recalling the fact that in another book on applied chemistry recently published nearly the same number of pages is devoted to naphthalene and its derivatives alone, it is obvious that considerable condensation must have been effected in the work before us. In connection with this article, every chemist who has any feeling for consistency in chemical nomenclature must protest against the playful variety of spelling which the editor permits in the names of the hydrocarbons. On the same pages we have paraff*ine*, benz*ol*, naphthal*ine*, anthrac*ene*, triphenyl meth*an*. This is really too bad!

Referring to the thirty pages devoted to dyeing and tissue-printing, we find a practical acknowledgment of the inadequacy of the treatment which many subjects receive by the insertion of a list of books to which the reader is referred for further information. And this recalls the fact that elsewhere throughout the volume references to authorities in the shape of original memoirs, books, or patent specifications are rarely given. This is an omission which might with great advantage be repaired in a future edition, not only because further information is often essential, but because there are, necessarily, scattered up and down the pages of such a comprehensive work as this a good many statements which require some qualification, or, at any rate, some positive evidence to make them completely acceptable, as well as others which are manifestly antiquated and obsolete. For example, the statements (p. 223) as to the influence of the addition of aluminium to iron and steel ought not to be put forward without proof. Certainly the fact that the manufacture of pure aluminium is now practically abandoned tends to show that it has not been found so valuable in connection with iron and steel as was at one time asserted.

There is no very pressing necessity for introducing photometry into a book on chemical technology; but if there is a reason for its introduction, there must be a still stronger reason for making the description practical. It is not so in this case, for while Bunsen's photometer is referred to, rather than described, in eight lines, Violle's attempt to utilize the light emitted from melted platinum as a photometric unit is described at length, with a figure, and the article concludes with the statement that the amyl acetate lamp will probably in time supersede the other units. We think not.

Manufacturers who have managed to keep the details of their plant and their processes practically secret must derive some amusement from books which profess to describe their manufactures. The successful English monopolists of phosphorus-making (Messrs. Albright and Wilson) are represented, no doubt correctly, as making more phosphorus than anybody else; but we can imagine the quiet smile with which they would regard the pictures of apparatus said to have been invented and employed by themselves, as well as such ridiculous statements as (p. 410) that "bone ash is now the only material used by phosphorus-makers," &c. If it were not for the lofty air of knowledge with which such things are described, it would not matter greatly; but it would be more candid towards the reader if the writer of such an article as that on phosphorus would begin by declaring roundly that he does not *know*, but only *imagines*, that the following is the process likely to be employed.

The difficulty of purifying the pages of successive editions of so large and complex a book of phraseology which is obsolete or inappropriate is illustrated on p. 398, where a brief exposition is given of the views entertained by "the late Dr. Gerhardt" concerning the constitution of fulminating mercury. Not a word is mentioned of more recent chemical discussions on the fulminates, though they have been renewed more than once in the five-and-thirty years which have elapsed since the death of the distinguished French chemist.

The editors have produced a volume which contains much entertaining and instructive reading. From what has been said, however, it is obvious that the reader must not expect to find the whole truth, and nothing but the truth, set forth in any such cyclopædic production. It is not possible for any one man, or any three men, to array, without mistake, the accumulated stores of human knowledge and experience in such a subject as applied chemistry.

W. A. T.

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AN AGRICULTURAL TEXT-BOOK.

Elements of Agriculture. A Text-book prepared under the authority of the Royal Agricultural Society of England. By W. Fream, LL.D. Pp. 450, 200 Figures. (London: John Murray, 1892.)

DR. FREAM'S new book, the first edition of which was exhausted on the day of publication, is a distinct gain to agricultural literature. We naturally expect such a work from Dr. Fream's pen to have the botanical portions of the subject dealt with at considerable length, and in this we are not mistaken, half the book being taken up by the discussion of the various plants with which the farmer has to deal, either as crops or weeds. This division has to be made, to some extent, at the expense of other branches of the subject, which is perhaps not quite a desirable thing to do in a general text-book; though it might be argued that it is far better to do one thing well than to do several things indifferently, and Dr. Fream has certainly done one thing well in his treatment of "The Plant." The style is good, and the descriptions very lucid, concise, and sound, the book containing a vast store of information in small compass. There is not, however, a single reference to any original paper, an omission especially to be regretted when the author is discussing the Rothamsted experiments; in fact, we think that the addition of a list of the papers published by Lawes and Gilbert would have considerably enhanced the value of the book.

The author divides his work into three parts. Part I., which occupies the first 77 pages, treats of the origin and properties of the different kinds of soils, their improvement, cultivation, and manuring. Part II., "The Plant," occupies a full half of the book (pp. 78–302). The first two chapters of this part treat of seeds and their germination, and the structure and functions of plants, these subjects being discussed in popular, but very instructive fashion.

Chapter xi. treats of the cultivated plants of the farm, grouped under the natural orders to which they belong. This is the longest chapter in the book, and includes 30 pages given to the consideration of the natural order Gramineæ. Then follow a short chapter on weeds, and chapters upon harvest machinery and the management of grass land. In chapter xvi., the theory of rotations is ably dealt with; the various crops of the farm, with their cultivation, are considered in turn; this chapter also contains a handy table of the characters of the seeds of the common farm crops. In the second column of this table, opposite *mangel*, is the statement, "Should germinate at least 120 per cent. (see p. 141)." On turning to p. 141 we find the following:—

"The seed of commerce (mangel) consists of the ovary with its seeds, embedded in the swollen base of the perianth, which thickens and hardens as it ripens, becoming angular and somewhat woody. Hence, when a mangel or beet *seed* is set to germinate, it is not unusual for two or three shoots to appear from a single seed."

On p. 97 are some remarks on this very matter, with definitions of fruit and seed: "The *fruit* is the ripened ovary, the *seed* is the fertilized and ripened ovule." Is it not better to adhere to these definitions, and thus avoid speaking of the germination of a sample of seed as *over 120 per cent.*?

On p. 282, Dr. Fream speaks positively upon the much-debated practice of burning diseased potato-haulms. Further research upon the liability of old haulms to propagate the fungus (*Phytophthora infestans*) is much needed. The writer's own observations by no means lead him to consider them so dangerous in spreading disease as some people think they are. The question of the utility of sulphate of copper as a remedy for potato disease is dismissed in very few words. It is to be hoped that the subject will receive fuller treatment in succeeding editions, as many trials have recently been made; and although these have not always been attended with success, many of the failures reported turn out upon investigation to be due to the fact that the Bouillie Bordelaise (or other mixture) was improperly prepared or improperly applied. It would appear that impurities in the mixture exert considerable influence upon its efficacy as a check on the growth of the fungus. The use of a mixture of sulphates of iron and copper with lime generally gives negative results; and even the presence of ferrous sulphate in any quantity in commercial bluestone appears at times to exert a deleterious effect.

The third part of the book contains a short account of the structure and functions of farm animals, their various breeds, and also chapters upon feeding. The subject of dairying is dismissed in a chapter of 20 pages, and is the least satisfactory portion of the book. Under "Cheddar Cheese making" is given a process which is rather indefinite, and seems a sort of mixture of several methods; the instructions for this process are vague, the temperatures (where given) are rather uncertain, and nothing whatever is said as to the proper condition of the curd at the end of each operation. On p. 429 this remark is made:—

"Real Stilton, for example, is a double cream cheese, the cream of the evening's milk being added to the morning's milk."

"Real Stilton" of this nature is a myth; the so-called "cream Stiltons" are made from two curds, and do not contain added cream, the term being used (rather ambiguously we confess) to signify that no cream was abstracted from the milk used in making the cheeses.

A few errors in the text may be mentioned here. On p. 65 and elsewhere, ammonium carbonate is spoken of as a volatile gas; it would be more accurately described as a volatile solid. The average percentages of albuminoids in certain foods given in Table xviii. (p. 343) are most of them too low. In Table xx. (p. 344) the percentage of albuminoids in cotton cake is put down at 23·17, whilst it is given as 19 in Table xviii. on the previous page; the former value is more nearly an average one. The percentages of soluble carbohydrates in some feeding stuffs given in Table xxi. are not quite satisfactory; those for wheat, barley, and bean straws are much too high, especially that of 35 for bean straw; about one-fifth that figure would be nearer the truth. We think it would have saved space, and much trouble and confusion to the student, if Tables xviii., xix., xxi., xxiii, xxiv., and xxvi. had been combined, and the full analyses of the feeding stuffs given in a single table.

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Table xxxi. (p. 408) is headed, "Percentage of nitrogen and minerals in the fasted live weights of cattle, sheep, and pigs." This heading should be, "Amounts of nitrogen and minerals in 1000 pounds fasted live weights, &c."

The illustrations in chapters xxv. and xxvi. are very poor indeed, especially Fig. 190, which is perhaps intended as a puzzle print. Also Figs. 171–3 are very bad attempts to convey an idea of what a cow is like. Figs. 160–4 are equally poor.

Apart from these details, we cannot but say that the book as a whole is an admirable work, and superior to anything of its kind which we have yet seen. It will prove a boon, alike to students and to educated farmers.

W. T.

HYLO-IDEALISM.

Further Reliques of Constance Naden. Edited by George M. McCrie. (London: Bickers and Son, 1891.)

WE have already in these pages expressed our opinion, in a notice of her "Induction and Deduction," that, had the hand of Death been withheld, Miss Naden would have made valuable contributions to philosophic thought. The volume of "Further Reliques" now before us serves to justify this opinion. It is questionable, however, whether her friends have been well-advised in including the "Geology of the Birmingham District," admirable as it is as a student's prize essay. In any case, since it was included, it would have been only just to Miss Naden to have requested someone acquainted with geological nomenclature to revise the proofs. On a single page we have *Trianic* for *Triassic*, *Keupes* for *Keuper*, *Llandeils* for *Llandeilo*, and *Paradoyidian* for *Paradoxidian*. This essay is dotted over with such misprints (the genus *Orthis* being printed, on p. 25, both *Orttiis* and *Orttus*). Nor are other parts of the book entirely free from errors due to careless editing. On p. 4, *destruction* is printed where the sense requires *distinction;* and on p. 160, *evidence*, where the author clearly meant to write *eloquence*.

As we before pointed out, for Miss Naden the fundamental principle in philosophy is the famous Protagorean formula of relativity, that "Man is the measure of all things, of things that are that they are, and of things that are not that they are not." In a kind of parable she describes the creation of the external world:—

"A myriad etherial waves, of inconceivable minuteness, enter the tiny window of the eye, and beat against the delicate lining of its darkened chamber. The pulsations are taken up, and transmitted along the optic nerve to the base of the brain, and thence to the gray thought-cells of the cerebral hemispheres. And in these gray thought-cells lives the God who says, 'Let there be light,' and there is light. If the optic nerve be an inefficient messenger; if, maimed or paralyzed, it fail to convey the vibrations received from without, the creative fiat will never be issued, and the world will remain, for the God of that one cerebrum, without form, and void. He is not a First Cause, since a stimulus is needed to set him in action; but he is certainly the only authentic Creator of the world as yet discovered by science, philosophy, or religion."

Whether this way of putting the matter is in the best possible taste, we do not pretend to decide. It is with Miss Naden's philosophical position, not with any other aspect of her views, that we can deal here. Philosophically, her view is, that the gray thought-cells are for her, and for each and all of us, the creators of the world. "But here," she continues—in her article on "Hylo-idealism," prefixed to certain letters addressed to her by Dr. Lewins, of the Army Medical Department—

"But here comes the most critical point of the inquiry. If the universe be simply a more or less coherent vision; if its very solidity and extension be but parts of the 'realistic' drama, how are we to know that there is any such thing as matter? . . . How are we to be sure that the brain itself really exists, and that the all-generating cells are not mere illusory appearances?"

How does Miss Naden answer this question, by which she is by no means the first to be puzzled?

"The puzzle," she says, "is not so hard as it looks. The uttermost sceptic tacitly assumes the possibility of argument; that is, of a course of reasoning, in which every step is dependent on the preceding step, while the origin of the whole is some group of observed facts. If this be a delusion, and the last step stand in no kind of causal connection with the first, evidently argument is impossible, and the sceptic's lucubration shares the general invalidity. A succession of mere mental phenomena, of mere inert pictures, cannot constitute reasoning, because one inert picture cannot produce or condition another. If a mental state possess no property except the property of being perceptible, it is obviously purely passive, and exerts no real influence upon subsequent mental states. Now, as this position is utterly unthinkable, and is not less destructive to scepticism than to materialism, we are obliged to assume the existence of some active basis of thought—that is, of something which thinks. What we assume of the individual self we extend analogically to other men, who are to us other selves. And having seen that sensation and motion follow upon excitation of the brain, and are suspended or destroyed by paralysis of the brain, we are justified in restoring our thought-cells to their proud creative eminence, and in proclaiming that they constitute this 'active basis of thought'; that they think, and therefore exist."

In an earlier paper on "Scientific Idealism," included in the "Further Reliques," Miss Naden says:—

"For the present I must be content to plagiarize from Descartes, and to say of the cerebrum, '*Cogitat ergo est.*' It can appear to us only phenomenally, and we cannot speak of it otherwise than in terms of phenomena; but here, at least, we are forced to assume an underlying *noumenon*, while renouncing the vain hope of penetrating to its essential nature by reason or intuition."

So, after all, the thought-cells which have been restored to their proud creative eminence and proclaimed as constituting the "active basis of thought," turn out to be phenomena like the rest of the "realistic drama," and "even the vibrations supposed to impinge on the surface of the body, and the molecular tumult propagated along the nerves, are merely convenient intellectual representations of the unknown"—to which Dr. Lewins adds in a footnote, "and nothing until asselfed," without, however, explaining how "nothing" in the process of asselfment becomes something.

Such, so far as we understand it, is hylo-idealism. In it one recognizes an old friend under a new name. It would seem that Miss Naden admitted with reluctance the phenomenal nature of the all-creative thought-cells.

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As it is, her views are hardly consistent. She generally appears to regard the organism or the thought-cells as the only reality—a reality set over against, and in marked contradistinction to, the "realistic drama" of the surrounding universe. But, in the passage above indicated, she admits the phenomenal nature of the thought-cells, and confesses an unknown *noumenon*. We do not think that Miss Naden's philosophy had reached its final form when Death so untimely snatched her hence.

C. LL. M.

PHYSIOGRAPHY.

The Realm of Nature. An Outline of Physiography. By Hugh Robert Mill, D.Sc. Edin., F.R.S.E. (London: John Murray, 1892.)

THE scope of physiography has perhaps been more misunderstood than that of any other subject. It had its birth in the Government system of science examinations, and the new name was applied to distinguish the subject from the older and narrower one of physical geography, which was so widely taught in elementary schools.

In the great majority of the text-books which have hitherto appeared, the authors have rigidly followed the lines laid down in the syllabus of the Science and Art Department, and have generally contented themselves with stating facts, more or less accurate, without properly considering their inter-relation. It is a great relief to turn from these to the book before us. It is written independently of all examinations, and in it the true place of physiography among the sciences is clearly defined.

The book is one of the University Extension manuals, of which the editor (Prof. Knight) says: "Their aim is rather to educate than to inform." No better subject could therefore have been selected as one of such a series, and Dr. Mill fully bears this aim in mind. Indeed, in this respect the book is worthy to be compared with the "Introductory" science primer written by Prof. Huxley some years ago. The opening chapter deals with such matters as "Nature," "Science," "Use of the Senses," "Reason," "Common Sense," "Cause and Effect, and 'Natural Laws,'" all of which are set forth very clearly, and made deeply interesting. For the benefit of those who may have doubts as to the meaning of physiography, we may quote the following from this chapter:—

"Physiography means literally the description of Nature (p. 1). . . . It describes the substance, form, arrangement, and changes of all the real things of Nature in their relations to each other, giving prominence to comprehensive principles rather than to isolated facts (p. 3). . . . In order to have a just conception of the universe, the results of specialized research must be fitted harmoniously together. This is the function of physiography, which has, consequently, a unique value in mental training, being at once an introduction to all the sciences and a summing up of their results. It enables a beginner to obtain a quicker insight into any of the special sciences and a fuller grasp of it, while, at the same time, a student versed in any one special science is enabled to appreciate far more fully than an unversed one its relation to all others and to the system of the universe" (p. 13).

Succeeding chapters deal with subjects somewhat after the order of the syllabus of the Science and Art Department. It is only necessary to say of these that they are excellent, and that they have been revised by well-known specialists, including such names as Prof. Tait, Dr. Copeland, and Dr. John Murray. In chapter xvi. there is an admirable account of "Life and Living Creatures," in which is given a good outline of the classification, distribution, and functions of animals and plants. The final chapter is reserved for "Man in Nature," and deals with the distribution of the various types of humanity, the effects of environment, migrations, and man's power of altering the course of Nature's works. A useful list of memoirs and books is appended to each chapter.

The book is fully illustrated, chiefly by new diagrams, and there are nineteen beautiful maps, which have been specially prepared by Mr. Bartholomew, whose competence for such work is well known. These maps form an important feature of the book, and illustrate, amongst other things, earthquake regions, isotherms, rainfall, and the evolution of continents.

The whole book shows signs of the greatest possible care in preparation, and it is not an easy matter to suggest improvements. It is a very valuable contribution to the literature of the subject, and we trust that it will meet with the appreciation it deserves. It is admirably adapted for all thoughtful persons desiring an insight into scientific methods; and although not intended as a book for use in schools, all teachers and students of the subject will do well to make themselves acquainted with its contents.

A. F.

OUR BOOK SHELF.

Grasses of the South-West. Plates and Descriptions of the Grasses of the Desert Region of Western Texas, New Mexico, Arizona, and Southern California. Part II. By Dr. George Vasey, Botanist, Department of Agriculture. 50 Plates, with descriptive Letterpress. (Washington: Government Printing Office, 1891.)

THE first part of this work was issued in October 1890, and duly noticed in our columns. It also contained 50 plates. It is a pity that in this second part the numbering from 1 to 50 is begun over again. It would have been much more easy to cite the plates continuously from 1 to 100. The present part goes all through the series of tribes again, and includes 4 species of Paspalum, 3 of Stipa, 4 of Muhlenbergia, 5 of Sporobolus, 9 of Triodia, 5 of Diplachne, and 4 of Eragostis. Most of the species are endemic, and very few of them have been figured before. None of them are British species, but one of the Stipas is a variety of the common European feather-grass (*Stipa pennata*). A good general handbook of grasses is one of the books most urgently wanted both by systematic botanists and agriculturists; and the United States Department of Agriculture is doing a very useful work in bringing out these bulletins. The next two bulletins are to be devoted to the grasses of the Pacific slope, and the four numbers will bind up into one volume, containing 200 figures.

Sporting Sketches in South America. By Admiral Kennedy. (London: R. H. Porter, 1892.)

THE sketches presented in this volume appeared originally in *Land and Water*, and are now reprinted with only such slight changes as "time and circumstances have

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rendered necessary." They are very pleasant reading, and should be of considerable service to naval officers, yachtsmen, and sportsmen who may visit South America. The commission of the *Ruby* in South American waters extended over three and a half years, and during that time the officers seem to have missed no opportunity of indulging their taste for sport. The game killed amounted to 13,349 head. What was killed was never wasted, for there were on board 250 persons to be fed. Although Admiral Kennedy has much to say that will be especially interesting to sportsmen, they are by no means the only class of readers to whom he appeals. He visited many districts which are well worthy of being described, and the impressions they produced upon him are invariably recorded in a fresh, simple, and straightforward style. The text is illustrated with copies of some pen-and-ink sketches by the author.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Cirques.

PERMIT me, while thanking Mr. I. Russell for his friendly criticism of my views as to the origin of *cirques* (p. 317), and for a copy of his memoir on the Mono Valley, in which his own are developed, to indicate the reasons why I remain unconvinced.

(1) The first part of his criticism appears to me to be wide of the mark. I never said that the three conditions, quoted by him, were *necessary* for the formation of *cirques*, only that they were "the most favourable." I have described two *cirques* in granite (*Geol. Mag.*, 1871, p. 535), and seen them in other crystalline rocks.

(2) I never said that other topographical features might not be associated with those dispositions of rocks, which are often found in *cirques*. I stated that for the production of *cirques*, not only certain materials, but also a particular machinery, were required. Lines of cliffs, ravines, alcoves, corries, are numerous in the Alps: *cirques* are comparatively rare.

(3) The instances described in my original paper (Quart. Journ. Geol. Soc., 1871, p. 312) were called *cirques* at that date, and, I believe, still retain the name. They are in all essential respects identical with Gavarnie and similar places in the Pyrenees, which are always called *cirques*. Hence, it appears to me, Mr. Russell cannot elude my arguments by proposing a new terminology. Moreover, to call these places alcoves, is to misuse the word. An alcove holds a bed, or screens two lovers in earnest conference from over-curious eyes—it is a small affair. The diameters of the Alpine *cirques* are measured by hundreds of yards; their walls by hundreds of feet. Mr. Russell's studies in the Appalachians (unless I misunderstand him through ignorance of the locality) appear to me to indicate no more than that (as I have repeatedly affirmed) no real demarcation can be drawn between *cirques*, corries, and bowl-like heads of valleys. Of each I have seen examples, little as well as big. The formation of an alcove, as described by him, is only a special case of an action, identical in kind (but different in degree and environment) with that by which the above-named are produced. As it seems to me, he proves, not that *cirques* and alcoves are genetically distinct, but that they are genetically identical. One stream will make an alcove, many a *cirque*.

(4) I have never denied that under certain conditions a basin (generally quite shallow) may be scooped out by ice-action on the floor of a corrie, but the peculiar "tooth-drawing" action of the base of the *névé*, postulated by Mr. Russell, appears to me only an hypothesis. I stated, when it was propounded by Mr. Helland, that I knew of no evidence in its favour, and much in opposition (*Geol. Mag.*, 1877, p. 273); I repeat the same now. Wherever I have seen the bottom of a corrie uncovered, the rock has been smooth and ice-worn, not rough with the sockets of extracted "plugs" of rock.

(5) As a *bergschrund* usually narrows in descending, the tension of the ice should be at a minimum at the base of the *névé*, and I do not believe that atmospheric cold is appreciably more potent there than at the bed of a glacier. Moreover, as the ice must move more slowly in the *névé* basin than in the glacier, the former standing to the latter in the relation of a pond to its effluent, the friction there should be least.

(6) Some of the Alpine *cirques* lie rather low, and are *not* in a position where glaciers would first appear, as is shown by the fact that they are now full 3000 feet below the snow-line. Perhaps, however, Mr. Russell supposes rock wrenched away to a depth of quite a thousand yards. If so, I have nothing to add to what I wrote in 1871 and 1877. All that I know of the Alps and of other mountains is opposed to any such notion.

(7) Above three at least of the *cirques*, described by me, the cliffs rose in inaccessible steeps to the very summit of the range. From the highest peak of the Diablerets one could almost throw a stone on to the floor of the Creux de Champ, a full mile below; the two *cirques* near the Surenen Pass are immediately under the crest of the range (as shown in my diagram). Yet *there are the streams* fed by snow-ledges, which, though comparatively narrow, keep them still at work. Doubtless, in some cases the recession of the walls may bring to an end the work of excavation. But even in Egypt, in a *cirque*-like glen beneath a narrow crest, Mr. Jukes-Browne found evidence of streamlets (*Geol. Mag.*, 1877, p. 477); and I may cite Herr J. Walther (*Abhand k. Sächs. Gesellsch.*, xvi. 345) for the occurrence of "amphitheater oder circusthäler" in that country, though he attributes more to the action of wind than I should venture to do.

I might continue, but it may suffice to say that the result of twenty years' experience has been increased confidence in the general accuracy of the views expressed by me in 1871. These, it appears to me, Mr. Russell combats only by attempting a distinction, which I believe to be non-existent, and devising a method of glacier-erosion, which I believe to be not only mechanically impossible, but also contrary to the facts of Nature.

T. G. BONNEY.

Bedford College and the Gresham University.

YOU have given publicity to various communications on the subject of the Albert or Gresham University; may we therefore request that you will extend a like favour to us, and insert a brief statement from one of the Colleges for Women, deeply interested in everything affecting higher education in and for London?

The following are some of the grounds on which the Bedford College, London, opposes the Draft Charter:—

(1) That the Bedford College, London, for Women, provides a complete academical course in the two faculties of arts and science, and therefore feels that from the very outset it is entitled to be included in any proposed University for London, on an equal footing with University and King's Colleges, with due representation on the Council. For such immediate affiliation the Draft Charter does not make any provision.

(2) That should the Draft Charter become law, a University would be created which, while exercising only some of the functions of a Teaching University, would do this in such a manner as to effectually prevent the foundation of any Teaching University which should be capable of expanding to the ever-growing demands of the Metropolis.

(3) That while the Act of 1871 abolished all ecclesiastical tests in the Universities of Oxford, Cambridge, Durham, and all Colleges therein to such an extent that all denominational Colleges are separate from the Universities, this Draft Charter of the Albert University allows one of its component Colleges to impose ecclesiastical restrictions. The Council, therefore, in its petition to both Houses of Parliament claims that the Bedford College be made an original constituent College in any new University for London, with due representation in the governing body, and prays that the present Charter be not granted unless so amended as to be far more comprehensive, more adequate to the present and future needs of the Metropolis, and free from all ecclesiastical restrictions.

W. J. RUSSELL, Chairman of Council.
LUCY J. RUSSELL, Honorary Secretary.

Bedford College, London, February 19.

The Implications of Science.

CAN you allow me space to reply to Miss Jones's courteous letter (p. 366), which I shall have the more pleasure in doing as I hope to be able to clear up the points in dispute between us?

I think it is clear—Miss Jones at least admits so much—that

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some part of the meaning of a term is determined arbitrarily; either by public opinion for general use, or by the individual reasoner, if he wishes to use the term for some special purpose of his own. For this does not imply that there ever was a formal contract as to the meanings of terms. The fact that terms do not mean the same to people of different nations is enough to show that the determination of the meaning by each nation is, in part at least, essentially arbitrary. It is also, I think, clear that there is a part of the meaning of some terms at least which is not arbitrary, when once the arbitrary part has been settled. There is a slight ambiguity about my use of the word "definition," which, however, when once noted, need cause no confusion, as the context will always show in which sense I am using it. Commonly a "definition" denotes an assertion which determines the arbitrary part of the meaning of a term; but when I speak of "the definition" as opposed to "the import" of a term, I mean not this assertion itself, but that part of the meaning of the term which it determines. This explains how it is that I do not regard "the definition" of a term (as opposed to its import) as a *thing*. Even if it is clearly conceived in the mind it is only an abstract idea; but the point is that it is not necessary that it should be conceived at all. Thus it is not necessary that a definition by connotation should be "of something," if that means that the term should have denotation. Even if the term has any denotation, *i.e.* if the mind grasps it as denoting a thing or idea, this denotation must, in a symbolic argument, be regarded as merely "accidental clothing."

Just as some part of the meaning of a term is arbitrary and some part not, so in any system of logic some part of the system is arbitrary and some part not. Now, either the denotation or the connotation of a term may be laid down arbitrarily, but *the connection between the two is not arbitrary*. One cannot arbitrarily lay it down that "such a thing possesses such attributes." If, therefore, a definition is to be an arbitrary assertion, it must only lay down either the denotation or the connotation of a term, and not parts of each. It would, of course, be logically permissible to use the word "definition" in a different sense, but then definitions would not be arbitrary assertions. This is exactly what is done if the assertion "Two straight lines cannot inclose a space" is called a definition by denotation; and therefore, in my system of logic, I cannot admit it to *be* a definition. This is one of the arbitrary features of my system. But it is not arbitrary when I say that in such assertions as "Nothing can both be and not be," and "Twice two is four," there is nothing absolute at all. The truth of these assertions is determined entirely by the arbitrary parts of the meanings of their terms. Miss Jones says: "If definitions were purely arbitrary, as Mr. Dixon holds, what would prevent my saying that *Four* $(1+1+1)$ *means twice two* $(1+1)+(1+1)$?" Precisely; this is exactly the point. *There would be nothing at all to prevent it.* Boole actually did this very thing, as his law of indices in his mathematical analysis of logic. According to his law $a^{1+1+1} = a^{(1+1)+(1+1)}$.

There remain one or two minor points to be answered. As to induction, I think Miss Jones's view is substantially the same as mine. Of course it is possible to set out an inductive generalization in a syllogistic form as Miss Jones does; only the whole of the induction is then contained in the assumption of the major premiss—the syllogism itself is in no sense inductive.

If Miss Jones thinks the truth of the formula $(a+b)^2 = a^2+2ab+b^2$ is deduced by generalization from a single concrete instance, how does she explain the fact that sometimes $(a+b)^2 = 0$, as in Grassmann's "Aeussere Multiplication," and sometimes $(a+b)^2 = a+b$, as in Boole's logic?

I see nothing to differ from in the paragraph about S and P; except perhaps that if I define such a term as "metal" by denotation I do *not* say "This and all other things like it in certain respects" are metal. That would be mixing up denotation and connotation in one definition. I say only "This, and this, and this, . . . are metal."

The explanation of the point about the mathematical truths is simply that I do not consider the assertion "Two straight lines cannot inclose a space" as a mathematical truth at all, if "straight line" is defined by denotation. I certainly believe it to be true, but its truth is not of the same nature as that of the assertion "Twice two is four," or even "*Cogito, ergo sum*"; it is neither a truism nor a necessary truth, in my senses of those terms, but can only be established by induction.

EDWARD T. DIXON.

Trinity College, Cambridge, February 19.

The Value of Useless Studies.

IT is rather surprising that Prof. Ayrton should indulge in covert sneers at Universities for devoting themselves to useless studies. It certainly ill becomes one whose life is bound up with electrical science, which is of such recent growth that nobody can pretend to forget how it owes its origin to those who studied it while useless. If Universities do not study useless subjects, who will? Once a subject becomes useful, it may very well be left to schools and technical colleges, to patent-mongers, and the trade. Mr. Bury is, on the other hand, mistaken in two respects. That a subject is useless is hardly worth considering as a recommendation for its being made *compulsory* on students. There are too many useless subjects for that. The great objection to compulsory Greek is that it is the principal stumbling-stone in the way of any literature being studied by ordinary University students. The Bible produced very little effect until it was read in translations; and the danger of a pagan revival, if ancient literature were studied without the obstruction of difficult languages, is the best reason for insisting on these languages in a Christian University. The second mistake of Mr. Bury is that it is any part of the business of a Universtiy to *teach*. Universities should certainly give facilities for students to *learn*. It is the business of the students to learn. If they are so ill prepared that they have not acquired the art of learning, they should go to a college or school or private teachers, and get taught; for teaching is the business of these institutions and persons. The business of Universities is to advance culture and knowledge, and to afford students an opportunity of learning how to do this. Prof. Ayrton, by omission rather than by commission, seems utterly unable to appreciate the value of literature for its own sake. How can all this fierce toil he extols so justly advance a lot of savages?

GEO. FRAS. FITZGERALD.

Trinity College, Dublin, February 13.

The Nickel Heat Engine.

IN trying Mr. Smith's experiment on the rotation of nickel (NATURE, January 28, p. 294), I find my disk has a more complex action.

It will be remembered that the nickel is mounted on a vertical axis, and has the poles of an electro-magnet put near two points, A and B, 90° apart. Let C be any point in the larger arc, and suppose heat is applied to A.

I find that at first the disk moves in the direction ACB with a hesitating, uneven motion; it revolves several times, and almost develops into a steady rotation, when it beats back, oscillates for a while, and goes off with a brisk even spin in the direction BCA. After about two minutes this fails, and a new start is attempted the opposite way, but there is little more movement, until time has been given for cooling.

It makes no difference at which pole the heat is applied. The disk is 50 mm. diameter, rather less than 1 mm. thick, and is covered with lamp-black.

W. B. CROFT.

Winchester College, February 12.

THE UNIVERSITY OF LONDON.

PROBABLY before this letter is in type the Charter of the Albert, or, as it is now to be called, the Gresham, University, will have been presented to Parliament. What will be its ultimate fate remains to be seen. But I have sat through a long night's debate in the House of Commons to see a Government turned out in the small hours of the morning on a University question. I do not suppose that the proposed Charter will raise an issue of that importance. But I think that the Government may not be unmindful of past history, and, in what is probably the last session of a moribund Parliament, may not be willing to push very vigorously what is perhaps the crudest scheme of University organization which has ever been proposed in this country.

Before Parliament takes so serious a step as founding a new educational institution, the question may fairly be asked, On what ground is it necessary? Everyone knows that we have a University in London which is a State institution; and one may not unreasonably inquire,

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In what respects is it inadequate to its duties; and, if so, why cannot its defects be remedied without beginning the work all over again?

To neither inquiry has, it seems to me, any very satisfactory reply at present been given. The reason perhaps is that beside the ostensible reasons for the Charter which are put forward, there are others in the background which are less conveniently avowable, and which therefore it is anything but easy to meet.

Of the former, the most important is that the existing University does not teach. This is an undoubted fact. If, however, it were provided with a body of Professors, as has often been suggested, it might teach just as much as Oxford and Cambridge do. That defect is clearly, then, not irremediable. But, on a little further investigation, it turns out that something more than teaching is meant. What is desired is that the teachers shall also have the control of the examinations. What is wanted is to introduce into London the teacher-examination system, which is said to be the distinctive feature of Universities in Germany. To quote an able article in the *Standard* of January 19:—

"There must be a Teaching University in order that the teachers might control the studies, and the degrees be a recognition, not of mere knowledge (which might or might not be superficial), but of sound intellectual training."

Now, I have discussed this position at some length in the pages of NATURE, and I need not repeat what I have already said. But it may be pointed out that, though the teacher-examiner system once obtained to some extent in the British Isles, it has almost entirely been abandoned. As Lord Sherbrooke used to remark, "Teachers no longer sample their own goods." Their doing so led, I think it cannot be doubted, to serious abuses, the possible nature of which is sufficiently obvious. Even in the English Universities, some of the Colleges practically had the entire control of the education of their undergraduates; but they deliberately abandoned this exclusive privilege, and handed over the business of testing the performance of their pupils to their respective Universities; in other words, they intrusted the business of sampling their goods to those who had nothing to do with their manufacture. The Scotch Universities most closely approximated to the German system; but here, again, external examiners have been introduced, who undoubtedly were intended to act as a check upon the Professors and their teaching.

It can thus, I think, be shown to demonstration that the system which obtains in Burlington Gardens is not really so anomalous as it is made to appear. The teacher-examiner cry is, then, of the nature of a reaction; and though to a certain extent I am not without sympathy with it, I am not sure that, on the whole, it is not a mischievous reaction.

Now, when a great institution like the University of London is attacked, one attaches naturally some importance to the quarter from which the attack comes. I quite admit that the University, perhaps from an excessive sense of its own dignity, has rather allowed the case against it to go by default, and has done little in its own defence. But the world outside may judge for itself. I am prepared to contend that there is no examining body in the world which does its work in a more efficient and conscientious way. Its examination rooms are crowded; it commands the services as examiners of the most distinguished teachers of the country; and whether in science, law, or medicine, amongst those who have successfully submitted to its ordeals are to be found the names of some of the most distinguished men of the time. It seems clear, then, that the attack on the University would not come from either the students or the public. It can hardly be doubted that the latter is more than satisfied. When parents allow their sons to embark in University studies, they like to get something tangible as a result; and in a degree at Burlington Gardens they undoubtedly do get something the value of which is generally accepted.

The complaint comes, however, from the teachers; and this not in the provinces, but in London alone. Their case was put before the Commission by Prof. Lankester in a very striking and able way. I do not doubt that his evidence has produced a very powerful effect. But having studied it, I am bound to say that, having been a graduate, an examiner, and a member of the Senate of the University, I cannot accept all he says as giving a fair account of the inner working of the system. The burden of his complaint is that the teacher is unreasonably fettered in giving the best and most modern kind of teaching by the restrictions imposed upon it by the University. In so far as there is ground for the complaint, I think it admits of remedy. But I cannot refrain from remarking that the provincial teachers who send candidates to the London examinations—and who, I suppose, are not, on the average, less competent and enthusiastic than their London colleagues—do not, as far as I can ascertain, in any way sympathize with their complaints. On the other hand, Prof. Lankester was for ten years an examiner in the University, and when he last left office I am much mistaken if he had not succeeded in moulding the curriculum in his subject practically into entire accordance with his own views. And even under the system as it exists I observe in his evidence that he admitted to Sir William Thomson, that "a teacher may, with judiciousness, of course, and common-sense in his teaching, teach the best that he knows."

I cannot keep feeling, then, that in the outcry raised by some of the London teachers there is something a little unreal. The existing system neither works so badly, nor is it in itself so bad, as it is represented to be.

The Privy Council, however, appears to have thought differently, and has set itself to the task of creating a Teaching University for London. And this is what Parliament is asked to sanction. The fatal objection to the scheme is that the title is a misnomer; the proposed University is, as I have already pointed out in NATURE, in effect, no more a Teaching University than the existing institution.

It is perfectly clear that by a Teaching University is meant, by those who understand what they are talking about, a University of the German type, governed and administered throughout by the professorial body. If I understand his views, this is the solution of the difficulty which Prof. Karl Pearson desires. I am far from saying that I am opposed to the establishment of such an institution. It would certainly be an interesting experiment, and it might be a great success. It might be created out of University College standing alone; and for my own part I have always wondered that that distinguished place of study has never sought such a transformation. It would, of course, require a profound reconstruction of its constitution, and the total elimination of what may be called the lay element in its government. Or it might be effected, though with more difficulty, if University and King's Colleges could be induced to fuse. Prof. Lankester contemplated such a possibility; but it is probably as hopeless as to try to get anything but an emulsion out of oil and water.

The organization of such a Teaching University is not without its drawbacks, and I am not at all sure that London would be the best seat for one. The system is only a success in Germany because Universities are very numerous, and the number of students in each for the most part not very large. The essence of the system, as I understand it, is the close personal contact of teacher and student. Each gets saturated, so to speak, with the other; at the end of the curriculum the Professor knows, or thinks he does, all that the pupil has acquired, and

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gives him his degree accordingly. I paint the picture in its most attractive colours. And for pupils of a certain type I believe the system to be excellent. But for the large majority I am not so sure; and I think that under such a system a little independent examination would often act as a considerable surprise both to teacher and taught.

It seems to me that the fact is often lost sight of that education in all its forms is rather a tedious business. What the teacher has to deal with are not merely the enthusiastic few who march with joy alongside their instructor, and scarcely see the obstacles that bestrew the path, but the large mass, headed at the best with stolid industry in front, to tail off with incorrigible straggling behind. Here the discipline of the examination-room, backed by parental wrath at failure, seems to me well-nigh indispensable. Even in such cases as these, no doubt personal contact with the teacher can do much; but then the classes must be small. The limit is soon reached when the individual powers of the teacher, however energetic, are reduced to a vanishing quantity. I have found myself that in a class of forty I reached the extreme limit on which I could hope to produce any effective direct impression. Frankly, I do not believe that for large classes, such as might be expected in London, the teacher-examiner system has any merit at all. I think myself that, if the teacher directs and organizes the teaching to the best of his powers, he may, with distinct advantage, leave the testing of the results to other hands. I know that this is what I should prefer myself; and I speak with the experience of a man who has spent, and not wholly without success, some of the happiest years of his life in the work of teaching.

But I certainly agree that the teacher should have something to say in the business of examining. He does, in London, already have a good deal to say as a matter of fact, but very indirectly. I would remedy this by giving all teachers of University rank a definite status in the University.

The bare statement of the fact that the University of London is merely an examining body does very inadequate justice to the work it has accomplished. It has undoubtedly stimulated and disciplined the studies of vast numbers of persons whom the older Universities would never have touched. And it has reacted on the teaching given in London and elsewhere by insisting on a progressively high standard. The educational influence has been consequently far-reaching, and in my judgment not small. This is effected by the schedules or syllabuses which are prescribed by the Senate and from time to time are varied as the progress of instruction in each subject makes change desirable. It is notorious that by this means the University of London has compelled the teaching bodies which send it candidates to develop the efficiency of their methods of instruction.

I am quite prepared to admit that in theory the machinery by which this result is brought about is not perfect. But in practice, like so much else in this country, it works tolerably well. In some of the evidence given before the Commission it is almost implied that the Senate draws up the schedules itself. But, having been both an examiner and a member of the Senate, I know something of the process. Generally speaking, it is something like this. An examiner finds that an existing schedule is scarcely in touch with the best current teaching. He communicates his opinion to the Senate, and suggests alterations. The Senate generally calls to its assistance one or more of the past examiners in the subject, and also any persons of acknowledged competence whose judgment will probably lead them to a right decision. The result is that, if the alterations proposed by the examiner are found to meet with general approval, they are adopted. In time the process is repeated; and probably no year ever passes without something of the kind being done. Practically, the work is well done, and the general esteem in which the degrees given at Burlington Gardens are held appears to me a tolerable proof of the fact.

It is evident that what I have described is accomplished by what may be called semi-official means. I should prefer myself to have it done by a more definite organization. If Faculties were constituted in the University, the most experienced teachers might meet to settle the schedules in the best possible way. Even then some would probably not be wholly satisfied; but they would have at least the opportunity of explaining their views to their fellow-teachers, and, if reasonable, it is improbable that they would not meet with some recognition.

Another very important result would flow from the organization of the Faculties. One of the absurdest things about the proposed Charter is that, apart from medicine, except University and King's Colleges, it ignores the existence of any other educational institutions which either at present exist, or are certain hereafter to be created, for the educational needs of so vast a city as London. A great evil at the present time is the isolation of such institutions. They do not recognize each other's existence or work in any way in conjunction. If their teachers, under the *ægis* of the central University, could be brought together to confer on educational matters, it can hardly be doubted that instruction in London would be better organized, gaps filled up, and wasteful overlapping obviated. All this might be done without any interference with the autonomy of the separate institutions, and by men exchanging ideas in conference and discussion.

There is one point on which the new Charter has been attacked with which I do not in any way sympathize. The inclusion of King's College in the proposed University is objected to on the ground of its being a denominational institution. But in Australia and Canada the association of denominational Colleges with the Universities is found a simple and effective solution of difficulties which for a long time will probably be insuperable in any other way. One of the most interesting things in the history of English Universities in modern times has been the removal to them of Nonconformist Colleges. For my part, I can see no possible objection to their being admitted ultimately to full University privileges. If the new University is to be accepted, it is idle to object to the admission of King's College, or to demand that it should abandon its characteristic features as the price of admission.

To sum up the position. The present University is denounced because it fetters the teaching and is not in touch with it: this turns out to be rather a matter of form than of fact. To remedy this a new Examining University is to be created, on much the same lines as that already existing, which in its turn is to be allowed to go on exactly as before. Surely, in the whole history of the reform of institutions, nothing so futile was ever proposed.

The real academic need of London is left perfectly untouched. This is the organization of the higher University teaching. I am more and more convinced that a distinction ought to be drawn between what may be called antegraduate and post-graduate study. The former, leading up to the ordinary Bachelor's degree, may very properly be left to the Colleges. More than this, without a vast increase of endowment and staff, I do not see how they can accomplish. I regard a student who has taken his Bachelor's degree as having learnt the technical language of his study. He may then, in a considerable number of cases, devote himself to original inquiry. And this the University of London encourages him to do, as examination is no longer compulsory for the Doctorate. It may be, and in fact is, obtained by original work embodied in a thesis. I confess I should like to see the University of

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London provided with a body of superior Professors, who would not merely add to its distinction by their own labours and public instruction, but would guide the studies and researches of the young graduates. With the right men for Professors, and such picked students as the vast area of London would supply, I do not doubt that the reputation of the University of London would at no distant date rival that at of any in the world. I hope Prof. Karl Pearson will forgive me if I appropriate in this connection a portion of his letter in the *Academy* of December 19:—

"The professorships ought to be the best in England, and the chief posts, at any rate, might remain in the gift of the Crown; the laboratories and libraries ought to be the best equipped in the Kingdom, and the University ought to draw students and investigators, not only from the five millions of London, but from the Greater Britain over the seas. Such a University would not only be able to retain in London men like Burdon-Sanderson, Seeley, Gardiner, Sylvester, and Lankester, but it would bring others there."

With the main idea expressed in these words with so much enthusiasm I most heartily agree. But I think those understand their countrymen and the possibilities of things best who would use, as I would, the existing University as a foundation. It has deserved well of the State; it has done its appointed work in the past well: instead of abusing it, we should strive to remove its defects, and give it higher work to do.

Two possible objections to my proposition may be considered. It may be said that such professorships are not wanted in London because such professorial teaching is supplied by Oxford and Cambridge. My reply is, that only a small portion of the population pass through those Universities, and London would draw from a much larger field. Again, it may be objected that the Professors of the existing teaching institutions in London would object to the creation of posts of a superior grade. On the other hand, such posts might stimulate ambition; they would be the prizes of the academic career. To object to them seems to me as reasonable as for a stuff-gownsman to object to the existence of judges, or for a curate to that of bishops.

The depletion of London of its most distinguished teachers, which continually goes on, is a real loss to its intellectual life. Dr. Dollinger says:—

"The force which moves the world, that which brings on the important crises in the history of mankind, is not to be found in material interests and passions, but in the great ideas which it is the business of Universities to work out."

Why should London, of all places in the world, dismiss from its midst, as it has long continued to do, those whose gift it is to open up most successfully new territory in the unknown world of knowledge? Yet the depletion goes on. Not one of the five officers of the Royal Society is at the moment resident in London. The President attends its meetings from Glasgow, and the Senior Secretary from Cambridge.

I must say a word about Gresham College. The promoters of the Albert University, whose untiring energy would be invaluable in a better cause, have, no doubt, done a clever thing in securing the adhesion of this obsolete institution to their scheme. Years ago I attended one of the prelections with two friends. The Dean of Manchester favoured, very unwillingly, an audience composed of ourselves and a few casual passers-by, whipped in apparently by the beadle, with a demonstration of Euclid I. 47, in the Latin tongue. Yet the institution which had descended to this mere husk of formality had once, if tradition is to be believed, been one of the most famous seats of learning in the world, and Francis I. is said, in emulation of it, to have founded the Collège de France in Paris. I believe the present Gresham Professors are not quite so sleepy as they were; but the contrast between the two rival institutions is more than melancholy. That Gresham College can nowadays clothe its dry bones and live is more than doubtful. It is, no doubt, only too glad to undertake the "teaching" department of the new University, which proposes to carry on its examining work in the empty building in Basinghall Street.

Kew, February 20. W. T. THISELTON-DYER.

A PRELIMINARY STATEMENT OF AN INVESTIGATION OF THE DATES OF SOME OF THE GREEK TEMPLES AS DERIVED FROM THEIR ORIENTATION.[1]

MR. LOCKYER has made out I think quite satisfactorily that the Egyptian temples were so oriented that the rising or setting of some conspicuous star on or near the axis of the temple, and visible from the adytum, would give warning of sunrise; and he applied to me for particulars of Greek temples for the purpose of seeing if there was any analogy, and the comparison appeared to promise a favourable result. Mr. Lockyer had found, before he had proceeded far in these studies, that he had been anticipated to a considerable extent by Herr Nissen, of Bonn, who has published several articles on the subject in the *Rheinisches Museum of Philologie*, and has brought within his scope both the Egyptian and the Greek temples. There is room, however, in the inquiry for a distinct work on the Greek temples, and especially with the help of more exact measurements of the orientation angles than Herr Nissen has made use of; as he appears to have contented himself with magnetic bearings—which are liable to considerable local variations, which are sufficient in an inquiry like the present to vitiate many of the conclusions that may be founded on such measurements—and there is a want of recognition of the influence of an elevated horizon. I had taken, in several instances, astronomic observations with a view to the more exact orientation of different temples, but something more is wanted even in the case of most of these—namely, the apparent altitudes of the mountains in the directions of the axes of the temples. I wish also to add that, but for Mr. Lockyer's suggestion, I should probably not have carried the inqury further than I already had done.

The great value of the inquiry lies in this: that it offers a means of determining, within tolerably close limits, the date of the foundation of a temple—not, perhaps, in most cases (although in some I believe it does) of the very structure which we now see, but of an earlier foundation on the same site. The key to the chronology lies in the movement of the stars with reference to the local horizon, owing to what is called the precession of the equinoxes. The object the ancients had in using the stars was to employ their rising and setting as a clock to give warning of the sunrise, so that on the special feast days the priests should have timely notice for preparing the sacrifice or ceremonial, whatever it may have been:

> "Spectans orientia solis
> Lumina *rite* cavis undam de flumine palmis
> Sustulit," &c.

The inquiry, even in its present state, is sufficient to establish a very high probability that the principle is a true one. There is nothing vague about it. It has to be kept within very severe limits, and it holds good nevertheless.

No stars can be accepted except from among the brightest, unless conspicuous star groups may have been used instead. Again, of single stars, only such can have been used for orientation in Greek temples which during

[1] Being the substance of a paper read to the Society of Antiquaries on February 18, 1892.

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a period not incompatible with reasonable archæology rose or set very near the line of the sun's course at some period of the year; and a further restriction is this, that the rising or setting must be just so far in advance of sunrise as to enable the star to be seen from the adytum of the temple, and, at the same time, not preceding it by any longer interval than is necessary.

If, in addition to this, we find, as is frequently the case in the Egyptian temples, and is not without parallel in Greece, that as the star to whose point of rising or setting the axis of a temple was first aimed worked away from its then position by *precession*, either the doorway of the temple was altered, or a new temple founded alongside, so as to retain the desired observation; and in every case of such new temple being so built it is found to have followed the same cult as the original; if, in addition to this, in different provinces temples are found of which the cult is known, and which are so planned as to be able to use the same star—with such decided differences of orientation, however, as were prescribed by latitude and the local circumstances of the surrounding heights—we obtain a further strong corroboration, and one that will in many cases be sufficient to determine the cult, where this has not been otherwise pointed out.

One further step requires to be taken to occupy the ground with perfect confidence—viz. to inquire what analogy is there between the days of the month when the sun would rise ushered in, as it may be said, by the temple's peculiar star, and the days of the festivals as derived from historical sources. In this comparison we must not expect a coincidence on every point.

The date of the temple foundation in many cases is pre-Homeric, whilst the basis of the historical account of the date of the feast is probably post-Persic. There may have been an interval of nearly 1000 years between the two, so that there is room for changes. Again, owing to their double system of reckoning months and years, considerable variation in the dates given by Mommsen, whose authority I mainly follow, is quite possible; and besides this, in some of the cases given below, the orientation day, if I may so call it, may be in fault one or possibly two days for want of the exact particulars of the site to which I have made allusion.

Firstly, speaking of Attic feasts, the great temple at Eleusis is an example very much to the point. The star which seems to have determined the orientation is Sirius, shining as it rose at midnight along the axis of the temple on September 14. The Eleusinian mysteries are stated to have commenced on the 16th of that month. In this case the sun was not looked for; the weird light of the star reflected from some combination of jewels was more likely to have been suited to the mysteries. It is perhaps less likely that this ceremony would have been changed than in most of the other cases.

The axis of the older Erechtheum had the central star of the fine constellation Aquarius setting heliacally on August 9.	The lesser Panathenaia, dated August 13–14.
Warning of the sunrise at Sunium was given by the setting of the Pleiades on October 20.	A feast to Minerva and Vulcan is dated October 30.
The star α Arietis rose heliacally to the older Olympieium at Athens, April 2, more than 1000 years B.C.	The feast Olympia is recorded for April 19 in later times.
The temple of Diana Brauronia on the Acropolis of Athens agreed with the rising of Aquarius (the central star ζ Aquarii in particular) on February 21 at the presumed date of its foundation.	The Little Mysteries were celebrated February 21.—N.B. A temple of Diana was in close connection with the great temple at Eleusis.

In the earliest times, as already explained, the stars were used as the only available clocks, but probably by the end of the sixth century, whether by the discovery of the behaviour of the stars or by the invention of the water-clock (which is recorded to have been used to some extent in the fifth century), or other approximate means of measuring time, the dependence on the stars alone had ceased; and the later temples, in Greece at any rate, appear to have been built without any reference to these.

At Athens this applies to the existing or new Erechtheum, the Theseum and the temple of Nike Apteros, which temples do not seem to have been built parallel to any old foundations. The old foundations under the Erechtheum have a very different angle. The sunrise, however, was considered in these just as much as before, for owing to the artistic instinct of the Greeks, they seem invariably to have secured for their principal festivals the fine effect of the first sunbeam on the statue; but as all the temples, whether old or new, admitted of two axial coincidences with the sunrise—one which might have a clock star (as it may be called) to announce the dawn; the other, except by rare accident, having none, the desired effect would have been attained on both occasions when the sun had the same declination.

It will be obvious that if the axis of a temple in any latitude had been directed due east (the horizon also being level), the rising sun would coincide with it at both the spring and autumn equinoxes. Similarly every other amplitude would have two solar coincidences (provided that is, in accordance with what has just been stated, the axis fell within the solstitial limits). When it had been found that the precessional movement had taken away the star from the direction of the axis, there would have been no reason for preferring one of these solar coincidences to the other, and the feast could have been shifted to a different date if it had been thought more convenient. It would appear that something of this sort may have taken place at Athens, for we find on the Acropolis the Archaic temple, which seems to have been intended originally for a vernal festival, offering its axis to the autumnal sunrise on the very day of the great Panathenaia in August.

The Chryselephantine statue of the Parthenon, which temple followed on the same lines as the earlier Hecatompedon (originally founded to follow the rising of the Pleiades after that constellation had deserted the Archaic temple alongside), was lighted up by the sunrise on the feast to the same goddess in August, the Synæcia, instead of some spring festival, for which both these temples seem at first to have been founded.

The temple at Sunium, already quoted for its October star-heralded festival to Minerva, was oriented also axially to the sun on February 21, the feast of the lesser mysteries.

Of temples of later foundation we have the following for which no suitable stars can be found:—

The Erechtheum, its sun axis days are March 2 and September 4, the latter being the date of the *Niceteria*, the special festival of that temple, supposed to record the celebrated contest between Minerva and Neptune, considered to have been on September 3. Another instance is the Theseum.

The *Thesea* are put down for October 8–9. The sunrise theory points out either March 7 or October 7. Does not this fact restore the disputed title of Theseus to this temple?

There cannot be so much known respecting the feasts in the other provinces.

The Olympic games were held, according to most authorities, soon after midsummer, but by others in the autumn. I quote on Nissen's authority the following:—

420 B.C., Sept. 14.	412 B.C., Sept. 12.
416 B.C., Aug. 31.	408 B.C., Sept. 2.

The heliacal rising of the star Spica (α Virginis) seems to belong to the Heræum, and indicates September 15.

The Isthmian games took place either in May or

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August, according to the Olympiad. The axis of the temple at Corinth coincides with the sunrise on both those months, but to only one of them (that of May) is a star applicable.

Further confirmation of the truth of this general theory—namely, that the amplitudes of stars determined the orientations of temples—lies in the fact that in the majority of instances, at any rate, the same star belongs to the same cult. I am satisfied that this can be established for the Egyptian temples. In Greece we find the following.

The star α Arietis is the brightest star of the *first sign of the Zodiac*, and would therefore be peculiarly appropriate to a temple of Jupiter. The heliacal rising of this star agrees both with the Olympieium at Athens and that at Olympia. There is a considerable difference in the deviation of the axes of these two temples from the true east; but this is exactly accounted for by greater apparent altitude of Hymettus over the distant Mount Pholoe.

The Pleiades are common to the following temples of Minerva, viz. the Archaic temple on the Acropolis, the Hecatompedon, and Sunium. In the two former it is the rising, the latter the setting star.

α Virginis or Spica must have been supposed to be sacred to Juno. The Heræum at Olympia agrees exactly with this view, and the Argive Heræum can be referred to no other; but, as the foundations of the earlier Heræum are not now visible, the exactness of the coincidence cannot be thoroughly established without re-excavating part of the site. There is nothing, however, inconsistent to this view in what is known about it. The nomenclature of the temple of Juno at Girgenti rests on a rather weak historical basis; but Spica entirely supports it.

There must have been something in common between the temples at Corinth, Ægina, and Nemea. The two last, at any rate, are reputed temples of Jupiter, and I have reason to think that also the Temple of Jupiter at Girgenti agrees with the same star—namely, Antares.

Approximate dates derived from the orientation of Greek temples.

Star.	Name of temple.	Place.	Month and day.	Year of foundation. B.C.	Star rising or setting.	No.
Pleiades (η Tauri) ...	Archaic temple of Minerva	Athens ...	April 20 ...	1495	R	1
	Hecatompedon temple of Minerva ...	Athens ...	April 25 ...	1120	R	2
	Temple of Minerva, Sunium	Sunium ...	October 20 ...	1125	S	3
Sirius	Temple of Ceres, Eleusis (for midnight mysteries)	Eleusis ...	September 14	1380	R	4
Fomalhaut (α Piscis Australis)	The same for sunrise	Eleusis ...	November 18	1350	S	5
Spica, *i.e.* α Virginis.	The Heræum, Olympia	Olympia ...	September 15	1300	R	6
	The Heræum, Argos	Argos ...	February ...	about the same time	S	7
	The Hertæum, Girgenti	Sicily ...	September 15	1180	R	8
α Arietis	Jupiter Olympius, Athens (temple attributed to Deucalion	Athens ..	April 1 ...	1135	R	9
	Jupiter Olympius, Olympia ...	Olympia ...	April 3 ...	760	R	10
Antares (α Scorpii) ...	Temple at Corinth	Corinth ...	May 1 ...	700	S	11
	Jupiter Panhellenius, Ægina	Ægina ...	May 6 ...	670	S	12
	Nemea, Temple of Jupiter	Nemea ...	similar to two last		S	13
Aquarius (ζ Aquarii)	Oldest temple at Epidaurus (the Hiero)	Epidaurus ...	July 29 ...	1270	S	14
	Older Erechtheum, Athens	Athens ...	August 9 ...	920	S	15
	Diana Brauronia	Athens ...	February 21	750	R	16
δ Corvi (?)	Temple of Themis, Rhamnus ...	Rhamnus ...	September ...	about 1150	R	17
	Temple of Nemesis, Rhamnus ...	Rhamnus ...	September ...	780	R	18
	Temples of later foundation for which no heliacal star has been found.					
	The Theseum	Athens ...				
	The new Erechtheum	Athens ...				
	The temple of Wingless Victory ...	Athens ...				

The above table of approximate results, which I have put together, must for the most part be considered preliminary, and subject to amendment when further particulars have been ascertained, which I am in hopes of being able to obtain in the course of the present season. Nevertheless, I do not think that as respects the examples mentioned in Nos. 1, 2, 9, 12, and 16, there will be much need of alteration, as of these I am already in possession, though not of all, yet of the most important measurements. Of the remainder I do not feel so confident, but there is still a good deal that can be pointed out in respect to some of them which is consistent with historical and architectural archæology.

Olympia must have been a sacred spot long before the Olympiads began to be dated, and the Heræum there appears to be the most archaic temple structure that exists in Greece. The date suggested by the orientation, 1300 B.C., does not seem unreasonable.

Then we come to the establishment of the Olympiads, which began 776 years B.C. Compare the date of the great Temple of Jupiter derived from its orientation, 760.

The temple at Corinth was thought by archæologists of the past generation to date from about the middle of the seventh century B.C. The date I get from its orientation is 700.

The temple at Ægina, it is evident from its architecture, is somewhat later than that at Corinth or the Olympian Jupiter. That is also the orientation view of the case. At the same time, I think that the interval between Nos. 11 and 12 ought to be more than thirty years. I rather expect that more exact measurements at Corinth will throw back somewhat the date of that example. Want of clear weather obliged me to be content with magnetic bearings at Corinth, and these may easily be at fault as much as 1°

F. C. PENROSE.

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VOLCANIC ACTION IN THE BRITISH ISLES.

AT the anniversary of the Geological Society, held on the 19th inst., the retiring President, Sir Archibald Geikie, gave the annual address, which was devoted to a continuation of the subject treated of by him last year. He now dealt with the history of volcanic action in this country from the close of the Silurian period up to older Tertiary time. The remarkable volcanic outbursts that took place in the great lakes of the Lower Old Red Sandstone were first described. From different vents over central Scotland, piles of lava and tuff, much thicker than the height of Vesuvius, were accumulated, and their remains now form the most conspicuous hill-ranges of that district. It was shown how the subterranean activity gradually lessened and died out, with only a slight revival in the far north during the time of the Upper Old Red Sandstone, and how it broke out again with great vigour at the beginning of the Carboniferous period. Sir Archibald pointed out that the Carboniferous volcanoes belonged to two distinct types and two separate epochs of eruption. The earlier series produced extensive submarine lava-sheets, the remains of which now rise as broad terraced plateaux over parts of the lowlands of Scotland. The later series manifested itself chiefly in the formation of numerous cones of ashes, like the *puys* of Auvergne, which were dotted over the lagoons and shallow seas in central Scotland, Derbyshire, Devonshire, and the south-west of Ireland. After a long quiescence, volcanic action once more reappeared in the Permian period; and numerous small vents were opened in Fife and Ayrshire, and far to the south in Devonshire. With these eruptions the long record of Palæozoic volcanic activity closed. No trace has yet been discovered of any volcanic rocks intercalated among the Secondary formations of this country, so that the whole of the vast interval of the Mesozoic period was a prolonged time of quiescence. At last, when the soft clays and sands of the Lower Tertiary deposits of the south-east of England began to be laid down, a stupendous series of fissures was opened across the greater part of Scotland, the north of England, and the north of Ireland. Into these fissures lava rose, forming a notable system of parallel dykes. Along the great hollow from Antrim northwards between the outer Hebrides and the mainland of Scotland, the lava flowed out at the surface and formed the well-known basaltic plateaux of that region.

The address concluded with a summary of the more important facts in British volcanic history bearing on the investigation of the nature of volcanic action. Among these Sir Archibald laid special stress on the evidence for volcanic periods, during each of which there was a gradual change of the internal magma from a basic to an acid condition, and he pointed out how this cycle had been repeated again and again even within the same limited area of eruption. In conclusion, he dwelt on the segregation of minerals in large eruptive masses, and indicated the importance of this fact in the investigation, not only of the constitution and changes of the volcanic magma, but also of the ancient gneisses where what appear to be original structures have not yet been effaced.

THE CENTENARY OF MURCHISON.

ON February 19, 1792, Roderick Impey Murchison was born at Tarradale, in Ross-shire. By a curious and appropriate coincidence, the anniversary of the Geological Society, the date of which is fixed by statute, fell this year on the 19th of the present month, the hundredth anniversary of the birth of the illustrious author of the "Silurian System." It was a further remarkable conjuncture that the President of the Society, who had to give the annual address, and take notice of the centenary, was Murchison's literary executor, who was designated by him as the first Professor of Geology in the chair which he founded in the University of Edinburgh, and who now fills the office which he held for so many years—that of Director-General of the Geological Survey. In referring to the doubly interesting features of this anniversary, Sir Archibald Geikie spoke of his great chief with warm admiration. The twenty years which have passed since Murchison's death enable geologists to make a truer estimate of Murchison's real achievements than was possible at the time when his commanding presence filled so prominent a place in the scientific world of his day. They have been able to correct some of his observations and discard some of his generalizations, yet the solid mass of original work done by him remains as a lasting memorial of his genius and industry. In the broad basis of facts, and in the skilful marshalling of these facts in their ordered relations, which distinguished his work among the Silurian rocks, the hand of a consummate master of geological investigation is to be traced. His name has become a household word in geology, and will go down to future ages as that of one of the great pioneers of the science.

Murchison, during all his scientific career, was closely associated with the Geological Society, and took a keen personal interest in its welfare. By his will he left a sum of money to found a medal and fund to be given annually for the reward and encouragement of geological research. This year the medal was awarded to Prof. A. H. Green, of Oxford, and the balance of the fund to Mr. Beeby Thomson. An interesting proof of the affectionate regard entertained for Murchison's memory was afforded by an announcement made by the President. He stated that, a few days before the meeting, an old friend of Murchison, who desired to remain unknown, had come to him and asked to be allowed to offer a slight tribute in remembrance of the man and his work, on his centenary, at the anniversary meeting of the Society. The President was requested to select two geologists (by preference Scotsmen) who were carrying on geological work in Murchison's spirit, and seeking to advance the special branches of research to which he devoted himself, and to present to each of them a cheque for £50, with a framed portrait of the author of the "Silurian System." Sir Archibald Geikie said that the task assigned to him was made comparatively easy by the terms of the generous gift. He had no doubt that the Society would agree with him that there were pre-eminently two Scottish geologists marked out as recipients of this benefaction, who were disciples of Murchison, and were carrying on his work, but with no slavish obedience to the opinions of their master, and who, by their conjoint work, alike with hammer and pen, well deserved this unexpected and appropriate reward—Mr. B. N. Peach and Mr. John Horne. As a touching addition to this pleasing incident, we have since learnt that while the anniversary was being held at Burlington House, the faithful friend who had made this offering to Murchison's memory was engaged in the cemetery at Brompton carefully brushing and washing his tomb. Driving snow was falling at the time from a gloomy sky, in strange contrast with the glow of affection that was piously renovating the inscription that records the name and resting-place of one of the great leaders of modern geology.

H. W. BATES, THE NATURALIST OF THE AMAZONS.

HENRY WALTER BATES was a native of Leicester, and was engaged in his father's warehouse when, about the year 1845, he made the acquaintance of Alfred Russel Wallace, then English master in the Collegiate

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School of that town. Bates was at that time an ardent entomologist, while Wallace was chiefly interested in botany; but the latter at once took up beetle-collecting, and after he left Leicester the following year kept up an entomological correspondence with his friend. Two years later Wallace proposed a joint expedition to Para in order to collect insects and other natural objects, attracted to this locality by the charming account of the country in Mr. W. H. Edwards's "Voyage up the Amazon," a choice confirmed by the late Edward Doubleday, who had just received some new and very beautiful butterflies collected near the city of Para. The two explorers sailed from Liverpool in April 1848, in a barque of 192 tons burthen, one of the very few vessels then trading to Para, and the results of their journey are well known to naturalists. They made joint collections for nearly a year while staying at or near Para, but afterwards found it more convenient to take separate districts and collect independently. Bates spent eleven years in the country, divided pretty equally between the lower and the upper Amazon, and he amassed a wonderful collection of insects. Returning home in 1859, he devoted himself to the study of his collections, and in 1861 read before the Linnean Society his remarkable and epoch-making paper on the Heliconidæ of the Amazon Valley. In this paper, besides making important corrections in the received classification of this group and its allies, he discussed and illustrated in the most careful manner the wonderful facts of "mimicry," and for the first time gave a clear and intelligible explanation of the phenomena, their origin and use, founded on the accepted principles of variation and natural selection. In spite of countless attacks—usually by persons who are more or less ignorant of the facts to be explained—this theory still holds its ground, and notwithstanding the constant accumulation of new facts, and its discussion by new writers, it has never been more clearly or more fully explained than by its original discoverer.

So early as March 1860, Mr. Bates commenced a series of papers for the Entomological Society, under the title of "Contributions to an Insect Fauna of the Amazon Valley." These were at first devoted to the Diurnal Lepidoptera, and in one of them he gave a new classification of the whole group, founded chiefly on the structure of the legs, and leading to the conclusion that the Papilionidæ formed one of the lowest families, while the Nymphalidæ were the highest. This classification has been very generally adopted by entomologists, though there are a few dissentients, who hold that the principle adopted to determine the rank or grade of the respective families is an unsound one. Later on he wrote many papers on the various groups of Longicorn beetles; and finding that his circumstances and the time at his disposal did not allow him to keep up and study two such extensive groups as the Coleoptera and Lepidoptera, he parted with his fine collection of South American butterflies to Messrs. Salvin and Godman, and thereafter devoted himself exclusively to the study of Coleoptera. Later still, he almost confined his attention to the Carabidæ, on which important group he became a recognized authority. His largest works in this direction were his contributions to the "Biologia Centrali-Americana": Vol. I., Part 1 (Geodephaga); Vol. II., Part 2 (Pectinicornia and Lamellicornia); Vol. V. (Longicornia). A supplement to the Geodephaga has since been published in the Transactions of the Entomological Society of London for 1890 and 1891; and a supplement to the Longicornia was in course of preparation, but not finished at the time of his death.

In 1864, he was appointed Assistant Secretary to the Royal Geographical Society, an appointment he held till his death. Besides editing the Journal and Proceedings, and carrying on an immense correspondence with travellers and others in every part of the world, he had practically the entire management of the large establishment of the Society, and the chief burden of the arrangements for the various meetings, as well as those for the Geographical Section of the British Association. There can be little doubt that it was the confinement and constant strain of this work that weakened his constitution and shortened a valuable life.

When we consider the originality and clearness of exposition in his first great paper on "Mimicry," the accuracy and fulness of knowledge displayed in his systematic and descriptive work, and the power of observation and felicity of style which characterizes "The Naturalist on the Amazons," we cannot but regret that circumstances should have compelled him to devote so much of his time and strength to the mere drudgery of office work, and be thereby to a great extent debarred from devoting himself to those more congenial pursuits in which he had shown himself so well fitted to excel.

His high reputation, both as a hard-working entomologist and philosophic naturalist, led to his being twice chosen President of the Entomological Society of London, first in 1869, and again in 1878; while he was elected a Fellow of the Royal Society in 1881. His somewhat rugged features, quiet, unassuming manners, and thoughtful utterance, must be familiar to all who have attended the evening meetings of the Royal Geographical Society during the last twenty-seven years. Rarely has any Society had a more efficient secretary, who not only carried on its work with accuracy and judgment, but also gained the respect and esteem of all who came in contact with him. He died on Febuary 16, at the age of sixty-seven.

A. R. W.

THOMAS ARCHER HIRST.

WE regret to have to record the death of Dr. Hirst, the well-known mathematician. He was the youngest of the three sons of Mr. Thomas Hirst, a woolstapler, and was born at Heckmondwike, in Yorkshire, on April 22, 1830. In 1844 he became an articled pupil of Mr. Richard Carter, land agent and surveyor at Halifax; but afterwards he went to Germany, and studied at several Universities, taking the degree of Doctor of Philosophy at Marburg in 1852. His intercourse with Steiner, at Berlin, gave a strong impulse to his studies, and ultimately determined their character. Dr. Hirst on his return to England filled the vacancy at Queenwood College caused by Tyndall's appointment to the Professorship of Natural Philosophy in the Royal Institution. The work at Queenwood occupied most of his time, so that during the three years for which he held the post his only original paper was a note "On the Existence of a Magnetic Medium" (R.S. Proc., vii., 1854).

Towards the close of 1854 he married, and in consequence of his wife's delicate health he passed the winter of 1856–57 in the south of France. During this period he wrote two papers "On Equally Attracting Bodies" (*Phil. Mag.*, xiii., xvi.).[1] On the return journey Mrs. Hirst died (1857) in Paris. After this sad event Dr. Hirst spent six weeks with Prof. Tyndall on the *mer de glace* (cf. "Glaciers of the Alps"): he then returned to Paris, and attended the lectures of Chasles, Liouville, Lamé, and Bertrand. At this time he translated Poinsot's famous memoir "On the Percussion of Bodies," and contributed a paper, "Sur le Potentiel d'une Couche infiniment mince comprise entre deux Paraboloides Elliptiques" (Liouville, *J. de M.*, ii., 1859).[2] The winter of 1857–58 was spent in Rome. Here was written for Tortolini's *Annali* the memoir "Sur la Courbure d'une Série de Surfaces et de Lignes" (vol. ii., 1859), an abstract of which was subsequently published in the *Quarterly Journal of Mathematics*. In these stirring times Dr.

[1] Cf. Chasles, "Rapport sur les Progrès de la Géométrie," p. 144.
[2] Chasles, "Rapport," p. 303.

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Hirst received a cordial welcome from the mathematicians of Southern Italy, and then going north he followed the victorious armies as far as San Martino and Solferino. After the Peace of Villafranca he visited the town of Cremona, and here commenced an acquaintance of life-long duration with Prof. Luigi Cremona.

In 1860, Dr. Hirst took up his residence in London, and for a short time took the advanced mathematical classes in University College School, in consequence of Mr. Cook's illness, and on that gentleman's death he became his successor. This office Dr. Hirst held for five years, and here, with Prof. Key's full concurrence (see Dr. Hirst's preface to Wright's "Elements of Plane Geometry," 1868), he taught geometry to classes of beginners without the use of "Euclid." Subsequently, in 1870, at the request of the Ladies' Educational Association, he gave a course of twenty-four lectures on the subject of geometry to a class of sixty ladies at St. George's Hall. The syllabus of these lectures was printed at the time. He was so well satisfied with the results of his attempt that when, in 1871, the Association for the Improvement of Geometrical Teaching was started, though he had taken no part, directly, in its formation,[1] he at once gave in his adhesion to the movement, and contributed very materially to its success, by his accepting the office of President, and by his doing yeoman's service during his tenure of the office (1871–78). Previous to this Dr. Hirst had, in 1865, been elected Professor of Mathematical Physics in University College. This post he vacated in 1867, when he succeeded Prof. de Morgan in the Chair of Pure Mathematics. It was on January 16, 1865,[2] that the London Mathematical Society was started. Of this Society Dr. Hirst was one of the pillars, and it was in a great measure through his fostering care that it has made the mark it has. He served on the Council from 1865 to November 1885, and for the session 1890–91. He vacated the office of Treasurer when he was elected President for the years 1872–73, 1873–74.

In 1870, Dr. Hirst was appointed to the new office of Assistant Registrar to the University of London, and thereupon resigned his Professorship, and the General Secretaryship of the British Association, which he had held from 1866. In 1873, when the Royal Naval College was founded, he became the Director of Studies, and held the office for ten years, when the precarious state of his health necessitated his retirement, and the passing of several winters abroad. He died on February 16.

In 1861, Dr. Hirst was elected a Fellow of the Royal Society. He was three times a member of the Council of the Society, and twice one of its Vice-Presidents. In 1883 one of the Royal Medals was awarded to him for "his investigations in pure geometry; and, more particularly for his researches into the correlation of two planes and into the complexes generated by them." He was a Fellow of the Royal Astronomical Society, a member of the Physical Society, and of several Continental Societies. He served for some years on the Council of University College, London, and was also a member of the Senate of the University of London.

Dr. Hirst revised the mathematical articles in Brande's "Dictionary of Arts and Sciences," and contributed new ones; and published a translation of Clausius's treatise on "The Mechanical Theory of Heat" (1867).

The following titles of papers may be mentioned:—"On the Volumes of Pedal Surfaces" (Phil. Trans., 1863; *Crelle*, lxii., 1863; and Tortolini, *Annali*, v., 1863). "On the Quadric Inversion of Plane Curves" (R.S. Proc., 1865; cf. Chasles, "Rapport," p. 167, "Ce mémoire est un travail fort complet"). This was his first *purely* geometrical paper. It was translated by Cremona in the *Annali di Matematica* (vii., 1865), and a form of it is published in the *Nouvelles Annales* (v., 1866). His remaining papers, mainly contributed to the London Mathematical Society's Proceedings, are:—"On Correlation in Space" (abstract of Presidential Address, 1874, Proc., vi.). "Note on the Correlation of Two Planes" (Proc., viii.). "On Cremonian Congruences" (Proc., xiv.). "On Congruences of the Third Order and Class" (Proc., xvi.). "On Cremonian Congruences contained in Linear Complexes" (Proc., xvii.). "On the Correlation of Two Spaces, each of Three Dimensions" (Proc., xxi.). "On the Complexes generated by Two Correlative Planes" (*Chelini* Memorial Volume, 1881). "Sur la Congruence Roccella" (*Circolo Matematico*, 1886).

[1] Opening remarks in the Presidential Address, A.I.G.T., First Report, January 17, 1871 (cf. also NATURE, vol. ii. pp. 65, 141, 164).
[2] Memoir of Augustus de Morgan, pp. 280–86.

DR. THOMAS STERRY HUNT.

DR. T. STERRY HUNT, who died at New York on the 12th of this month, in his sixty-sixth year, was widely known from his geological works, especially those relating to chemical geology. For some years past he had been in feeble health, suffering much from heart-disease. Early in this year he was attacked with influenza, from which he seemed to be recovering, but a relapse occurred, from which he failed to rally. Born on September 6, 1826, at Norwich, in Connecticut, he was educated for the medical profession, but in 1845 became assistant to Prof. B. Silliman at Yale College, and was also chemist to the Geological Survey of Vermont. In 1847 he joined the Geological Survey of Canada, under Sir W. Logan, as chemist and mineralogist. From 1856 to 1862 he was Professor of Chemistry at Laval University in Quebec, giving his lectures in French. From 1872 to 1878 he was Professor of Geology at the Massachusetts Institute of Technology. He was elected a Fellow of the Royal Society in 1859, and in 1881 received the honorary degree of LL.D. at Cambridge. Dr. Hunt was one of the founders of the International Geological Congress at Philadelphia, in 1876; he attended the meetings of the Congress at Paris in 1878, Bologna in 1881, Berlin in 1885, and London in 1888, taking an active part in the proceedings of each.

Although by birth a citizen of the United States, he is best known as a Canadian geologist, and, after retiring from the Canadian Survey, he lived for some years in Montreal. But latterly he preferred to consider himself once more as belonging to the United States, and for a few years before his death was a resident in New York.

Dr. Hunt's most important geological work was done in connection with the Geological Survey of Canada, with and under Logan. They led the way in the study of the Archæan rocks of that area, and Hunt gave to them many of the names which have since become well known, and too widely used, in the Archæan controversy. His work on the geology of petroleum was of high value, and he long ago clearly stated generalizations as to its occurrence which later investigations, over wider areas in North America and in other districts, have fully verified. Other important researches, published in the official Reports of the Canadian Survey and elsewhere, related to limestone, dolomite, and gypsum; salt; the chemistry of natural waters; the porosities of rocks; rock-weathering, &c. The well-known "Geology of Canada," issued by Logan in 1863 as Director of the Survey, was in large part written by Hunt, the parts on lithology and on economic geology being almost entirely his; he likewise read the proofs of the whole. He also wrote much on Alpine and Italian geology, and on the classification of the older Palæozoic rocks; in the Cambro-Silurian controversy he was a warm advocate of Sedgwick. The origin of serpentine was also a favourite subject, he stoutly maintaining its aqueous origin. As regards the ancient crystalline rocks generally, he to a large extent

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reverted to the Wernerian view, but with some important modifications; these he explained in his "crenitic hypothesis."

Dr. Hunt's earlier papers (1846–49) were wholly on chemistry and mineralogy, and to these subjects he always gave much attention. Some of his latest writings are purely chemical, dealing mainly with the more speculative aspects of that science. Perhaps in these questions, as is certainly the case with many of his theoretical views on geology, Dr. Hunt failed to carry conviction to the minds of his fellow-workers; and it may well be doubted if some of his views on these matters will ultimately add to his scientific reputation. But it would be unjust on this account to ignore the mass of solid work which he accomplished, and the suggestive hints which are scattered throughout his writings.

Dr. Hunt was a man of wide reading and general culture; he possessed a marvellous memory, and great conversational powers. In his company one might for hours forget that science was his special study, so well informed was he in history, literature, and philosophy. His conversation on such subjects possessed an additional interest from his personal acquaintance with many American authors. He was thus an excellent travelling companion, and the writer will not soon forget with what thrilling effect he recited Macaulay's "Horatius," within sight of Cortona and its Etruscan walls.

W. TOPLEY.

NOTES.

THE date of the Bakerian Lecture to be delivered before the Royal Society has been altered to March 10. Prof. James Thomson has chosen as his subject "The Trade Winds."

THE general arrangements for the Edinburgh meeting of the British Association have now been completed. The first general meeting will be held on Wednesday, August 3, at 8 p.m., when Dr. William Huggins, F.R.S., will resign the chair, and Sir Archibald Geikie, For.Sec.R.S., Director-General of the Geological Survey of the United Kingdom, President-Elect, will assume the Presidency, and deliver an address. On Thursday evening, August 4, at 8 p.m., there will be a *soirée*; on Friday evening, August 5, at 8.30 p.m., a discourse will be delivered by Prof. A. Milnes Marshall, F.R.S.; on Monday evening, August 8, at 8.30 p.m., a discourse on magnetic induction will be delivered by Prof. J. A. Ewing, F.R.S.; on Tuesday evening, August 9, at 8 p.m., there will be another *soirée*; and on Wednesday, August 10, the concluding general meeting will be held at 2.30 p.m. The different Sections will assemble for the reading and discussion of Reports and other communications on Thursday, August 4, and on the following Friday, Saturday, Monday, and Tuesday. The delegates of Corresponding Societies will meet on Thursday, August 4, and Tuesday, August 9, at 3.30 p.m. Excursions to places of interest in the neighbourhood of Edinburgh will be made on the afternoon of Saturday, August 6, and on Thursday, August 11.

IT is proposed that Englishmen shall celebrate the fourth centenary of the discovery of the New World, and do honour to the memory of Columbus, by establishing in Jamaica a marine biological station on the lines of the marine laboratories at Naples and Plymouth. The institution would be called "the Columbus Marine Biological Station." An excellent letter on the subject by Lady Blake appeared in the *Times* on Wednesday. The scheme has been laid before Prof. Huxley, Prof. Ray Lankester, Prof. Flower, Dr. Günther, Dr. Ball, Sir John Lubbock, Mr. Scott, Mr. Sclater, and numerous other scientific men, all of whom warmly approve of it. For the establishment of the laboratory on a sound basis an outlay of £15,000 will be required. The following have consented to receive subscriptions:—Prof. Ray Lankester, Oxford; Dr. Günther, British Museum (Natural History), Cromwell Road; Dr. Ball, Science and Art Museum, Dublin; the Duchess of St. Albans, Bestwood Lodge, Arnold, Notts.; and Messrs. Coutts and Co., bankers, 59 Strand. The Hon. Walter Rothschild, 148 Piccadilly, has undertaken the duties of honorary secretary.

ON Saturday last a meeting was held in the Combination Room of St. John's College, Cambridge, to discuss a proposal for the provision of a national monument to the late Prof. Adams. The Rev. Dr. Taylor, the Master of the College, presided; and among those present were Dr Peile (Master of Christ's, and Vice-Chancellor), Dr. Ferris (Master of Caius), Dr. Porter (Master of Peterhouse), Mr. Aldis Wright (Vice-Master of Trinity), Dr. Forsyth, Prof. Hughes, Dr. Hobson, Prof. Thomson, Dr. Glaisher, Dr. Frost, Dr. Sandys, Prof. Mayor, and Sir George G. Stokes, M.P. The Master said that Prof. Adams had memorials in Cambridge in the Adams Prize, and his portraits at that College and at Pembroke. His own work was his monument in the annals of science. They wished to commemorate his name and personality in the eyes of the world in that central sanctuary where, age after age, they commemorated their national types of various kinds of supreme excellence which were the glory of the world. The first suggestion of that came to him from Archdeacon Farrar. The suggestion had been mentioned at a College meeting and by it adopted, and they were met that day to carry it out. He thought the better method would be to form a large and influential committee, containing the most prominent names in mathematics and science, which would enable them to show there was a general feeling in favour of it. Then he thought the request might be made to the Dean and Chapter, on behalf of the Committee, by the Chancellor, the Duke of Devonshire, and in a letter which he had received from the Duke he stated that he should be very glad to give any assistance in his power to carry out the wishes of the Committee. Among those who had agreed to join the Committee were the Astronomer-Royal, the Master of Trinity, Dr. Salmon (Provost of Trinity College, Dublin), the Master of Corpus, Mr. Justice Romer, Prof. Jebb, Mr. Courtney, Lord Rayleigh, Prof. Newton, the Gresham Professor of Astronomy, Prof. Cayley, and Sir Donald Smith (Chancellor of Montreal University), who asked to be allowed to subscribe £100. The following motion, proposed by the Master, seconded by Sir G. G. Stokes, and supported by Dr. Glaisher and Prof. Liveing, was carried unanimously: "That the late Prof. John Couch Adams, by his discovery of the planet Neptune, and other masterly work, published or unpublished, is entitled to be named with the great astronomers of the world; and that this meeting pledges itself (so far as in it lies) to promote and carry out the scheme for placing a memorial to the late Professor in Westminster Abbey." The following resolutions were also carried:—"That the memorial consist of a bust, with tablet and inscription." "That a Committee be formed (with power to add to their number) to carry out the scheme; that the Master of Pembroke College and Prof. Liveing be the Treasurers, and the Master of Peterhouse, Dr. D. MacAlister, and Dr. Glaisher the Secretaries, and that such and such persons be the Executive Committee." "That any surplus from subscriptions after payment of the necessary expenses to be used in the first instance to defray the cost of presenting copies of the collected papers of Prof. Adams to learned Societies and libraries at home and abroad, and that the remainder (which, if of sufficient amount, shall be constituted a permanent memorial fund) be offered to the Master and Fellows of St. John's College to form an Exhibition or Scholarship fund for the encouragement of the study of mathematics or physics by the undergraduate students of the

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College, such fund to be administered in such a manner as the Masters and Fellows may from time to time determine."

AT a meeting of the electors to the Lowndsean Professorship of Astronomy at Cambridge, held on February 20, Sir Robert S. Ball, Astronomer-Royal for Ireland, was elected to succeed the late Prof. Couch Adams. Sir Robert Ball is fifty-one years of age. He is a native of Dublin, and was educated at Trinity College. When twenty-five years old, he was appointed Lord Rosse's astronomer at Parsonstown. He became Professor of Applied Mathematics and Mechanism at the Royal College of Science of Ireland in 1867, and Professor of Astronomy at the Dublin University, and Astronomer-Royal for Ireland, in 1874. In 1873, he had been made a Fellow of the Royal Society. He has done much by his writings and lectures to create and foster a popular interest in astronomical study. In relation to this appointment we give the following extract from the *Cambridge University Reporter* of February 23:—The Council of the Senate beg leave to report to the Senate as follows: "The arrangement by which 'the superintendence and management of the Observatory' were intrusted to the late Lowndsean Professor (Grace, May 2, 1861, Ordinances, p. 239) has now terminated, and as no provision has been made for the future direction of the Observatory, the Council think it desirable that a special Syndicate should be appointed to consider the question." The Council therefore recommend: "That a Syndicate be appointed to consider what provision should be made for the future superintendence and management of the Observatory, and to report to the Senate before the end of the present Lent Term." "That the Vice-Chancellor, Dr. Ferrers, Master of Gonville and Caius College, Prof. Sir G. G. Stokes, Dr. Glaisher, Prof. Liveing, Prof. Thomson, and F. Whitting, M.A., of King's College, be appointed a Syndicate to consider what provision should be made for the future superintendence and management of the Observatory, and to report to the Senate before the end of the present Lent Term."

THE Queen has approved the appointment of Dr. Thomas Clifford Allbutt, F.R.S., to be Regius Professor of Physic in the University of Cambridge, in the room of the late Sir George Paget.

MR. J. SCOTT KELTIE has been appointed to succeed the late Mr. H. W. Bates, F.R.S., as Assistant Secretary of the Royal Geographical Society.

AT the meeting of the Royal Geographical Society on Monday, Mr. Theodore Bent read before a large audience a paper on his recent exploration among the Zimbabwe and other ruins. The paper was one of great interest. Mr. Bent said that, with his wife and Mr. Robert Swan, he went to Mashonaland primarily to examine the ruins of the Great Zimbabwe. These ruins, so named to distinguish them from the numerous minor Zimbabwes scattered over the country, were situated in south latitude 20° 16′ 30″, and east longitude 31° 10′ 10″, at an elevation of 3300 feet above the sea-level, and formed the capital of a long series of such ruins stretching up the whole length of the west side of the Sabæ River. They covered a vast area of ground, and consisted of the large circular building on a gentle rise with a network of inferior buildings extending into the valley below, and the labyrinthine fortress on the hill, about 400 feet above, naturally protected by huge granite boulders and a precipice running round a considerable portion of it. Mr. Bent gave a minute description of the ruins, drawing attention to evidence that their ancient inhabitants must have been given to the grosser forms of native worship. Perhaps the most interesting of their finds in one portion were those in connection with the manufacture of gold. Mr. Bent held that the ruins and the things in them were not in any way connected with any known African race; the objects of art and the special cult were foreign to the country altogether, where the only recognized form of religion was, and had been since the days when the early Portuguese explorers penetrated into it and El Masoudi wrote, that of ancestor worship. It was also obvious that the ruins formed a garrison for the protection of a gold-producing race in remote antiquity. So we must look around for such a race outside the limits of Africa, and it was in Arabia that we found the object of our search. All ancient authorities speak of Arabian gold in terms of extravagant praise. Little, if any, gold came from Arabia itself; and here in Africa gold was produced in large quantities, both from alluvial and from quartz, from the remotest ages. A cult practised in Arabia in early times was also practised here; hence there was little room for doubt that the builders and workers of the Great Zimbabwe came from the Arabian peninsula. He had no hesitation in assigning this enterprise to Arabian origin, and to a pre-Mahomedan period.

AT the anniversary meeting of the Geological Society, held at Burlington House on Friday last, the following officers were elected:—President: W. H. Hudleston, F.R.S. Vice-Presidents: Prof. T. G. Bonney, F.R.S., L. Fletcher, F.R.S., G. J. Hinde, Prof. J. W. Judd, F.R.S. Secretaries: Dr. H. Hicks, F.R.S., J. E. Marr, F.R.S. Foreign Secretary: J. W. Hulke, F.R.S. Treasurer: Prof. T. Wiltshire. The following are the members of the Council: Prof. J. F. Blake, Prof. T. G. Bonney, F.R.S., James W. Davis, R. Etheridge, F.R.S., L. Fletcher, F.R.S., Prof. C. Le Neve Foster, Sir A. Geikie, F.R.S., A. Harker, H. Hicks, F.R.S., G. J. Hinde, W. H. Hudleston, F.R.S., Prof. T. McKenny Hughes, F.R.S., J. W. Hulke, F.R.S., Prof. J. W. Judd, F.R.S., J. E. Marr, F.R.S., H. W. Monckton, Clement Reid, J. J. H. Teall, F.R.S., W. Topley, F.R.S., Prof. T. Wiltshire, Rev. H. H. Winwood, H. Woodward, F.R.S., H. B. Woodward.

IN the February number of the *Kew Bulletin* much useful information on sisal hemp (*Agave rigida*, Mill.) is presented. The cultivation of sisal hemp has lately been developed to so remarkable an extent in the Bahamas that hemp-growing has become, for the moment, one of the most prominent of the new industries of the tropics. The *Bulletin* mentions most of the localities where plants of sisal hemp are now found, and the material it has collected will be of great service to all who may think of embarking in a fibre industry at the present time.

AMONG the other contents of the *Kew Bulletin* is an interesting correspondence between Mr. Thiselton-Dyer and the Vice-Chairman of the Middlesex County Council on the question of instruction in horticulture. There is so much vague talk nowadays about technical education that all who wish the words to be used in the right sense will read with pleasure Mr. Thiselton-Dyer's remarks on the proper way of learning the art of cultivating plants. "The cultivation of plants," he says, "is an art which can only be acquired by practice, and therefore, it appears to me, cannot be taught in the lecture-room any more than painting or shoe-making. I know of no royal or theoretical road to the acquisition of a competent or even useful knowledge of the gardener's art except by beginning at the bottom and going through every operation, from the most elementary to the most difficult and refined. If an intelligent young man does that, and keeps his eyes open, he may become a successful gardener. But the mere reading of books and attendance on lectures will never, in my judgment, make anyone even a moderately competent gardener."

A REPORT on the botanical collections made by Dr. Brown Lester, Medical Officer to the Gambia Delimitation Commission,

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was published in the *Kew Bulletin* for October and November 1891. A translation of the botanical section of the reports made by the French members of the Commission is given in the February number of the *Bulletin* for the purpose of supplementing Dr. Brown Lester's notes.

APPENDIX II., 1892, of the *Kew Bulletin* contains a list of the new garden plants of the year 1891. The list includes, besides the plants brought into cultivation for the first time in 1891, the most noteworthy of those which have been reintroduced after being lost from cultivation. Other plants in the list have been in gardens for several years, but either were not described or their names had not been authenticated till recently.

IN their Irish Education Bill the Government propose that a large proportion of the funds at their disposal for the improvement of national education in Ireland shall be spent for the benefit of the teachers, who as a class have hitherto been too much neglected. The rest of the amount will be devoted to a capitation grant, and to the freeing of all schools in which the fees do not exceed six shillings a year per child. Attendance at elementary schools, if the Bill becomes law, will be compulsory in Irish towns, but in rural disiricts it will be open to the people to accept or reject compulsion as they may think fit.

THE National Association for the Promotion of Technical and Secondary Education has issued an appeal to the electors of the London County Council on the subject of technical instruction. As everyone interested in technical education knows, London has devoted to the relief of the rates the whole of its share of the grant obtained from the proceeds of the beer and spirits duties. This has been done in direct opposition to the wish of Parliament; and the Association has no difficulty in showing that the grant will be continued only if it is used for the purposes to which the House of Commons intended it to be applied. It may be hoped that the appeal will be widely read, and that voters will perceive that it deals with a matter by which their interests must sooner or later be vitally affected.

THE principal article of interest to meteorologists in the *American Meteorological Journal* for January is by A. L. Rotch, on the mountain meteorological stations of the United States. At the present time the only stations in operation throughout the year are the Lick Observatory, in California, and the Blue Hill Observatory, in Massachusetts. That on Mount Washington (6280 feet above the sea) was established in 1870, and partially closed in 1887; during the three following years it was opened during the summer months only. At no other station in the world was such severe weather experienced, as the highest wind velocity often occurred with the lowest temperature. During a storm in February 1876, when the temperature fell to − 50°, a wind velocity of 184 miles an hour was recorded. In foggy weather the frost formed upon the anemometer cups in such quantity as to break off the arms. The observations at this station have been much lessened in value, owing to their not being published in detail, and to the want of a low-level station for comparison. The Blue Hill Observatory is only 640 feet above the sea, and was opened in 1885. The hourly values for five years have been printed in the Harvard College Observatory. For several years hourly observations of clouds have been made, with a view to benefit weather predictions. The Observatory on Pike's Peak, Colorado (14,134 feet), was built in 1873, and for fifteen years was maintained by the Signal Service. It was closed in 1888, and the observations have been published in the Annals of the Harvard College Observatory. The average annual temperature was 19°, and the extremes 64° and − 39°. Pike's Peak is remarkable for its electrical storms. When the air is moist, and generally when snow is falling, sparks emanate from the fingers of the outstretched hands; but the station was only once struck by lightning. The Lick Observatory is on Mount Hamilton, 4300 feet above the Pacific Ocean, which is plainly visible from the summit. Fragmentary observations have been made at various other stations, the most important of which were those by Prof. Langley, on Mount Whitney, California, in 1881, which have served to change the theory of the nature of the heat received from the sun, and to show that the sun is much hotter than had been supposed. The article is accompanied by photographic illustrations of several of the stations.

ELECTRICITY is being applied to a novel use in the U.S. Navy. Four electric fans have been placed by the Crocker Wheeler Company in the turrets of the powerful iron vessel *Miantonomah*, the intention being that they shall blow away the smoke from the guns.

AN interesting compound of carbon with the metal barium, possessing the composition C_2Ba, is described by M. Maquenne in the current number of the *Comptes rendus*. It may be considered, perhaps, as an acetylide of barium—that is, a compound formed by the replacement of the hydrogen of acetylene, C_2H_2, by metallic barium. For immediately it is brought in contact with water pure acetylene gas is evolved with great rapidity. M. Maquenne has obtained the new substance by the direct action of metallic barium, employed in the form of an amalgam consisting of one part barium and four parts mercury, upon powdered retort-charcoal. Upon distilling such a mixture in a current of hydrogen, when the mercury had been expelled and the temperature attained redness, an energetic reaction was found to occur between the barium and the carbon, with production of the new carbide or acetylide. The hydrogen took no part in the reaction, and M. Maquenne has subsequently found that it may be replaced by nitrogen; the latter, however, being less advantageous, inasmuch as the carbide produced is then admixed with more or less cyanide. The new substance, as obtained when hydrogen is employed to furnish the atmosphere, consists of a grey, friable mass, which remains quite unaltered when heated to bright redness. The moment, however, it is thrown into cold water it is decomposed, with a rapid effervescence of a gas which possesses the odour of acetylene, burns in the air with a luminous flame, precipitates a red substance resembling acetylide of copper from an ammoniacal solution of cuprous chloride, and, in short, possesses all the properties of acetylene. M. Maquenne adds that the acetylene thus obtained is remarkably pure. The reaction with water may be expressed by the equation—

$$C_2Ba + 2H_2O = C_2H_2 + Ba(OH)_2.$$

Barium acetylide would appear to be analogous to the compounds obtained by M. Berthelot by heating the metals of the alkalies in a current of acetylene, and also to the acetylide of calcium prepared by Wöhler. The direct formation of this substance from barium and carbon, together with its reaction with water, afford another mode of synthesizing acetylene, which M. Maquenne considers to be of interest from the point of view of the formation of the natural hydrocarbons. He considers it probable that other metals possess this same property of forming acetylides under the influence of high temperatures. If, therefore, as M. Berthelot has attempted to show, it is a fact that acetylene forms the primary material, or starting-point, for the formation of other hydrocarbons, it is quite possible that such compounds of metals with carbon, upon coming in contact with water under conditions of more or less pressure, may give rise to the production of the immense stores of natural hydrocarbons, such as those which exist in the petroleum wells of Russia and the New World.

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THE additions to the Zoological Society's Gardens during the past week include a Sykes's Monkey (*Cercopithecus albigularis* ♀) from East Africa, presented by Mr. G. N. Wylie; a Beatrix Antelope (*Oryx beatrix* ♀), an Indian Gazelle (*Gazella bennetti*) from Arabia, presented by Lieut.-Colonel Talbot; a Goshawk (*Astur palumbarius*), European, presented by Captain Noble; a Common Quail (*Coturnix communis*), European, presented by W. K. Purnell; a Hybrid Goose (between *Anser cinereus* and *A. brachyrhynchus*), captured in Holland, presented by Mr. F. E. Blaauw, C.M.Z.S.; a Gould's Monitor (*Varanus gouldi*), a Stump-tailed Lizard (*Trachydosaurus rugosus*) from New South Wales, presented by Mr. T. Hellberg; a Chub (*Leuciscus cephalus*), British fresh waters, presented by Mr. H. E. Young; two Yaks (*Poephagus grunniens* ♂ ♀) from Tibet, three Gigantic Salamanders (*Megalobatrachus maximus*) from Japan, deposited; an Azara's Agouti (*Dasyprocta azaræ*), a Pucheran's Hawk (*Asturina pucherani*), a Sulphury Tyrant (*Pitangus sulphuratus*), two Short-winged Tyrants (*Machetornis rixosa*) a Brown Milvago (*Milvago chimango*), an Orange-billed Coot (*Fulica leucoptera*), a Cayenne Lapwing (*Vanellus cayennensis*), six Rosy-billed Ducks (*Metopiana peposaca* 3 ♂ 3 ♀) from South America, purchased; an American Bison (*Bison americanus* ♂) from North America, received in exchange; a Gayal (*Bibos frontalis* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE SOLAR DISTURBANCE OF 1891, JUNE 17.—In the October number of the *Observatory* Mr. H. H. Turner publishes an article on the luminous outburst on the sun observed by M. Trouvelot on June 17, and recorded in these columns on July 9. The disturbance was of such an unusual character that M. Trouvelot hazarded the suggestion that it was possibly accompanied by perturbations of the magnetic elements. Mr. Whipple was good enough to look over the Kew curves to see if they showed any such variations, and a negative result was obtained. Mr. Turner, however, after an examination of the Greenwich records has succeeded in finding "a very minute, though unmistakable, disturbance at almost precisely the time noted by Trouvelot. . . . The disturbance is smaller than many others on the same day, although the day itself was very quiet: but it differs from others in its abruptness, which is clearly shown in all three curves. The change in declination is only about 1′, and in H.F. 0·0005 of the whole H.F." Diagrams illustrating these fluctuations accompanied Mr. Turner's paper. It seemed strange that the Kew and the Greenwich records should differ in their indications, so a further enquiry was sent to Mr. Whipple, who replied as follows:—"I have again referred to the curves of June 17, 1891, and fail to find any trace of what can by any means be termed to be a magnetic disturbance at the time in question—accepting Sabine's interpretation of a magnetic disturbance (see Phil. Trans, vol. cliii., p. 274), and so avoiding loose expressions. According to the *Observatory*, October 1891, Father Sidgreaves is quite of our opinion as to the case in point." The evidence in favour of a magnetic disturbance simultaneously with Trouvelot's observation is thus not very strong.

PHOTOGRAPHY OF SOLAR PROMINENCES.—In a communication to the Paris Academy on February 8, M. Deslandres described some new results obtained by him in the photography of solar prominences. The object of the research was to photograph the spectra of prominences further into the ultra-violet than had previously been done. In July of last year, M. Deslandres, following Prof. Hale, succeeded in photographing the spectra to λ 380. He has now been able to obtain negatives upon which the spectrum extends from λ 410 to λ 350. In order to obtain this result, a siderostat with a mirror 8 inches in diameter has been employed to project the sun's image, a Rowland grating has been used to produce the spectra, and the lenses of the observing telescope have been made of quartz. The photographs show eight bright lines of the ultra-violet hydrogen series, and it is believed that observations made from an elevated station would lead to the detection of the remaining two. The line a little more refrangible than hydrogen α (λ 388), is also recorded upon the plates. Photographs have been taken of the spectra of spots and faculæ. The calcium lines at H and K often appear bright upon them, and are always stronger than the hydrogen lines. But no new facts appear to have been discovered in this direction of work.

ON THE VARIATION OF LATITUDE.—Dr. S. C. Chandler has published a series of papers on the variation of latitude, in the *Astronomical Journal* from No. 248 to No. 251. The general result of a wide discussion indicates a revolution of the earth's axis of inertia about that of rotation from west to east, with a radius of 30 feet measured at the earth's surface, in a period of 427 days.

NON-EUCLIDIAN GEOMETRY.[1]

EVERY conclusion supposes premisses; these premisses themselves are either self-evident and have no need of demonstration, or can only be established by assuming other propositions; and as we cannot continue this process to infinity, every deductive science, and especially geometry, must rest on a certain number of axioms which cannot be demonstrated. All treatises on geometry therefore commence with the enunciation of these axioms. But a distinction must be made between them: some—such as this for example, "Two quantities that are equal to a third quantity are equal to one another"—are not geometrical propositions, but are analytical ones. I regard them as analytical *a priori* judgments, and as such I will not discuss them. But I must insist on other axioms which are special to geometry. Text-books for the most part state them very explicitly:—

(1) Only one straight line can be drawn between two points.

(2) A straight line is the shortest distance between two points.

(3) Only one straight line can be drawn through a point parallel to a given straight line.

Although the demonstration of the second of these axioms is generally dispensed with, it would be possible to deduce it from the other two, and from those, of which the number is more considerable, that we admit explicitly without stating them, as I shall explain in the sequel.

Efforts have also for a long time been made without success to demonstrate the third axiom, known under the name of the *postulatum d'Euclide*. The amount of trouble that has been taken in that chimerical hope is truly beyond imagination. Finally, at the commencement of the century, and almost simultaneously, Lowatchewski and Bolyai, two men of science, a Russian and Hungarian respectively, established, in an irrefutable manner, that such a demonstration was impossible; they have very nearly rid us of the inventors of geometries without postulates: since their time the Academy of Sciences only receives annually one or two new demonstrations.

The question was still not settled; soon a great step was made by the publication of the celebrated memoir of Riemann, entitled "Ueber die Hypothesen welche der Geometrie zum Grunde liegen." This small treatise has inspired the majority of recent works, of which I will make mention subsequently, and among which must be mentioned those of Beltrami and Helmholtz.

The Geometry of Lowatchewski.—If it were possible to deduce the *postulatum d'Euclide* from the other axioms, it would evidently happen that in denying the postulate and admitting the axioms, we should be led to contradictory results; it would then be impossible to base a coherent geometry on such premisses.

But this is precisely what Lowatchewski has done. He supposes in the first place that—

"*Several straight lines can be drawn through a point parallel to a given straight line.*"

And he moreover retains all the other axioms of Euclid. From these hypotheses he deduces a series of theorems among which it is impossible to detect any contradiction, and he constructs a geometry the faultless logic of which is not inferior to that of the Euclidian geometry.

The theorems are, certainly, very different from those to which we are accustomed, and they disconcert us a little at first.

Thus, the sum of the angles of a triangle is always less than two right angles; and the difference between this sum and two right angles is proportional to the surface of the triangle.

[1] Translation of an article that appeared in the *Revue Générale des Sciences*, No. 23, by M. H. Poincaré.

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It is impossible to construct a figure similar to a given figure, but of different dimensions.

If a circle be divided into n equal parts, and tangents be drawn to the points of division, these n tangents will meet and form a polygon, provided that the radius of the circle be small enough; but if this radius is sufficiently large, they will not meet. It is useless to multiply these examples; the propositions of Lowatchewski have no longer any connection with those of Euclid, but they are not less logically connected together.

The Geometry of Riemann.—Let us imagine a world peopled only with beings deprived of thickness; and let us suppose that these animals, "infinitely flat," are all in one plane, and are not able to get out of it. Let us admit, further, that this world is removed sufficiently from others to be free from their influence. As we are making these assumptions, we may as well endow these beings both with reasoning powers and the capacity of founding a geometry. In this case they would certainly attribute to space only two dimensions.

But let us suppose, however, that these imaginary animals, all still devoid of thickness, have the form of a portion of a spherical figure, and not of a plane one, and are all on one and the same sphere without being able to leave it. What geometry would they construct? It is clear at once that they would only attribute to space two dimensions: that which will play for them the part of the straight line will be the shortest distance between two points on the sphere—that is to say, an arc of a great circle; in a word, their geometry would be spherical geometry.

What they will call space will be this sphere which they cannot leave, and on which occur all the phenomena of which they can have any knowledge. Their space then will be *without limits*, since on a sphere one can always go forward, without ever coming to an end, and nevertheless it will be *finite*—one will never find the limit, but one can make the circuit of it.

In fact, the geometry of Riemann is spherical geometry extended to three dimensions. To construct it, the German mathematician had to throw overboard not only the postulates of Euclid, but even the first axiom: *Only one straight line can be drawn between two points.*

On a sphere only one great circle in general can be drawn through two given points (which, as we have just seen, would play the part of the straight line to our imaginary beings); but to this there is an exception; for, if the two given points are diametrically opposed, an infinite number of great circles can be made to pass through them.

In the same way, in the geometry of Riemann, only one straight line in general can be drawn between two points; but there are exceptional cases where an infinite number of straight lines can be drawn between them.

There is a kind of opposition between the geometry of Riemann and that of Lowatchewski.

Thus, the sum of the angles of a triangle is—

Equal to two right angles in Euclid's geometry.

Less than two right angles in that of Lowatchewski.

Greater than two right angles in that of Riemann.

The number of parallels that can be drawn to a given straight line through a given point is equal—

To one in the geometry of Euclid.

To zero in that of Riemann.

To infinity in that of Lowatchewski.

Let us add that the space of Riemann is finite although without limit, in the sense already given to these two words.

Surfaces of Constant Curvature.—There was, however, one possible objection. The theorems of Lowatchewski and of Riemann present no contradiction, but, however numerous the consequences which these two geometers have drawn from their hypotheses, they were compelled to stop before they had exhausted all of them, for the number would be infinite: who can say, therefore, that, if they had carried their deductions further, they would not finally have found such contradictions?

This difficulty does not exist for the geometry of Riemann, provided that it is limited to two dimensions; for, in fact, the geometry of Riemann for two dimensions does not differ, as we have seen, from spherical geometry, which is only a branch of ordinary geometry, and consequently outside all discussion.

M. Beltrami, in considering in the same way the two-dimensional geometry of Lowatchewski to be only a branch of ordinary geometry, has equally refuted the objection in this case.

This he has done this in the following manner:—Consider on a surface any figure. Imagine this figure, traced on a flexible and inextensible cloth, to be laid on this surface, in such a way that when the cloth is moved and changes its shape, the various lines of this figure can change form without altering their length. In general this flexible and inextensible figure cannot leave its place without quitting the surface; but there are certain particular surfaces for which a similar movement would be possible; these are the surfaces with constant curvature.

If we resume the comparison that we previously made, and imagine beings without thickness living on one of these surfaces, they will regard the movement of a figure all of whose lines preserve a constant length as possible. A like movement, on the other hand, would appear absurd to animals without thickness living on a surface whose curvature was variable.

These surfaces of constant curvature are of two kinds:—

Some are of *positive curvature*, and can be so deformed as to be laid on a sphere. The geometry of these surfaces becomes then spherical geometry, which is that of Riemann.

Others are of *negative curvature*. M. Beltrami has shown that the geometry of these surfaces is none other than that of Lowatchewski. The two-dimensional geometries of Riemann and Lowatchewski are thus found to be re-attached to Euclidian geometry.

Interpretation of Non-Euclidian Geometries.—Thus the objection disappears as regards geometries of two dimensions.

It would be easy to extend M. Beltrami's reasoning to geometries of three dimensions. The minds which space of four dimensions does not repel will see here no difficulty; but they are few. I prefer, then, to proceed otherwise.

Let us consider a particular plane that we will call fundamental, and construct a kind of dictionary, making a double series of words, written in the two columns, correspond each to each, in the same way that the words of two languages, having the same signification correspond in ordinary dictionaries:—

Space...	Portion of space situated above the fundamental plane.
Plane	Sphere cutting orthogonally the fundamental plane.
Right line... ...	Circle cutting orthogonally the fundamental plane.
Sphere	Sphere.
Circle	Circle.
Angle	Angle.
Distance between two points ...	Logarithm of the anharmonic ratio of these two points and the intersections of the fundamental plane with a circle passing through these two points and cutting it orthogonally.
&c.,	&c.

Let us take, then, the theorems of Lowatchewski, and translate them by means of this dictionary, as we should translate a German text with the aid of a German-French dictionary. *We shall obtain then the theorems of ordinary geometry.*

For example, this theorem of Lowatchewski—"The sum of the angles of a triangle is less than two right angles"—is translated thus: "If a curvilinear triangle has for its sides the arcs of a circle which if prolonged would cut orthogonally the fundamental plane, the sum of the angles of this curvilinear triangle will be less than two right angles." Thus, however far one pushes the results of the hypotheses of Lowatchewski, one will never be led to a contradiction. Indeed, if two of Lowatchewski's theorems were contradictory, the translations of these two theorems, made with the help of our dictionary, would also be contradictory; but these translations are theorems of ordinary geometry, and everyone agrees that ordinary geometry is free from contradictions. Whence comes this certainty, and is it justified? This is a question that I cannot treat here, but which is very interesting, and, as I believe, soluble. The objection that I have formulated above no longer then exists.

But this is not all. The geometry of Lowatchewski, susceptible of a concrete interpretation, ceases to be a frivolous logical exercise, and is capable of application: I have not the time to mention here either these applications or the use that M. Klein and myself had made of them for the integration of linear equations.

This interpretation, moreover, is not unique, and one could construct several dictionaries analogous to that given above, and by which we could by a simple "translation" transform the theorems of Lowatchewski into theorems of ordinary geometry

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Implicit Axioms.—Are then the axioms explicitly enunciated in treatises the only foundations of geometry? One can be assured to the contrary when one sees that, after having successively abandoned them, there still remain some propositions common to theorems of Euclid, Lowatchewski, and Riemann. These propositions ought to rest on some premisses, as geometers admit, although they do not state them. It is interesting to try to liberate them from classical demonstrations.

Stuart Mill has made the assertion that every definition contains an axiom, since, in defining it, the existence of the object defined is implicitly affirmed. This is going too far: it is seldom that one gives a definition in mathematics without following it by the demonstration of the existence of the object defined, and when it is omitted, it is generally because the reader can easily supply it. It must not be forgotten that the word existence has not the same sense when it is the question of a mathematical creation as when we have to do with a material object. A mathematical creation exists, provided that its definition involves no contradiction either in itself or with the properties previously admitted.

But if Stuart Mill's remark cannot be applied to all definitions, it is none the less true for some of them.

A plane is sometimes defined in the following manner:—The plane is a surface such that the straight line which joins any two points in it lies altogether in the surface.

This definition manifestly hides a new axiom: we could, it is true, alter it, and that would be better, but then it would be necessary to enunciate the axiom more explicitly.

Other definitions give place to reflections no less important.

Such is, for example, that of the equality of two figures: two figures are equal when they can be superposed; to superpose them it is necessary to displace one until it coincides with the other; but how must it be displaced? If we ask, we should be answered that it ought to be done without changing its shape, and in the manner of an invariable solid. The "reasoning in a circle" would then be evident.

In truth, this definition implies nothing. It would have no meaning for a being who lived in a world where there were only fluids. If it seems clear to us, it is that we are accustomed to the properties of natural solids that do not differ greatly from those of ideal solids whose dimensions are all invariable.

Meanwhile, however imperfect it may be, this definition implies an axiom.

The possibility of the movement of an invariable figure is not a truth evident by itself, or at least it is only one in the same way as the *postulatum d'Euclide*, and not as an analytical *a priori* judgment would be.

Moreover, in studying the definitions and the demonstrations of geometry, we see that one is obliged to admit, without demonstrating it, not only the possibility of this movement, but even some of its properties.

This results, first of all, from the definition of the straight line. Many defective definitions have been given, but the true one is that which is understood in all the demonstrations where the straight line is in question:

"It may happen that the movement of a constant figure is such that all points of a line belonging to this figure remain immovable while all the points situated outside this line are displaced. Such a line will be called a straight line."

We have in this enunciation purposely separated the definition from the axiom that it implies.

Several proofs, such as those relating to the equality of triangles which depend on the possibility of letting fall a perpendicular from a point on a line, assume propositions that are not enunciated, since we must admit that it is possible to carry a figure from one place to another in a certain manner.

The Fourth Geometry.—Among these implicit axioms, there is one which seems to me worth mentioning, not only because it has given rise to a recent discussion,[1] but because in abandoning it, one can construct a fourth geometry, as coherent as those of Euclid, Lowatchewski, and Riemann.

To demonstrate that we can always raise from a point, A, a perpendicular to a straight line, AB, a straight line, AC, is considered movable round the point A, and in the first instance coinciding with the fixed line AB; and it is made to turn round the point A until it lies in the prolongation of AB.

We thus assume two propositions: first, that such a rotation is possible, and then that it can be continued until the two lines are in one straight line.

If the first point be admitted, and the second rejected, we are led to a series of theorems still more curious than those of Lowatchewski and Riemann, but equally free from contradiction.

I will quote only one of them, and that not the most singular: *A true straight line can be perpendicular to itself.*

The Theorem of Lie.—The number of implicit axioms introduced in classical demonstrations is greater than it need be, and it would be interesting to reduce them to a minimum. We can ask ourselves, in the first place, if this reduction is possible, if the number of necessary axioms, and imaginable geometries is not infinite.

M. Sophus Lie's theorem dominates all this discussion: it can be thus stated:—

Let us suppose that the following premisses are admitted:—

(1) Space has n dimensions.

(2) The movement of an invariable figure is possible.

(3) To determine the position of this figure in space, p conditions are necessary.

The number of geometries compatible with these premisses will be limited.

I can even add that, if n be given, a higher limit to p can be assigned.

If, then, the possibility of movement be admitted, only a finite number (and that a restricted one) of geometries can be invented.

The Geometries of Riemann.—However, this result seems to be contradicted by Riemann, because this investigator constructed an infinite number of different geometries, and the one which generally bears his name is only a particular case.

Everything depends, he says, on the way in which we define the length of a curve. But there are an infinite number of ways of defining this length, and each of these can become the starting point of a new geometry.

That is perfectly true; but most of these definitions are incompatible with the movement of an invariable figure, which is supposed possible in Lie's theorem. These geometries of Riemann, so interesting on many grounds, can only then remain purely analytical, and do not lend themselves to demonstrations analogous to those of Euclid.

The Nature of Axioms.—Most mathematicians regard the geometry of Lowatchewski only as a simple logical curiosity; some of them, however, have gone further. Since several geometries are possible, is it certain that ours is the true one? Experience, doubtless, teaches us that the sum of the angles of a triangle is equal to two right angles; but this is only because we operate on too small triangles; the difference, according to Lowatchewski, is proportional to the surface of the triangle; will it not become sensible if we work with larger triangles, or if our means of measurement grow more accurate? Euclidian geometry would only then be a provisional geometry.

To discuss this question, we ought in the first instance to inquire into the nature of geometrical axioms.

Are they synthetical conclusions *a priori*, as Kant used to say?

They would appeal to us then with such force, that we could not conceive the contrary proposition, nor construct on it a theoretical edifice. There could not be a non-Euclidian geometry.

To convince oneself of it, let us take a true synthetical *a priori* conclusion; for example, the following:—

If an infinite series of positive whole numbers be taken, all different from each other, there will always be one number that is smaller than all the others.

Or this other, which is equivalent to it:—

If a theorem be true for the number 1, *and if it has been shown to be true for* $n + 1$, *provided that it is true for* n, *then it will be true for all positive whole numbers.*

Let us next try to free ourselves from this conclusion, and, denying these propositions, to invent a false arithmetic analogous to the non-Euclidian geometry. We find that we cannot; we shall be even tempted in the first instance to regard these conclusions as the results of analysis.

Moreover, let us resume our idea of the indefinitely thin animals: surely we can scarcely admit that these beings, if they have minds like ours, would adopt Euclidian geometry, which would be contrary to all their experience.

Ought we, then, to conclude that the axioms of geometry are

[1] See MM. Renouvier, Léchalas, Calinon, *Revue Philosophique*, June 1889; *Critique Philosophique*, September 30 and November 30, 1889; *Revue Philosophique*, 1890, p. 158. See especially the discussion on the "postulate of perpendicularity."

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experimental truths? But we do not experiment on straight lines or ideal circles; only material objects can be dealt with. On what would depend, then, the experiments which would serve to found a geometry? The answer is easy.

We have seen above that one argues constantly as if geometrical figures behaved like solids. That which geometry would borrow from experience is therefore the properties of these bodies.

But a difficulty exists, and it cannot be overcome. If geometry were an experimental science, it would not be an exact science—it would be liable to a continual revision. What do I say? It would from to-day be convicted of error, since we know that a rigorously invariable solid does not exist.

Geometrical axioms, therefore, are neither synthetic a priori conclusions nor experimental facts.

They are *conventions*: our choice, amongst all possible conventions, is *guided* by experimental facts; but it remains *free*, and is only limited by the necessity of avoiding all contradiction. It is thus that the postulates can remain rigorously true, even when the experimental laws which have determined their adoption are only approximate.

In other words, *axioms of geometry* (I do not speak of those of arithmetic) are only definitions in disguise.

This being so, what ought one to think of this question: Is the Euclidian geometry true?

The question is nonsense.

One might just as well ask whether the metric system is true and the old measures false; whether Cartesian co-ordinates are true and polar co-ordinates false; whether one geometry cannot be more true than another—it can only be more convenient.

Now, Euclidian geometry is, and will remain, the most convenient:—

(1) Because it is the simplest; and it is not so simply on account of our habits of thought, or any kind of direct intuition which we may have of Euclidian space; it is the most simple in itself in the same way as a polynomial of the first order is simpler than one of the second.

(2) Because it agrees sufficiently well with the properties of natural solids, those bodies which come nearer to our members and our eye, and with which we make our instruments of measurement.

Geometry and Astronomy.—The above question has also been stated in another way. If the geometry of Lowatchewski is true, the parallax of a very distant star would be finite; if that of Riemann be true, it would be negative. Here we have results which seem subject to experience, and it has been hoped that astronomical observations would have been able to decide between the three geometries.

But what one calls a straight line in astronomy is simply the trajectory of a ray of light. If then, as is impossible, we had discovered negative parallaxes, or shown that all parallaxes are greater up to a certain limit, we should have the choice between two conclusions:—

We could renounce Euclidian geometry, or modify the laws of optics, and admit that light is not propagated strictly in straight lines.

It is useless to add that everyone would regard the latter solution as the more advantageous.

Euclidian geometry, then, has nothing to fear from new experiments.

Let me be pardoned for stating a little paradox in conclusion:—

The beings which had minds like ours, and who had the same senses as we have, but who had not received any previous education, might receive conventionally from an exterior world choices of impressions such that they would be led to construct a geometry different from that of Euclid, and to localize the phenomena of this exterior world in a non-Euclidian space, or even in a space of four dimensions.

For us, whose education has been formed by our real world, if we were suddenly transported in this new one, we should not have any difficulty in referring the phenomena to our Euclidian space.

Anyone who should dedicate his life to it could, perhaps, eventually imagine the fourth dimension.

I fear that in the last few lines I have not been very clear. I can only be so by introducing new developments; but I have already been too long, and those whom these explanations might interest have read their Helmholtz.

Desiring to be brief, I have affirmed more than I have proved: the reader must pardon me for this. So much has been written on this subject, so many different opinions have been put forward, that the discussion of them would fill a volume.

W. J. L.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 11.—"The *Rôle* played by Sugar in the Animal Economy: Preliminary Note on the Behaviour of Sugar in Blood." By Vaughan Harley, M.D.

This communication was to show that the causes why the whole amount of added sugar can seldom be recovered from blood are threefold. Firstly, the imperfections in the as yet known methods of analysis. Secondly, the different ways in which the albumens of the blood behave themselves while coagulating; some coagulating in the form of firm clots, which retain the saccharine matter in their interstices, rendering it impossible to extract all the sugar from them by washing; others separating as loose, flocculent curds, from which the sugar can be regained with comparative facility. While, thirdly, as bacteria were distinctly ascertained to have nothing to do in the matter, and yet the loss of the sugar added to the blood is in every instance distinctly progressive—according to the period of time the sugar is left in contact with the blood before the analysis is begun—Dr. Vaughan Harley considered himself justified in saying that there must exist in the normal blood itself a sugar-transforming agent. This he described as an enzyme; but refrained from going into any further particulars regarding it until his researches upon the subject are more advanced.

He gave tables of the results of his experiments, and compared them with those recently published by Schenk, Rohmann, and Seegen; showing that while the percentages of the sugar regained by the first observer ranged from 20 to 55 per cent., and those recovered by the two last experimenters fluctuated between 80 and 96 per cent., in his three different series of experiments, where different methods of analysis were employed, the percentages of the added sugar regained ranged respectively between 85 and 100; 92·9 and 99·3; and 94·7 and 99·9 per cent.

Mathematical Society, February 11.—Prof. Greenhill, F.R.S., President, in the chair.—The following communications were made:—On the logical foundations of applied mathematical sciences, by Mr. Dixon. He maintained the importance of distinguishing in all sciences between what is dependent on verbal conventions and what is not. He thus distinguished between that part of the meaning of a term which is laid down as its definition, and the part which remains to be discovered as a consequence of the definition. So also sciences might be divided into purely symbolic sciences, which being based on definitions alone conveyed no real information; subjective sciences, which deal with concepts and objective sciences, which deal with actual things. He then stated the conditions under which a set of assertions might be arbitrarily laid down as the definition of a term; and applied these conditions to show that Newton's three laws of motion could be regarded as a definition of the term force, that if this was done there could no longer be any discussion as to whether or not force alone is sufficient to account for the movements of matter. The anomaly that we are apparently able to determine directions absolutely, though we can determine positions only relatively, was explained, and a formal proof of all the elementary theorems of mechanics, including the principle of virtual work, might be deduced.—Note on the inadmissibility of the usual reasoning by which it appears that the limiting value of the ratio of two infinite functions is the same as the ratio of their first derived, with instances in which the result obtained by it is erroneous, by Mr. Culverwell.—On Saint Venant's theory of the torsion of prisms, by Mr. A. B. Basset, F.R.S.

DUBLIN

Royal Society, January 20.—Prof. W. N. Hartley, F.R.S., in the chair.—Reports on the zoological collections made by Prof. Haddon in Torres Straits, 1888–89: the Hydrocorallinæ, by S. J. Hickson. The specimens described are a female stock of *Stylaster gracilis*, *Distichopora violacea*, and *Millepora Murrayi*. Some of the smaller colonies of *Distichopora* are bright orange in colour, others vandyke brown, and the larger ones are deep purple with pale yellowish tips. The author

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believes that the differences in colour mean a difference in age and sexual condition. The smallest colonies are not sexually mature, the brownish have male ampullæ, and the oldest stocks are violet in colour, and are apparently female. In any case, the colour of *Distichopora* can no longer be regarded as the principal character for specific definition.—Sir Howard Grubb, F.R.S., exhibited and described a 4-inch equatorially-mounted refracting telescope of novel construction, in which the right ascension and declination circles were situated at the eye end of the telescope itself, instead of, as usual, on the polar and declination axes; thereby rendering the working of the instrument most convenient for the observer. The circles at the eye end of the telescope are connected by gearing to the polar and declination axes; but Sir H. Grubb described the method by which the "loss of time," and other errors, consequent on gearing, were almost totally eliminated, and the readings rendered quite sufficiently accurate for all ordinary purposes to which such a telescope would be put.—The first part of a memoir on the fossil fishes of the Coal-measures of the British Islands, by Mr. James W. Davis, was communicated by the Honorary Secretaries of the Society.

PARIS.

Academy of Sciences, February 15.—M. d'Abbadie in the chair.—On a new method of organic analysis, by M. Berthelot. The method consists in heating the compound in oxygen under a pressure of 25 atmospheres in a calorimetric bomb. Combustion is total and instantaneous, and therefore differs from that which appertains to the use of copper oxide.—On the employment of compressed oxygen in the calorimetric bomb, by the same author.—Action of alkaline metals on boric acid: critical study of the processes used in the preparation of amorphous boron, by M. Henri Moissan. The general result of the investigation is that when an alkaline metal acts on boric acid the reaction that occurs is accompanied with considerable heat, and, on account of the elevation of temperature, the greatest part of the boron set free combines with the excess of alkali, and with parts of the metal vessel used for the experiment. When this is afterwards washed out with water and hydrochloric acid, a mixture of boron, boride of sodium, boride of iron, boron hydride, nitride of boron, and hydrated boric acid is obtained after desiccation. This mixture is said to have the same composition as the substance which has hitherto been regarded as amorphous boron. M. Moissan will describe a method of preparing amorphous boron in a future paper.—Experimental researches on the transmissibility of cancer, by M. Simon Duplay.—The temporary star in Auriga, by M. G. Rayet. On February 10 and 11 the star appeared to M. Rayet to be orange-yellow or pale yellow. Its spectrum was examined by means of the 14-inch equatorial fitted with a spectroscope. It appeared to be continuous, the red and violet portions being comparatively bright. Four bright lines were seen in the green, and their wave-lengths were determined as 518, 501, 493, and 487.—Extension of Lagrange's equations to the case of sliding friction, by M. Paul Appell.—On the distribution of prime numbers, by M. Phragmen.—On the measure of high temperatures; reply to M. H. Becquerel, by M. H. Le Chatelier.—Remarks on the surface tension of liquid metals; a reply to a note by M. Pellat, by M. Gouy.—Variation, with temperature, of the dielectric constant of liquids, by M. D. Negreano. Experiments on benzine, toluene, and xylene, between 5° and 45° C., indicate that the dielectric constant diminishes with increase of temperature.—On the influence exercised on electro-magnetic resonance by an unsymmetrical arrangement of the long circuit along which the waves are propagated, by MM. Blondlot and M. Dufour. The experiments show that the wave-length, measured by means of a resonator, is independent of the dissymmetry of the two wires which transmit the electro-magnetic undulations.—The propagation of electric waves studied by a telephonic method, by M. R. Colson.—Magnetic perturbation of February 13 and 14, by M. Moureaux. The disturbance was first indicated on the magnetograph of the Parc Saint-Maur Observatory at 5h. 42m. on the morning of the 13th inst. The declination and horizontal force curves suffered a simultaneous rise, while the vertical component decreased. The most important phase of the perturbation occurred between 11 p.m. and 2 a.m.; and about 5 p.m. of the 14th the elements had returned to their normal value. The disturbance in declination amounted to 1° 25′, and the horizontal and vertical components varied respectively more than $\frac{1}{37}$ and $\frac{1}{18}$ of their normal value.—Observations of atmospheric electricity, made by means of a captive balloon, by M. E. Semmola.—On the determination of the state of dissolved salts from a study of contraction, by M. Georges Charpy.—On some properties of bismuthic acid, by M. G. André.—On a carbide of barium, by M. Maquenne. (See Notes.)—Transformation of aromatic amines into chlorinated hydrocarbons, by MM. Prud'homme and C. Rabaut.—On the principles which accompany chlorophyll in leaves, by M. A. Etard.—Improvement of the culture of industrial and fodder potatoes in France: results of the season 1891, by M. Aimé Girard.—Contributions to the study of unplastered wines, by M. H. Quantin.—On the assimilation of carbohydrates, by M. Hanriot.—On the presence of numerous diatoms in the Cretaceous of the Paris basin, by M. Cayeux.—On the existence of zeolites in the calcareous Jurassic rocks of Ariège, and on the dissemination of these minerals in the Pyrenees, by M. A. Lacroix.

AMSTERDAM.

Royal Academy of Sciences, January 30.—Prof. van de Sande Bakhuyzen in the chair.—Prof. Pekelharing spoke of the composition of the fibrin ferment. When oxalated blood-plasma is diluted with water and treated with acetic acid till moderate acid reaction, the precipitate consists chiefly of a substance which is soluble in alkali, in an excess of acid, and in neutral salt-solutions, and from which, by the action of pepsin-hydrochloric acid, is split off a nuclein—a substance that thus must be considered as a nucleo-albumin. This nucleo-albumin acquires, combined with lime, all the properties of fibrin ferment. It is very probable that this nucleo-albumin issues from the corpuscles of the blood.—Prof. Max Weber gave some results of his investigations of the fresh-water fauna of the islands of Sumatra, Java, Flores, Celebes, and Saleyer. Among the Crustacea, the Entomostraca are not essentially different from European forms. Isopods are only represented by marine species: Ichthyoxenus, Tachæa, Rocinela, and Bopyridæ. Amphipods are extremely rare, and only Orchestia was found. Nearly 70 species of Decapods were collected, out of which 33 are living also in brackish and sea water. It could be proved that immigration out of the sea into the rivers had taken place. An account was given of the life-history of *Ichthyoxenus Tellinghausii*.

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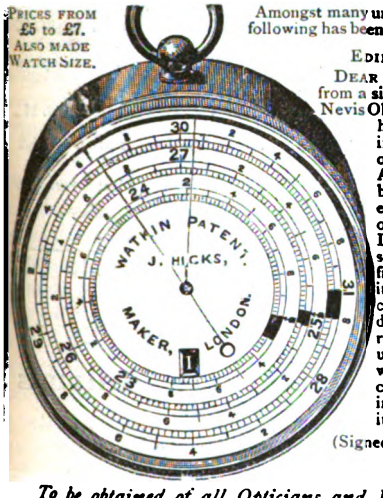

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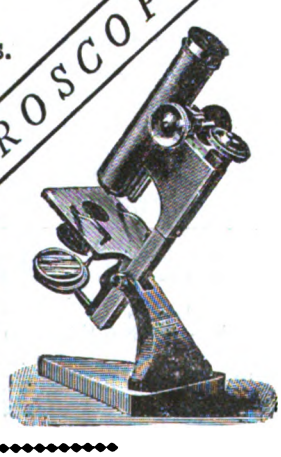

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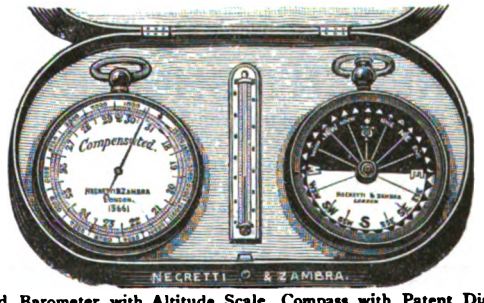

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THURSDAY, MARCH 17, 1892.

MAN IN NATURE.

L'Homme dans la Nature. Par Paul Topinard. (Paris: Baillière et Cie., 1891.)

IT is with much pleasure that we announce the appearance of a new work from the pen of Dr. Paul Topinard, formerly General Secretary of the Anthropological Society of Paris, and Professor in the École d'Anthropologie. Dr. Topinard is well known in this country as one of the most eminent, if not the most eminent, physical anthropologist in France at the present day, and it is with much regret that anthropologists here have observed the shameful way he has been treated by a faction of the Anthropological Society of Paris, who have done their best to diminish his usefulness and retard the advancement of true anthropological science in Paris. Notwithstanding the troubles and anxieties he has gone through—to which we would not have alluded here had they not been public property, and discussed in French scientific journals—it is gratifying to find that Dr. Topinard has pursued the even tenor of his way, and been able to enrich anthropological science by another of his valuable works, forming the seventy-third volume of the "International Scientific Series."

The work under review is divided into twenty-two chapters. The first contains, in addition to a statement of the scope of the book, a short but interesting history of the development of anthropological science. In the second chapter, its nature, its proper limits, and its relation to biology, ethnology, psychology, and sociology, are pointed out. The term "anthropology" is restricted to the study of man as an animal and a member of a group in the zoological series, in conformity with the acceptation in which it is used by Blumenbach, Broca, Quatrefages, and others. In the third chapter, the various subjects of study included in anthropology, as above restricted, are set forth, and the general principles of zoology on which the distribution of animals in groups of different values rests, the choice of characters on which they are founded, and the differences between race, species, family, and order are indicated.

The methods employed in anthropological research are considered in the fifth chapter. These may be briefly stated as descriptive and anthropometric. As descriptive terms are liable to vary very considerably according to the ideas of different observers, their value has hitherto been much less definite than characters based upon measurement. For some time past the author has been endeavouring to elaborate a system of observation which will render descriptive characters more uniform and trustworthy than heretofore. Having had some practical experience of the plan advocated by Dr. Topinard, we may state that we have been favourably impressed with it, not only for producing more uniform results, but also for saving labour to the observer, facilitating the analyses of observations, and for classifying the latter according to their type.

The character of the hair is dealt with in the sixth chapter. The hair is shown to furnish us with characters which are of importance not only in distinguishing races, but also in comparing man with the animals most nearly allied to him. The author shows that, in respect to their hair, the negro races differ most from the monkeys, while the white races most nearly resemble them, the yellow races being intermediate. In the straight-haired races the hair corresponding to the long coarse outer fur of mammals displaces the woolly hair—the homologue of the woolly under-fur; while in the woolly-haired races the reverse takes place. These characters of the hair would have formed an impassable gulf between the yellow and negro races had not some intermediate forms fortunately been left. The abundant generalization of fur as *lanugo* in the fœtus would go to prove that man descended from a furred progenitor.

The value of statistical maps is discussed in the seventh chapter, and as an example of their use in tracing race-characters, the distribution of the blonde and dark types in France has been taken and illustrated. It is shown, however, that the combination of the descriptive and anthropometric methods gives the most trustworthy results in determining the natural types of man. The latter method is discussed in chapter viii., and the use of indices, projections, seriations, graphic curves, averages, &c., are also explained.

The ninth chapter deals with measurements of the skeleton and the living body, and how these should be made by travellers and others. The directions given for the measurement of the long bones by ascertaining their maximum length is undoubtedly the best method. The proportions which these bones bear to the height of the skeleton (the latter being taken as 100) he gives as follows: humerus 20·0, radius 14·3, femur 27·3, tibia 22·1. To get the stature of the subject when alive, he adds 35 mm. to the height of the skeleton obtained from the detached bones by the above formula. The proportions just mentioned correspond very closely to those of the second series of French skeletons given in the "Éléments d'Anthropologie," and also with Rollet's important observations. Our own observations on subjects which have been carefully measured before dissection, go to support those of Rollet, and to show that the formulæ above given indicate the actual stature as nearly as possible, without adding 35 mm., as Dr. Topinard does. This is probably due to difference in mounting the skeletons—a fertile source of error. We are of the opinion that where it is not possible to get measurements of subjects before dissection in sufficient numbers to establish the formula for the race, it is more trustworthy to do so from the mean stature of the living, than from the height of articulated skeletons. The directions given for measuring the living body and tabulating its descriptive characters, are specially arranged for the purpose of encouraging travellers to undertake anthropometric observations. A model schedule of observations is given, arranged so that the various questions may be answered by a figure or a measurement, which should prove valuable to travellers. The system of measuring the body aimed at in it is to obtain the relative proportions of the several parts, rather than absolute anatomical dimensions; in other words, the author's idea is that it is better that travellers who are not anthropologists, and consequently not trained to anthropological research, should confine their attention to obtaining such measurements as will enable the canon

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of proportion of the several parts of the body to be made out, and compared in different races to that which obtains in Europeans, rather than attempt to do work which requires previous anatomical knowledge. The measurements are divided into two classes, according to their importance, but all of them are such as any intelligent traveller, with a little practice, should be able to make with accuracy.

Chapters x. and xi. are devoted to the study of the skull and the head of the living subject. A list of the chief measurements of the skull which the author recommends is given. These are to our mind much more satisfactory than any previous list he has produced, and will, we feel certain, be hailed with pleasure by anthropologists in this country, as being quite in accord with their ideas of what the essential measurements of the skull are. This list also agrees more nearly with the measurements used by our fellow-workers in Germany and Austria; indeed, we might even go so far as to say that when an international system of measuring the skull s arrived at (a time which we hope is not far distant), the difference between it and that now propounded by Dr. Topinard will be slight. The most important measurements of the living head, he considers, are those of the nose, and next to them the diameters of the head. As an example of what may be learned from a single craniometric character when systematically studied, the cephalic index is selected, and he alludes with satisfaction to the international agreement recently arrived at regarding the measurements from which it is obtained and its divisions, and shows that it is a character of the first importance for distinguishing types of races.

Having thus prepared the way, he next discusses the connection between man and the animals which approach nearest to him, the distance which separates them, and the relative place which man occupies amongst them. This naturally leads him to the consideration of the characters and descriptive morphology of the Primates. As the subject is a very wide one, he has restricted his investigation to a comparative study of the brain and the skeleton. Chapters xiii. and xiv. are occupied with the evolution of the brain in the vertebrate series, the form and volume of that organ, and the arrangement of its convolutions. The mechanism of the evolutionary transformation of the cranium of the animal into that of man is traced in the fifteenth chapter, and the craniometric characters connected with this transformation are dealt with in the succeeding chapter. The characters of the head, the vertebral column, the thorax and pelvis connected with the quadrupedal and bipedal attitude, are discussed in the seventeenth chapter, and those relating to the attitude of the body and the function of prehension in the eighteenth.

The nineteenth chapter is devoted to the zoometric characters related to the adaptations of the limbs for prehension and locomotion; the muscular and visceral characters connected with attitude are also discussed shortly. Other characters distinctive of man, the anthropoids, and the monkeys, in the vertebræ, the sacrum, mandible, teeth, liver, &c., are pointed out in the twentieth chapter. Retrogressive anomalies, or the accidental appearance in man and other animals of morphological arrangements foreign to the type, but resembling those which occur normally in some other type of animal; rudimentary organs, absolutely useless to man, but which are more or less developed in other animals; and progressive anomalies, are the subject of the twenty-first chapter.

The author concludes his task by devoting the last chapter to a *résumé* of the previous chapters. He classifies the Primates as follows:—

1st Sub-order—Man.

2nd Sub-order—The Monkeys
- 1st family, Anthropoids.
- 2nd ,, Pithecidæ.
- 3rd ,, Cebidæ.
- 4th ,, Arctopithecidæ.

3rd Sub-order—The Lemurs.

Supposing the distance between the Cebidæ and the Pithecidæ to be 1, that between the latter and the Anthropoids would also be 1, while the distance between the Anthropoids alone, or in conjunction with the other two families, on the one hand, and man on the other, would be represented by 3; the same figure would also represent the distance of the lemurs from the monkeys. We note that Dr. Topinard includes Galeopithecus among the lemurs, although this genus is now accepted by zoologists as belonging to the Insectivora, but this slight error is immaterial.

The important subject of the relationship and descent of man is dealt with more briefly than we could have wished. He shows that man cannot be descended from an Anthropoid, which is essentially a perfected and specialized monkey, and that we must look to a lower source for the origin of the human stem, to one where more generalized conditions obtained; indeed, to a type sufficiently far back in time and low down in the animal series as to be the progenitor of the monkeys and man. The beginning of the Miocene period is pointed out as a very remarkable one in the history of the world, during which many of the initial types of our existing genera were formed, and amongst others the first monkey succeeding the lemurs of the preceding Eocene period. It is during this epoch that we must seek for the stem proper of man, and that common to the monkeys, or to both of these sub-orders. According to Cope, man has descended directly from the lemurs, without passing by way of the monkeys and Anthropoids, the lemurs themselves coming through the Marsupials. Beyond this the genealogy of man is merged with that of the Mammalia, of which the first representatives existed as far back as the Trias.

Regarding the question of the unity or plurality of origin of man the author has not very much to say, and although he has reasoned on the assumption of the monophyletic hypothesis of the human species throughout the work—that is to say, that man has originated from a single stem—he considers that there is not sufficient evidence to show clearly one way or other whether this is the case, or that man has a double stem of origin developed during one epoch, or at two epochs separated from one another by a considerable interval of time. This question has yet to be definitely solved with respect to the monkeys, notwithstanding the fact that some American zoologists have shown that the monkeys of the New World have not the same origin as those of the Old World, which, if substantiated, would support the argument of a double cradle of origin for man—namely, according to the

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author, one common to Asia and America, the other, situated in some southern continent uniting Africa and Oceania, for the negro.

Whatever the origin of man may have been, the author shows that two periods must be recognized in his history, one before the acquisition of language, which relates to his precursor, and the other after that, during which man properly so-called was constituted.

The author sums up respecting man, the Anthropoids, and the monkeys, by comparing the order of the Primates to a tree. The lemurs are the roots, and give rise to one or several stems. One of these is the stem of the monkeys, a branch of which sends out a bough more elevated than the others—namely, that of the Anthropoids. Another, of which the point of origin or of contact with the former has yet to be discovered, gives off the human branch, which grows up parallel with that of the Anthropoids, but without relation to it, and shoots beyond it.

The book concludes with a few words on the question of whether or not man has attained his perfection. The author's idea is that the volume of the brain cannot increase much more, though it is possible for the anterior lobes to become larger. One thing he thinks is certain, that dolichocephaly will be replaced by universal brachycephaly. Although the brain has probably attained its limits in respect to size, the limits to which the quality of its cells may improve are, as far as can be seen, uncircumscribed, and in this direction man may yet hope to attain to still higher perfection by the development of his intellectual faculties.

From the critical sketch we have given, it will be seen that the book is an important addition to anthropological literature. Not only will it be useful to anthropologists, but also to general readers who desire to obtain an insight into anthropology and to follow what is being done in that science. As an exposition of the subject, we have no hesitation in stating that it is a work of much merit, and worthy of the high reputation of its author. There remains yet the duty to be performed by the publishers, of putting it within the reach of a much wider range of readers than it is accessible to in its present form, by the publication at an early date of the English edition. J. G. G.

FURNITURE WOODS.

The Art and Craft of Cabinet-making. By D. Denning. (London: Whittaker and Co., 1891.)

THIS neat little volume purports to be "a practical hand-book to the construction of cabinet furniture, the use of tools, formation of joints, hints on designing and setting out work, veneering, &c., together with a review of the development of furniture." It is well planned, and written in a pleasing and simple style, and appears admirably adapted for its purpose in general.

There is one drawback to this book, common to all works of its kind, and that is the meagre information given in the section dealing with the various kinds of woods employed. These woods are mahogany, cedar, oak, walnut, ash, rosewood, birch, beech, satinwood, pine, and a few others. The author argues that the amateur requires no information at all—or practically none—about these woods, but recommends him to trust a respectable dealer. We venture to remark that both the dealer and the amateur stand in need of, and would have been much interested in, a good description of these various woods.

It would at least have been worth mention that the word mahogany, like most trade names of the kind, may refer to very different woods: thus the Cuban or Spanish mahogany (*Swietenia Mahogani*, L.) is a very different wood from the mahogany of India (*Cedrela Toona*, Roxb.), which goes more commonly, perhaps, under the name of cedar (Moulmein cedar), another fallacious appellation, since it has nothing in common with the cedar of the botanist (*Cedrus*), or the pencil cedar of commerce, which is a *Juniper*, while it is closely allied to the "Cuban cedar," also known as Honduras and as Mexican cedar (*Cedrela odorata*, L.). The author is partially alive to this, as his remarks on p. 42 show; but we think he might have put the whole matter in a much clearer light by giving good descriptions of these very different woods. The African mahogany, from Sierra Leone, is a different wood again.

Under *oak*, the author, as elsewhere, begs the whole question by the remark, "Oak, like mahogany, is too well known to require any minute description." Possibly so—it all depends on the meaning of the word "minute"; but we think that such a work as this would be very much more useful if a description of the general distinctive characters of oak were given, and that the reader is entitled to expect such a description. He mentions that several kinds of oak are in the market, but this kind of thing only confuses, instead of helps, the reader.

Then, again, what does the author mean by "rosewood"? The rosewoods of India—*Dalbergia latifolia* (Roxb.) and *Pterocarpus indicus* (Willd.)—are by no means the only timbers which come into this country under that name, and the author might have done much more than merely remark that, "so far as mere appearance is concerned, there is not much difference between the various kinds."

We think that the author has missed an opportunity of compiling what is very much needed in this country, a concise and practical chapter on the distinctive characters of the various cabinet woods, on the lines, for instance, sketched in Marshall Ward's little manual on "Timbers, and some of their Diseases." The expectation that something of the kind might have been attempted is the fairer, since the author, in this very chapter on "furniture woods," goes out of his way to reinstruct amateurs in the use of the multiplication table and superficial measurements, which "may have been forgotten since their school days."

The statement on p. 57 that wood does not contract in length requires modification; and some of the remarks on warping and shrinking would be more intelligible to a reader who understood something of the structure of his woods.

The illustrations are numerous, and, on the whole, good and decidedly useful; and in spite of the omissions we point out, we think the book admirably adapted in many respects for the amateur's shelves.

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OUR BOOK SHELF.

L'Électricité dans la Nature. Par Georges Dary. "Bibliothèque Internationale de l'Électricité et de ses Applications." (Paris: Georges Carré, 1892.)

A POPULAR and accurate account of the various forms and ways in which electricity appears in Nature is sure to find a great many readers, for the subject is most interesting. Everyone should become acquainted with at any rate some of the most ordinary electrical phenomena of every-day life, even if he should learn no more than the cause of a flash of lightning and the subsequent peal of thunder. In this volume the author has brought together accounts of many interesting phenomena that have been observed from time to time, with the hypotheses that have been put forward to explain them. The subject is divided into seven parts or chapters. The first relates to the origin, presence, and distribution of electricity in our atmosphere, and also to cosmical electricity, in which the influence of solar spots on atmospheric electricity and the electrical nature of comets and nebulæ are mentioned. There are also descriptions of various conductors and electrometers, the principles of each being brought out clearly. Chapter ii. deals with storms. The author in this part has collected many typical examples which represent various classes of storms. After reference to the formation, height, and constitution of storm-clouds, and the variation and distribution of the storms themselves on the earth's surface, he describes the various forms in which lightning has been observed. Very curious effects, both on men and trees, are recorded to have taken place. An interesting instance here given relates to a flash of lightning that, having struck one tree and travelled down its trunk spiral fashion, suddenly leapt across to another one close by, and went to earth, the spiral curve being continued on this second tree. Chapter iii. contains some useful information relating to lightning-conductors, in which a brief historical summary is given: many kinds that have been or are now in use are described, with accounts of their action, verification, and efficiency. In the next two chapters, hail-storms, waterspouts, tornadoes, and cyclones are dealt with, while earthquakes and auroræ form the subjects of the concluding chapters.

In the above summary of the contents of this volume there is much to which we should have liked to refer, but the reader at any rate will be able to form a general idea of the range of subjects treated of in these four hundred pages.

Besides being interesting, the book will form a useful volume to many readers, for its value is very much increased by the great number of references inserted.

The First Book of Euclid's Elements. By the Rev. J. B. Lock, M.A. (London: Macmillan and Co., 1892.)

NOW that the concession has been made by the University of Cambridge of allowing in all her public examinations any sound proof of the propositions of Euclid provided that their logical sequence remains unaltered, teachers of geometry will have a far freer scope; since they are no longer bound by any hard and fast rule. The present work, by a writer familiar to our readers, will be read with interest, for the arrangement of the text has undergone somewhat of a change from the sequence usually followed. With regard to the order of the propositions, it will be noticed that the theorems are separated from the problems. This seems to be advantageous, for after all there is a fundamental difference between theorems and problems: as the author says, a theorem is a geometrical truth based on fundamental ideas and definitions of geometry, while problems entirely depend on postulates which are practically impossible.

The definitions have also received great attention, and are here thoroughly and clearly explained; in two cases, that of the "straight line" and "angle," the author has thought fit to make a slight divergence from the customary definitions. Accompanying the propositions are numerous exercises, while interpolated occasionally are many worked out examples.

Altogether, the book is one that should be in the hands of teachers, and is worthy of being well tested by them in their classes. W.

The Ilford Manual of Photography. By C. H. Bothamley. (London: The Britannia Works Co., Ltd., 1892.)

THIS manual, which has been compiled at the request of the Britannia Works Company, will be found by our photographic readers to be both well written and useful, containing as it does all information generally needed by amateurs. It is not a complete treatise on the subject, but is intended to aid those who are indulging in the various applications of this art at the present day from a thoroughly practical point of view. The first few chapters are devoted to the description of the apparatus, developing manipulations, faults in negatives, and various printing processes, all of which are well treated; we then come to the method of making enlargements, mounting and framing, lantern slides and transparencies. The concluding chapters are of special interest, consisting of portraiture, copying, photographing of objects in motion, orthochromatic photography, and photography by artificial light.

Preceding the appendix are tables of English and French weights and measures, while the appendix itself contains some formulæ and reprints from one or two photographic journals. Besides an account of the Ilford universal hydroquinone developer, there are papers by Mr. John Howson, on the "Printing Paper of the Future," "Lantern Plates," together with the best methods of cutting up printing-out paper.

A most useful table presents a list of dealers and dark rooms situated all over the world, ranging from modest dealers in Bettws-y-Coed and Leighton-Buzzard to those in South Africa, New Zealand, and Japan. From this table, it can be at once ascertained whether, at a certain place, Ilford plates or paper, chemicals or apparatus, are kept in stock; whether amateurs can receive help or get work done for them, such as printing, mounting, &c.; or whether a dark room is obtainable or not. This list, when thoroughly completed, and other first-class firms included, will be invaluable for tourists on the Continent, while at present it should be used very largely by those travelling in this country. W.

The Advanced Class-Book of Modern Geography. By William Hughes and J. Francon Williams. (London: George Philip and Son, 1892.)

IT is impossible not to have some pity for the unfortunate boys and girls who will have to learn geography from this gigantic "class-book." It consists of more than 800 closely-printed pages, the very appearance of which would suffice to discourage most young students. Geography is one of those subjects in the learning of which very much more depends on the teacher than on the text-book; and a good teacher would have no desire to see so elaborate a work as this in the hands even of "advanced" pupils. The information, so far as we have been able to examine it, is accurate; but it is not, as a rule, presented in a way that would be likely to excite interest or curiosity.

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LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Carpenter on Eozoon.

THE scientific world is deeply indebted to Dr. Dallinger for his excellent new edition of Carpenter's invaluable work on the microscope, and among other things for his retaining unchanged the description of *Eozoon canadense*, as a monument of an important research up to a certain date.

Dr. Carpenter devoted much time to the study of Eozoon, and brought to bear on it his great experience of foraminiferal forms, and his wonderful powers of manipulating and unravelling difficult structures. After having spent years in studying microscopic slices of Eozoon and the limestones in which it occurs, I have ever felt new astonishment when I saw the manner in which, by various processes of slicing and etching, and by dexterous management of light, he could bring out the structure of specimens often very imperfect. Not long before Dr. Carpenter's death I had an opportunity to appreciate this in spending a few days with him in studying his more recently acquired specimens, some of them from my own collections, and discussing the new points which they exhibited, and which unhappily he did not live to publish. Some of these new facts, in so far as they related to specimens in our cabinet here, have since that time been noticed in my *résumé* of the question in the Memoirs of the Peter Redpath Museum, 1888; but I hope my friend Prof. Rupert Jones may yet be able to complete Dr. Carpenter's work.

Those who know Dr. Carpenter's powers of investigation will not be surprised that later observers, without his previous preparation and rare insight, and often with only few and imperfect specimens, should have failed to appreciate his results. One is rather surprised that some of them have ventured to state with so great confidence their own negative conclusions in a matter of so much difficulty, and requiring so much knowledge of organic structures in various states of mineralization. For myself, after working for fifty years at the microscopic examination of fossils and organic rocks, I feel more strongly than ever the uncertainties and liabilities to error which beset such inquiries.

As an illustration in the case of Eozoon: since the publication of my memoir of 1888, which I had intended to be final and exhaustive as to the main points, and in so far as I am concerned, I have had occasion to have prepared and to examine about 200 slices of Eozoon from new material: and while most of these have either failed to show the minute structures or have presented nothing new, a few have exhibited certain parts in altogether unexpected perfection, and have shown a prevalence of injection of the canal system by dolomite not previously suspected. Since that publication also, the discoveries of Mr. Matthew in the Laurentian of New Brunswick, and the further study of the singular Cambrian forms of the type of Cryptozoum, have opened up new fields of inquiry.

I think it proper to state, in reference to Dr. Dallinger's footnote on the recent paper of Mr. Gregory, that it must not be inferred from it that Mr. Gregory had access to my specimens from Madoc and Tudor, though he no doubt had excellent material from the collections of the Canadian Geological Survey. It might also be inferred from this note that I have regarded the Madoc and Tudor specimens as "Lower Laurentian." The fact is that I was originally induced in 1865, by the belief of Sir W. E. Logan at that time that these rocks were representatives in a less altered state of the middle part of the Laurentian, to spend some time at Madoc and its vicinity in searching for fossils, but discovered only worm-burrows, spicules, and fragments of Eozoon, which were noticed in the Journals of the Geological Society for 1866. (The more complete specimen from Tudor was found by Vennor in 1866.) On that occasion I satisfied myself fully that the beds are much older than the Cambro-Silurian strata resting on them unconformably; but I felt disposed to regard them as more probably of the age of some parts of the Huronian of Georgian Bay, which I had explored with a similar purpose under Logan's guidance in 1856. As my work was not official, and was palæontological rather than stratigraphical, it did not seem proper to express any dissent from what were at the time the probable conclusions of stratigraphical work; but I was quite prepared to assent to the new views afterwards adopted.

In conclusion, the new material bearing on Eozoon is accumulating so rapidly that I cannot hope to be able to master it in detail, but shall be glad to aid others who may have more time; but I hope to be able, in a work now in preparation, at least to present the facts up to date in a popular form.

J. WILLIAM DAWSON.

McGill College, Montreal, February 3.

The Samoan Hurricane.

REPLYING to the communication in NATURE of December 17, 1891 (p. 161), signed "H. F. B.," relative to my preliminary report on the Samoan hurricane of March 1889 (published in the Proceedings, U.S. Naval Institute, vol. xvii., No. 2, and in the *American Meteorological Journal*, July 1891), I would submit the following statement.

First of all, I wish to acknowledge Mr. Blanford's appreciation of the difficulties involved in the consideration of the subject, owing to the meagreness of the data; and at the same time to express my own appreciation of the fact that he himself has not had access even to such data as we have succeeded in collecting, but only to my necessarily brief discussion thereof, and the conclusions that I have drawn therefrom.

Mr. Blanford's explanation, as I understand it, is as follows: the vortex of the hurricane formed north or north-east (on the equatorial side) of Apia on the afternoon of the 15th, within a "region of disturbance" that had already caused stormy weather throughout the Samoan Islands and a decided barometric depression at Apia. The first effect of this formation was (by adding slightly to the normal evening rise of the barometer) to cause a decided rise of pressure, which, however, decreased again as the vortex slowly approached the harbour, thus causing the second minimum (the afternoon of the 16th), the duration of the storm being explained by the usual slow motion of the newly formed hurricane.

To the above explanation it is necessary to make a correction, I think, owing to the fact that the shifts of wind at the time of and immediately after the first minimum show that the centre of the disturbance then passed to the westward of Apia, and as the wind thereafter remained from north to north-east, the centre (or vortex) evidently remained to the southward and westward. This fact, however, merely introduces a change in the position where the new vortex formed, according to the theory under discussion.

Revising Mr. Blanford's explanation, then, in the light of this correction, it appears that the track of the *depression* is about as I have drawn it, but that a *vortex* formed slightly to the *southward* and *westward* of Apia, thus causing a slight rise of pressure at first, succeeded by a second fall, and the slow motion of this newly formed vortex caused the duration of the northerly gale.

Now, I must here take exception to one of Mr. Blanford's statements, which is as follows (referring to the theories given in my paper): "None of these explanations seem to take account of the circumstances that attend the formation of tropical cyclones, which, as we have elsewhere pointed out, differ in many respects from the storms of the temperate zone." A reference to my paper will show, I think, that I took into consideration the special peculiarity to which Mr. Blanford calls attention, and went so far as to insert a plate in order to illustrate two types of storms—namely, (1) the characteristic tropical hurricane type (where there is a decided vortex, or "centre of aspiration"); and (2) the type where there is a comparatively wide central region surrounded by an annular space where there are steep barometric gradients and correspondingly high wind velocities, but without any decided vortex, properly so called. I said also that "it will be seen that the indications are that the Samoan hurricane (on the 15th and 16th, at least) was of the second type, although during the 17th and 18th it doubtless became more like the first." In a word, I said (both explicitly and by means of the varying strength of the track drawn on the chart) that the depression passed Apia on the afternoon of the 15th, recurved (increasing in intensity and delaying whilst recurving, each of which is to be expected), and then moved off to the southward and eastward. I do not intend to convey the impression that I made any definite statement as to just when or where the vortex formed, nor am I wholly prepared to hazard such a statement even at

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this late date. I can still only repeat here what I said before, as follows:—

"My idea is, briefly, that the first depression occurred as the storm passed on its westward track, followed by the usual shift of wind to the northward. Along this branch of its trajectory its severity was probably not quite so great as it was later, and the force of its southerly winds was masked by the mountains on the island of Upolu; possibly careful observations of the rapidity of motion and the character of the clouds, or of the state of the sea off the harbour, might have indicated a severe storm, but this does not appear from the evidence at hand, though well worth considering. During its recurve the hurricane probably increased in intensity, the barometric depression at the centre deepening, and thus causing the second depression observed at Apia, which was slightly deeper than the first, although the centre itself was really at a greater distance than on the previous day."

Unfortunately, as stated above, we have no very definite data regarding the severity of the storm before it reached Apia, although I must say that a barometric depression of 0·76 inch below the normal in the tropics is very suggestive of the presence of a fully developed vortex, and it seems more than likely that if the harbour had not been well sheltered to the southward the southerly gales would have been quite as severe as those from the north. The only early data that we have, other than the reports from Apia, are contained in the following very brief report from the American schooner *Equator*, Captain Reid:—

"March 14 (Samoan date), lat. 12° 00′ S., long. 170° 50′ W., wind S., S.W., W., N.W.; thick, squally; hove to.
"March 15 (Samoan date), lat. 13° 00′ S., long. 170° 40′ W., wind N.W.; fierce gale; squalls; heavy sea."

This, although brief, seems to indicate that it was a fully developed hurricane that was approaching Apia, and I am inclined to the opinion that such was actually the case.

Mr. Blanford will surely admit that it is one recognized peculiarity of tropical hurricanes to reduce their speed of translation whilst they are recurving, and he will thus admit also that such recurvature accounts, in the present case, for the duration of the northerly gale at Apia. So far as the sharpness of recurvature is considered, I would say that the report of the *Equator* is sufficiently vague to allow us to draw a curve with a somewhat less sharp recurvature, and I should myself have done so but for the single fact that a hurricane approaching from a more northerly direction must have sent a northerly or north-easterly swell into the harbour that would have been felt to at least such an extent as to be noted in the log of one or more of the vessels there, but I have looked in vain for any such remark, until after the shift of wind on the afternoon of the 15th.

In conclusion, I take pleasure in expressing my obligation to Mr. Blanford for his interesting and able discussion of the subject, which, even with all the data at hand, has still many perplexing features. The principal object of this preliminary publication was to elicit comment and discussion, and it will be very gratifying if other authorities will give us the benefit of their experience and suggestions.

EVERETT HAYDEN.

Hydrographic Office, Washington, D.C.,
January 4.

THE distinction to which allusion was made in the passage quoted by Mr. Hayden is not, as he seems to have understood, that between tropical and extra-tropical storms when fully formed, but between the circumstances of their respective formation. These are that, in the former, the cyclonic circulation of the winds is preceded by much irregular action, which sometimes extends over a considerable area. Within this area there are local squalls and shifts of wind, with heavy rainfall, but the action is not for some time definitely concentrated and cyclonic. This preliminary stage does not appear to obtain in the storms of the temperate zone, where the deviating effect of the earth's rotation is so much greater than in low latitudes, and indeed, if we accept the views of Werner Siemens and Prof. Hann, the cyclonic circulation is the cause and forerunner of the storm. I cannot think it probable that a vortex, once fully formed, and travelling towards higher latitudes, should recurve so sharply as to produce a fall of the barometer on two successive days (with a rise in the interval) at the same place, simply by twice passing in its vicinity. To effect this, the recurvature must, as I apprehend, describe an angle considerably less than a right angle, and of such I know of no example among tropical cyclones. At the same time, my own view was put forward merely as a suggestion, and in no dogmatic spirit.

H. F. B.

Phoronomy.

ABOUT thirty-five years ago, I had a conversation with the late Dr. Donaldson, a well-known Greek scholar of the time, in which we discussed the appropriateness of the use of the word kinematics, in the sense in which it was then, and is now, employed by writers on mathematical science. Dr. Donaldson's opinion was that it is not the best word which can be employed to represent the science of pure motion, without regard to causation. He said that the word *κινέω* involved the idea of the cause of motion, and therefore that it ought not to be used when the idea of causation is to be completely set aside. He further gave it as his opinion that the word *φορέω* is more nearly expressive of the idea of mere going, without any reference to the cause of motion, and therefore that the proper word would be phoronomy, or phoretics.

I was so much impressed by this conversation that, for many years, I headed with the word phoronomy the papers of questions on the subject of pure motion which I was in the habit of preparing for College lectures and private pupils.

I have recently consulted a very eminent Greek scholar, and his opinion is that, on the whole, the word phoronomy is more distinctly expressive of the science of pure motion than the word kinematics. He agreed with Dr. Donaldson that the word kinematics suggests some idea of causation, whereas no such idea is suggested by the word phoronomy.

As a matter of history, the word *cinématique* was introduced by Ampère to represent the purely geometrical science of motion in the abstract, and was anglicized into kinematics by, I think, the late Dr. Whewell.

Sir W. Thomson and Prof. Tait, in the preface to their "Natural Philosophy," adopt the suggestion of Ampère, and employ the word in the same sense. They also employ the word *dynamics* in its true sense as the science which treats of the action of *force*, whether it maintains relative rest, or produces acceleration of relative motion. They further state that these two corresponding divisions of dynamics are conveniently entitled *statics* and *kinetics*.

Here, then, we have two words, kinematics and kinetics, both derived from the same root-word, employed to represent two entirely different sets of ideas; and there is not the same broad line of demarcation between the words themselves as there is between the sets of ideas which they are intended to connote.

Hence it appears to me that the word phoronomy, the law of going, is the most suitable, as it is the most expressive word, to represent the science of pure motion in the abstract.

At the time of my conversation with Dr. Donaldson, we were neither of us aware that the word had been already invented and employed. Some years after, I found that a treatise was published at Amsterdam in 1716, entitled "Phoronomia, sive de viribus et motibus corporum. Autore Jacobo Hermanno, Basil." Hermann, however, uses the word in much the same sense as we now use the word dynamics.

The point has been recalled to my mind by the discovery that the word has been employed in Germany in the sense in which Dr. Donaldson advocated that it should be used.

In the treatise entitled "Allgemeine Mechanik der Punkte und Starren Systeme," by E. Budde, published in 1890 at Berlin, the word phoronomy is adopted, and the author gives his reasons in the following words:—

"Man kann eine Ortsveränderung zunächst rein geometrisch, ohne alle Rücksicht auf ihre Ursachen, betrachten, und das soll in den nächsten Capiteln geschehen. Die Disciplin, welche sich mit dieser Betrachtung befasst, heisst *Phoronomie* oder *Kinematik*. Der Name Kinematic ist seit Resal der gebräuchlichere gewesen; neuerdings aber wird von Reulaux und seinen Schülern die Morphologie der Verknüpfung von Maschinentheilen als 'Kinematik' bezeichnet. Wir wählen deshalb hier den Namen Phoronomie."

With reference to this statement of Budde's, I observe that Ampère, in the "Essaie sur la Philosophie des Sciences," particularly mentions trains of machinery, such, for instance as the works of a watch, as coming under the heading kinematics.

I also find that Grassmann, in the "Ausdehnungslehre von

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1844," published at Leipzig in 1878, speaks of "Phorometrie" as representing "die reine Bewegungslehre"; and I see that Möbius uses the adjective in an article on the "Phoronomische Deutung des Taylor'schen Theorems."

A change in scientific nomenclature is by no means an unprecedented occurrence.

For instance, notwithstanding the great authority of Lagrange, the phrase "virtual velocity" has been practically superseded by the phrase "virtual work," and in the year 1876 the word "work" was substituted for "virtual velocities" in the regulations, published in the Cambridge University Calendar, for the Mathematical Tripos.

Another instance is the fact that the phrase *vis viva* has been superseded by *kinetic energy*, as a more convenient term in the expression of the principle of energy.

Further, I notice that Prof. Tait, in lectures on "Recent Advances in Physical Science," gives the suggestion that the time-honoured word force is in all probability destined, as science advances, to be relegated to the limbo of departed nomenclature.

For these various reasons, then, I trust that I shall not be regarded as an iconoclast, if I venture to substitute, for the word *kinematics*, the word *phoronomy*.

W. H. BESANT.

St. John's College, Cambridge, February.

On the Terms "Centrifugal Force" and "Force of Inertia."

THE retention, in the last edition of Mr. Loney's "Elements of Dynamics," 1891, of a paragraph (p. 141) which resuscitates the objections formerly urged by some writers against the use of the term "centrifugal force" seems to call for a protest. It is to be regretted that students of dynamics should find absolutely contradictory statements presented to them respecting the validity of this term. While, however, in one set of text-books we find a perfectly clear definition and consistent use of the phrase "centrifugal force," there does not, on the other hand, appear to be unanimity of ideas amongst the objectors, nor always sufficient clearness in expressing the same.

In the uniform circular motion of a ball rolling on a table against the inner surface of a vertical cylinder, the pressure of the cylinder upon the ball is a centripetal force directed towards the centre of the circle. The contrary pressure of the ball upon the cylinder is the "centrifugal force," which is defined as the reaction to the centripetal in this case, and in every case as the reaction to the normal component of the centrifugal force.

The foregoing definition or usage of the term is adopted without hesitation or apology in the following works, named in order of date:—

Poisson's "Traité de Mécanique," 1833, vol. i. p. 332, or Harte's translation, 1842, p. 256.

Walton's "Mechanical Problems," 1842, pp. 240, 260, 269.

Prof. Niven in "Cambridge Senate-house Problems," 1877, p. 78.

Thomson and Tait's "Natural Philosophy," 1879, p. 221.

Garnett's "Elementary Dynamics," 1875, p. 205, and 1882, p. 255.

Routh's "Rigid Dynamics," Part I., 1882, p. 217, and Part II., 1884, p. 15.

Williamson and Tarleton's "Dynamics," 1889, p. 88.

Objections to the term appear in—

Goodwin's "Course of Mathematics," 1849, p. 275.

Parkinson's "Mechanics," 1863, p. 249.

Blaikie's "Dynamics," 1887, p. 32.

Rankine, "Encyclopædia Britannica," "Mechanics," 9th edition.

Loney's "Statics and Dynamics," 1891, p. 141.

Other authors might have been cited, but I have referred to such as I happen to possess.

Nearly all these objectors evince the same reluctance to giving the name of "force" to the reactionary effect of the body's inertia in the direction of the normal outwards. Yet, if we admit that "to every force there is an equal and opposite reaction," it is not easy to escape from the conclusion that such a reactionary force exists.

Mr. Loney, however, postulates both forces, but adds:—"Centrifugal force is a very misleading term. It seems to imply that the force belongs to the mass instead of being an external force acting on the mass. A somewhat less misleading term is centripetal force. We shall avoid the use of either expression; the student who meets with them will understand that either (*sic*) means the force which must act on a mass to give it the acceleration normal to the curve in which it moves."

These are confusing directions to the student, who must be left in complete bewilderment as to any distinction in meaning between "centripetal" and "centrifugal." "Centrifugal," from its derivation, signifies that the force has a tendency to make the body fly away radially from the centre. And such a tendency there is, and such a motion would result if we could make the centrifugal force last after the centripetal has ceased. But in the objections taken the word "tendency" is regarded as though it implied an actual subsequent motion in the direction of the tendency. A beginner is almost certain to fall into the error of imagining that, when the cord is slipped, the stone from a sling will dart away in a direction intermediate between that of the string and its own previous motion in the circle. But the name "centrifugal" is not answerable for this. The idea is due to the unmistakable pull upon his hand of an outward tending force, to which "centrifugal" merely gives the right name. Clearer conceptions show him that the two forces, the action and the reaction, cease at the same instant when the string is cut, and that there is no initial velocity in either direction.

Uniform circular motion is perfectly unique. In the direction of the force there is no motion, in the direction of the motion there is no force. The real *crux* lies in this conception of a constant acceleration with a perpetual zero velocity in the direction of the acceleration. How, says one, can there be a rate of change when the change itself is zero? But the objection is a metaphysical one, and it may be urged with equal force against the whole doctrine of limiting ratios.

Mr. Loney's statement that the centrifugal reaction is not a force belonging to the mass, but "an external force acting on the mass," requires some elucidation. Dr. Parkinson, in the paragraph referred to, has something similar. He says that the term "centrifugal force" "vaguely conveys an impression, as it were, that the particle of itself resisted curvilinear motion and exerted a force *per se* to move in a rectilinear path, which innate tendency was only overcome by the action of some external force." He also grudgingly recommends the student to use the obnoxious phrase "simply as an equivalent for the moving force in the direction of the normal." Here again "centrifugal" is made to signify a tendency towards the centre! Is not the vagueness complained of imported into the subject in some measure by the writers themselves?

Whatever names are employed, the facts are these. The force towards the centre communicates to the body an acceleration in that direction, which acceleration gives rise (we know not how; but we say by the law of inertia) to a force equal and opposite to the force which produced the acceleration. This reaction always appears to emanate from the mass of the moving body, and it has therefore been called "the force of inertia" of the body. Although this view has been combated by Poisson and others, some of the latest authorities are reasserting it. Thus in Thomson and Tait, 1867 and 1879, we find in Article 216: "Matter has an innate power of resisting external influences. . . . This the inertia of matter, &c." Again, in Sir Robert Ball's "Experimental Mechanics," 1888, p. 252: "When any agent acts to set a body in motion or to modify its motion in any way, the body reacts on the agent, and this force has been called the kinetic reaction."

I cannot see any objection to designating this reaction "the force of inertia." It is a provisional term, which will serve our purpose until the nature of force is better understood. Poisson's argument against it, derived from our experience of friction, appears to me invalid, and his illustration irrelevant, because the law of resistance is not the same as in the case of inertia. If it had happened that the law governing friction was that the resistance to motion was directly proportional to the acceleration, then if a body were moving with constant velocity upon a rough plane there would be no resistance from friction. The smallest acceleration of velocity would give rise to a correspondingly small amount of friction, a double acceleration would double the resistance from friction, and so on, precisely as with the resistance from inertia.

GEORGE S. CARR.

A Lecture Experiment in Surface Tension.

HOPING it may be of interest to some of your readers, I venture to send you the following description of a simple ex-

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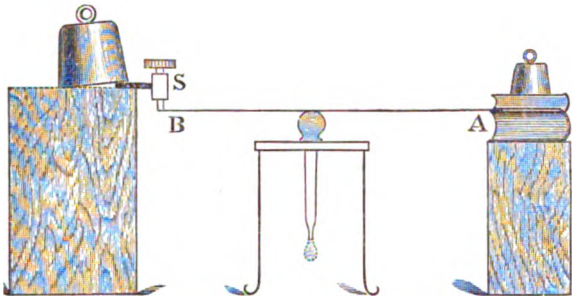
periment on surface tension. A drop of water hanging from the end of a vertical glass tube is regulated in size until it is just on the point of falling away from the tube. On dipping the end of a penholder in ether, and bringing the wet end within a few millimetres from the drop, the latter promptly falls. The drops may be produced and adjusted in the following manner:—

A piece of glass tube, about 8 cms. long and ·8 cms. in diameter, is made into a pipette with an orifice about ·15 cms. diameter, and fitted with a small india-rubber ball at the other end. The tube is then passed through a hole in a piece of wood large enough to rest on the top of a common tripod stand, as shown in the figure. A strip of wood, or a paper-



knife, about 30 cms. long, is placed with its centre resting on the top of the ball, one end (A) being held between the two uppermost of a pile of books, and the other end (B) passing under a screw (S). The ball is squeezed, a beaker of water brought under the end of the pipette, and the tube allowed to fill by the expansion of the rubber. By working the screw S, a drop is formed at the end of the tube, and since one complete turn of the screw I use only lowers the end of the rod (B) $\frac{1}{28}$ inch, it is possible to adjust the drop with great nicety. As the ether is brought up, the absorption of its vapour diminishes the surface tension over a small area of the drop of water, and currents, made visible by suspended dust, *appear* to pass from the interior towards the weakened spot. Bringing the ether still nearer, the drop often becomes much agitated, and finally, when the distance is reduced to about 4 mm., it falls away from the tube.

E. D. FRIDLANDER.

Mason College, Birmingham, February 29.

The Orientation of Ancient Monuments.

THE deeply interesting results obtained by Mr. Norman Lockyer with regard to the orientation of Egyptian temples, and by Mr. F. C. Penrose with regard to the Greek, tempt me to call attention to an extract from the *Century Magazine*, May 1883, from an article by Frank Cushing, describing a visit to the Zuñis, a typical tribe of the Pueblo Indians of New Mexico, in 1879:—

"Each morning just at dawn, the Sun priest, followed by the master priest of the Bow, went along the eastern trail to the ruined city of Ma-tsa-ki by the river side, where, awaited at a distance by his companion, he slowly approached a square open tower, and seated himself just inside upon a rude ancient stone chair, and before a pillar sculptured with the face of the sun—the sacred hand—the morning star and the new moon. There he awaited, with prayer and sacred song, the rising of the sun. Not many such pilgrimages are made ere the 'suns look at each other,' and the shadows of the solar monolith, the monument of Thunder Mountain, and the pillar of the Gardens of Zuñi lie along the same trail; then the priest blesses, thanks, and exhorts his father, while the warrior guardian responds as he cuts the last notch in his pine-wood calendar, and both hasten back to call from the house-tops the glad tidings of the return of spring. Nor may the Sun priest err in his watch of Time's flight; for many are the houses in Zuñi with scores on their walls or ancient plates embedded therein, while opposite a convenient window or small port-hole lets in the light of the rising sun, which shines but two mornings of the 365 on the same place. Wonderfully reliable and ingenious are these rude systems of orientation, by which the religion, the labours, and even the pastimes of the Zuñis are regulated."

In like manner, we read in Prescott that at Quito the festival of the Sun-god was held when "he sat upon the pillar," *i.e.* a pillar cast no shadow.

Might not this Zuñi ceremonial be a description of one enacted at some primitive astronomical temple such as Stonehenge? and do we not gain the best insight into the minds of those who in the Archaic period first orientated—the Babylonian and Assyrian ziggurats, the pyramids and temples of Egypt, the temples of Greece, and our own Stonehenge—by seeing how a people in a similar stage of intellectual and scientific development are acting to-day? FRED. F. GRENSTED.

Merchant Taylors' School, Crosby.

The Nickel Heat Engine.

MR. KARAMATE, in a letter which appears in your issue of March 3 (p. 416), alludes to a new form of heat engine described by me in your issue of January 28 (p. 294). He states that my device is similar to one by Prof. Stefan. I wish to point out that the two heat engines are quite different in design. The engine of Prof. Stefan has a step by step action; that described by me is under certain conditions absolutely continuous in its action. The contrast is shown by Mr. Karamate's description of Prof. Stefan's engine; he writes:—"Nickel plates were fixed on a wheel, like that of a water-mill, and a magnet was placed before it. By heating a nickel plate before the magnet, it was repulsed by the magnet, and a succeeding plate was attracted, so that the wheel commenced to rotate." From this it seems clear that the action of the machine must be step by step, since the different pieces of nickel must come successively under the influence of the magnet and the flame. In my disk form of engine, the action, when the disk has a *certain thickness*, is continuous. Mr. Croft has recently shown that when the disk is not so thick as mine was the disk starts in one direction, then stops, and sets off in the opposite direction. It will also be noticed that the distribution of the magnetic field due to two poles is entirely different from that in the machine of Prof. Stefan. Mr. Karamate writes: "By heating a nickel plate before the magnet, it was repulsed by the magnet." I hardly see how this *repulsion* takes place. Faraday, writing about the behaviour of nickel, states ("Experimental Researches," vol. iii., 2346): "Upon being heated the nickel soon became indifferent to ordinary magnets, but, however high the temperature, still it pointed to and was attracted by the electro-magnet." Surely the action of the engine is due to one piece of nickel becoming partly non-magnetic owing to a rise of temperature, thus upsetting equilibrium, and allowing the next piece of nickel to approach the magnet and consequently the flame.

I may add that E. Berliner in 1885, and Edison in 1887, patented magnetic heat machines on the step by step principle.

FREDERICK J. SMITH.

Trinity College, Oxford, March 4.

The Limpet's Power of Adhesion.

I WOULD like to call the attention of your readers to the results of some experiments, which, I think, are original, referring to the power of adhesion of the common limpet (*Patella vulgata*).

The experiments were carried out as follows, with the assistance of my friend J. Sinel, of the Biological Laboratory here. The shell of the limpet *in situ* was perforated, so as to allow of the attachment of a delicate spring balance, by means of which a gradually increasing strain could be put upon the animal, in a direction normal to the foot. The pull was increased until the animal became detached, and the final weight and the greatest and least diameters of both foot and shell noted.

The following figures are compiled from a series of twenty consecutive experiments, and are typical of other similar series. The individuals operated on were not chosen in any way.

Averages of Twenty Consecutive Experiments.

Area of shell, in square inches	1·07	
Pressure per square inch of shell	22·5	pounds.
Area of foot, in square inches	·45	
Pressure per square inch of foot...	54·3	pounds.
Gross weight	23·9	,,
Maximum pressure per square inch of shell area	31·3	,,
Minimum ,, ,, ,, ,,	13·9	,,
Maximum ,, ,, ,, foot area	71·1	,,
Minimum ,, ,, ,, ,,	37·1	,,
Maximum gross weight	32	,,
Minimum ,, ,, ...	12	,,

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The areas in the above table are calculated on the assumption that both shell and foot are ellipses, which is fairly near the truth. I need hardly point out that the old theory of atmospheric pressure will not account for these figures, and in the case of *Patella* the mucus is too thin to have much cohesion. Perhaps some of your readers can throw light on the subject.

29 St. James's Street, Jersey. PERCY A. AUBIN.

On the Variation of Latitude.

IN connection with the interesting investigations of Dr. Chandler on the variation of latitude, permit me to suggest the desirability of calling attention to Maxwell's remarks on the same subject at the conclusion of his paper "On a Dynamical Top," Ed. Phil. Trans., xxi., Part iv. p. 568 (read April 1857). Maxwell, taking the value of $\frac{C}{A}$ as 1·00309, as deduced from the amount of precession (Pontécoulant, ii. 268), finds the time of revolution of the pole of the earth about the true pole to be about 325·6 solar days. Dr. Chandler's period of 427 days would on the same principles correspond to the value $\frac{C}{A} = 1{\cdot}00235$.

ROBT. B. HAYWARD.

Fairlight, Harrow, February 28.

ORNITHOLOGY OF THE SANDWICH ISLES.

THE departure for Honolulu of Mr. Robert C. L. Perkins (as already announced in these pages), the gentleman sent out by the Joint Committee appointed by the Royal Society and by the British Association to investigate the zoology of the Sandwich Islands, may render the present moment opportune for noticing what has already been done towards obtaining a knowledge of their presumably expiring Fauna, though I can only lay claim to acquaintance with a single Class of it—the Birds. Yet students of that important branch of Biology which is known by the clumsy and not strictly accurate title of the "Geographical Distribution of Animals," will bear in mind that the first successful attempt to grapple with and elucidate it was based upon this very Class; and, furthermore, that by far the most comprehensive work on the subject has proceeded from the pen of a most proficient ornithologist, while nobody can doubt that but for his intimate knowledge of Ornithology many of his results would have been inconclusive if not unattainable. Into the reason why the most vagrant Class in creation was thus so serviceable to Mr. Sclater and to Mr. Wallace, there is no need here to enter. It is perhaps enough to state that indubitable fact to warrant the publication of the present remarks having reference to a small portion only of the Hawaiian Fauna; and, if they should furnish an indication of what may be proved when the rest comes to be better known, it will so much the more redound to the credit of Ornithology; while, should further acquaintance with other Classes contradict the inferences to be drawn from the Birds, the suspicion, to call it by no stronger name, at times expressed, that what is a good Law of Nature for one set of animals will not hold for another, may be justified.

Moreover, within the last six months has been published a very remarkable dissertation, "On the Structure of certain Hawaiian Birds, with reference to their Systematic Position," contributed by Dr. Gadow to Mr. Scott B. Wilson's beautifully illustrated "Birds of the Sandwich Islands." From its title, this essay might be supposed to interest only the taxonomer or the ornithotomist; and the zoologist of wider views might leave it unheeded as having a scope too limited for his purposes. The very contrary is the truth, and those who will follow the author's deductions to their logical end will perceive that his "Remarks" disclose a state of things which is not only subversive of the generally-received opinion as to the nature and affinities of the avifauna of the Sandwich Islands, but is fraught with evidence of a kind hardly hitherto suspected in regard to the origin and derivation of the animal population of that group. Hints to this effect may, indeed, be gathered from Mr. Wallace's works, and especially from his "Island Life"; and that, with the few facts at his disposal, he was able to give them, is proof of the depth of his perception; but henceforth he will be able to speak boldly, and drop every uncertain phrase.

The dearth of facts with which Mr. Wallace had to contend, even in 1880, shows that the Sandwich Isles have not been fortunate in their Natural Historians, though perhaps no worse off in this respect than many another group "lying in dark purple spheres of sea." Discovered in 1778 by Cook, during the last of his celebrated voyages, his ships communicated with one of the more western islands—Atooi, as its name sounded to him and his companions, but since, and doubtless more correctly, written, Kaui. The admiration of the visitors was excited by the cloaks and helmets of the natives, beautifully bedecked with feathers, the more or less moth-eaten remains of which may yet be seen in many a Museum; and the scarlet birds which furnished the most brilliant adornment of these ingenious works of art were duly mentioned by Cook in his journal as published. After less than a fortnight's stay, in the course of which the existence of five islands was made out, his ships stood off to the northward to prosecute their voyage of discovery. Towards the end of the year they returned, and Cook, having had experience of the hospitable treatment of the islanders, designed to make his winter-quarters in the Sandwich Isles, as he had named them, after the then First Lord of the Admiralty; but, keeping more to windward, the first land he made was the most eastern of the group, one that he had not even seen on his first visit. This was the historic Owhyhee—nowadays written Hawaii—which, being the largest of them, and that which produced the warrior-king and statesman who eventually subdued all the rest, has given its official name to the Archipelago.

Though Owhyhee was sighted on November 29, Cook's course along its eastern and southern coast was so deliberate that it was not until January 17, 1779, that he found a safe anchorage, and that in Kealeakakua Bay, on its western side. What passed there during the next three weeks need not be here recorded, but those who know how to read his narrative and the accounts since divulged from native sources will admit that it throws an important and yet most lurid light on the history of superstition. To the unprejudiced it must be doubtful whether even now the whole truth is or ever can be known. The ships sailed on February 4; but in making her way to the northward, the *Resolution* sprung her mainmast, and within a week returned to her old anchorage. Three days later occurred the terrible tragedy which deprived the world of one of its greatest seamen. A week after Cook's death, the ships sailed to the westward, touching at some of the intermediate islands—Mowee (Mauai), Lanai, and Morotai (Molokai), making once more for Atooi (Kauai) and Oneehow (Niihau), the last famous for its yams. Then, on March 15, they bore away again to the northward, and did not return.

Now, the object of giving here these details is to show that the natural history specimens obtained by Cook's ships were procured only on the islands of Hawaii, Kauai, and Niihau. This is the more needful because the first descriptions of any of the birds of the Sandwich Isles were given, with two exceptions, by Latham in his "General Synopsis of Birds," published in 1781–85, and most of the specimens so described no longer exist. Some were in the British Museum or the collection of Sir Joseph Banks, afterwards transferred thereto; the rest were in the Leverian Museum. Of the former, as is

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well known, not one remains; but fortunately, at the breaking up of the last in 1806, a few were bought by the then Lord Stanley, who (dying in 1851, as thirteenth Earl of Derby, and President of the Zoological Society) bequeathed his collection to the borough of Liverpool, and there, thanks to the care that has been taken of them, they still exist in fair condition. A few more were bought for the private collection of the then Emperor of Austria, and are still carefully preserved in the Museum of Vienna. Of several of the species it is not known that any other specimens were brought to Europe until some three years ago. On both of Cook's previous voyages qualified naturalists had been sent; but, as is known, the arrangements for publishing their discoveries were so imperfect that little credit followed to anyone concerned. On this, his third and last voyage, there was no expert, though Mr. William Ellis, who in an irregularly published narrative, calls himself "Assistant Surgeon to both vessels," was somewhat of a draughtsman, and made a series of sketches, which, becoming the property of Banks, subsequently passed to the British Museum. The commoner species of Sandwich-Island birds are generally recognizable, but others are so unhappily limned that even the word caricature (which always implies some likeness) seems too strong to apply to them. Nevertheless, Mr. G. R. Gray adventured to determine all of them.

More than a quarter of a century passed before any further progress was made in the knowledge of the zoology of the Sandwich Isles, though they were visited by numerous ships, and in 1794 were ceded to Britain under Vancouver. In 1814 an attempt was made to seize them for Russia; and Kotzebue, whose voyage has so much scientific interest, was there in 1816–17, but the accomplished naturalists, Chamisso and Eschscholtz, who were with him, took little heed of the fauna of the islands.[1] The year 1822 saw the arrival of the more celebrated William Ellis, whose missionary labours throughout the Pacific and in Madagascar are so widely known. The Sandwich Isles had by that time fallen under the sway of the conquering Kamehameha I., whose son and successor, desirous of seeing European civilization, arrived in England in 1824 with his wife—both to die of measles within a few weeks. The British Government determined to send their remains for interment in Honolulu, by that time become the capital of the islands, and accordingly H.M.S. *Blonde*, commanded by George Anson seventh Lord Byron (first cousin and successor to the poet), was commissioned to convey the dismal freight. The duty was performed, and the islands again were ceded to the British Crown, but again declined. On board the *Blonde* sailed as chaplain Mr. Rowland Bloxam, together with his brother Andrew, who was somewhat of a naturalist, and it was intended that the published account of her voyage should contain a proper appendix on the natural history of the islands. An "Appendix" there indeed is, but one utterly unworthy of its reputed author, for the book was edited by a lady (as I have been informed) who had nothing but a few of his notes to guide her, and though assisted, as it is stated, by "the gentlemen connected with that department in the British Museum," is a disgrace to all concerned, since, so far from advancing the knowledge of the subject, it introduced so much confusion as to mislead many subsequent writers.[2] Some years later another great opportunity was missed, and this time by the American traveller Townsend, who, after crossing the Rocky Mountains to the Columbia River, sailed, in company with Nuttall, the well-known naturalist, for the Sandwich Islands, where they arrived in January 1836, and stayed nearly three months, visiting Oahu and Kauai. Returning at the end of the year, Townsend found the Prussian naturalist Deppe at Honolulu, and with him passed some time in the pursuit of natural history, visiting most of the windward islands before he left in March 1837. Among the specimens obtained by Deppe for the Berlin Museum were some of two species for which Lichtenstein rightly established a new genus—the singular form *Hemignathus*—and, as it has since proved, both these species were new, though he had, not unnaturally, identified one of them with a species described by Latham. Of Townsend's collection, a considerable part was given to the Academy of Natural Sciences at Philadelphia,[1] where it still remains; but he sent several specimens to Audubon, at that time, I believe, in Edinburgh, and he parted with them to Carfrae, a dealer there, who sold them to the late Sir William Jardine, at the dispersal of whose collection I was so fortunate as to secure them—some of them bearing Townsend's label—for the Museum of this University. If Townsend had but published a list of his captures, he would indeed have rendered a very good service; but of course the value of island-forms, to say nothing of the fact that many of them were threatened with extirpation by colonization and civilization, had not then been appreciated, if even entertained, by naturalists. In the year of Townsend's departure, the French frigate *Vénus*, in the course of her troublous career under Du Petit-Thouars, arrived in the Sandwich Islands, with two naturalists, Léclancher and Néboux, on board; and some years later the atlas of plates illustrating the zoology of her voyage appeared; but the text was deferred for a long while, and, indeed, was not completed till 1856. Herein was figured and described, though not for the first time, a third species of the curious *Hemignathus*. In the meanwhile the celebrated expedition of Commodore Wilkes took place, and he, with some of his ships, wintered there. In the course of their six months' stay, the naturalists attached, Pickering and Peale, seem to have made large collections; but nearly all was lost in the shipwreck of the *Peacock*, one of the ships of the squadron. By 1848, Peale had completed his report on the specimens of Mammals and Birds collected, and it was printed off. A few copies only had been distributed, when the rest were destroyed by fire. It was by no means a bad performance; and I cannot understand why the late Mr. Cassin made so many changes in it when he, ten years later, brought out a new edition of it. Some of them (I speak only of those relating to the Sandwich Island fauna) were certainly not improvements. However, a distinctly forward step was made by the Peale-Cassin labours, and since few can obtain access to the original work, I may mention that Dr. Hartlaub considerately published an abstract of it,[2] just as two years later he did[3] of the French "Voyage au Pôle Sud," wherein, having sorted out the different species observed by various voyagers on the several Pacific groups, he gave a useful list of those found on each, and thus he assigned to the Sandwich Isles *thirty* species of birds, marking two of them as doubtful. One of them is now known to be rightly included, but the other must be struck out, as well as, for one reason or another, four more—leaving a total of *twenty-five*, only *sixteen* of which are Land-birds and only *fourteen Passeres*.

Hitherto, no list of the birds of the Sandwich Isles had been published, so that Dr. Hartlaub's met a great want, though it had, of course, been possible, since 1814, for anyone to pick out for himself the species assigned to that group from the general list compiled by Tiedemann

[1] The same negative results attended his second visit in 1824-25.

[2] I have reason to believe that Mr. Bloxam's original notes are still in existence, though hitherto they have not been accessible to me. It is possible that they would remove uncertainty on several points.

[1] In mentioning these facts, I desire to record my deep gratitude to the authorities of both these Museums—Berlin and Philadelphia—for their obliging readiness in allowing me to have these valuable specimens, one of them unique, for examination.

[2] *Archiv für Naturgeschichte*, 1852, Heft. i. pp. 93–138.

[3] *Journal für Ornithologie*, 1854, pp. 160–171.

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("Anatomie und Naturgeschichte der Vögel," ii. pp. 426-436), and, in like manner, since 1859, from Mr. G. R. Gray's useful "Catalogue of the Birds of the Tropical Islands of the Pacific Ocean," printed by order of the Trustees of the British Museum, but the former was obsolete, and the latter, as we now know, very erroneous.[1] Mr. Gray's references show him to have been as usual a model of accuracy, but his judgment as an ornithologist was frequently at fault. It was, therefore, with great pleasure that, some time in the winter of 1870-71, I received a copy of a "Synopsis of the Birds hitherto described from the Hawaiian Islands," which had been communicated in February 1869 to the Boston Society of Natural History, by Mr. Dole, a resident in those islands, and had been published in the Society's Proceedings (xii. pp. 294-309); and Mr. Sclater, who I knew had long taken an interest in the ornithology of the group, lost no time in noticing this very important publication (*Ibis*, 1871, pp. 356-362), adding thereto some valuable observations.[2] This list has naturally proved a serviceable foundation for future work. Forty-eight species were included, the author stating that this number "probably comprises but little more than half the avifauna of the group." That the list should be free from error was not to be expected, and a revised version of it, published in the "Hawaiian Almanack and Annual for 1879" (pp. 41-58), corrected some of the mistakes; but it was an honest piece of work, doing credit to its compiler. In the meanwhile, however, the historic voyage of H.M.S. *Challenger* had commenced, and one of the places at which she was to call was the Sandwich Islands. Of course the main object of her voyage was the exploration of the depths of the sea. Nevertheless, the terrestrial zoology of the countries visited, though forming a very subordinate part of the original plan, was not to be wholly neglected—nor was it in this case, for, during the three weeks she stayed in Hawaiian waters (July 27 to August 19, 1875), her officers availed themselves to some extent of the opportunity of studying the ornithology of the islands, though it does not appear that they had received any special instruction in regard to our imperfect knowledge of it. Here, then, was another great chance lost; for had those who drew up the directions for the scientific members of the Expedition taken the trouble to acquaint themselves with the particular points on which investigation was needed, so as to indicate the lines on which further research was desirable, no doubt some one of the *Challenger's* staff would have supplied, even in the short time of her stay, some of the missing facts, or at least would have thrown some light on the subject. As it was, the collection was reported as "small" (24 bird-skins and no specimen in spirit), and "containing nothing absolutely new except a single species of *Anas*," afterwards named *A. wyvilliana*[3] (Proc. Zool. Soc., 1878, p. 350). Something more, however, may yet be expected. The late Prof. Moseley, in his "Notes of a Naturalist on the *Challenger*," states (p. 514) that the last excursion on shore of his colleague, Von Willemös Suhm, was at Hilo in Hawaii, with a native guide, "in pursuit of the interesting endemic birds," and that "almost the last notes he wrote were some on the Sandwich Islands, relating especially to the birds." These notes do not seem to have been placed at the disposal of the ornithologists who described the specimens obtained by the *Challenger*; but they can hardly have been destroyed.

Having myself felt a good deal of interest in the avifauna of the Sandwich Isles—which, like that of many other islands throughout the world, was, as I had learnt, threatened with extirpation, chiefly in consequence of the destruction of the forests—I could not fail to be disappointed at the meagre results obtained by our people on this celebrated cruise, when it would have been so easy for them to have done better had their attention been duly called, and I cast about in several directions to find some suitable person to visit the islands with the view of investigating their ornithology in a thorough way. My young friend Mr. Scott Barchard Wilson (son of the well-known Mr. George Wilson, F.R.S.)—of whose taste for natural history I was well assured by his residence in my own College, by his journey to Portugal with Dr. Gadow, and by his subsequent sojourn in Switzerland (*Ibis*, 1887, pp. 130-150)—willingly took up the enterprise, and left Liverpool on February 24, 1887, for Honolulu, where he arrived on April 8, having on his way paid a visit to Washington to confer with Dr. Stejneger, whose name had already appeared in connexion with the birds of the Sandwich Isles. Mr. Wilson stayed in the islands until towards the close of the following year. He brought back such a collection as had never before been made there; but, rich as it was in some respects, defects became apparent as it was gradually worked out, and some of these defects are so grave that, until they are remedied, no complete list of the avifauna can be formed. I am deeply sorry that he has not been able to return; for, with his knowledge of what is wanted, it would be more easy for him to fill up the *lacunæ* than anyone else; and I long hoped that he would pay a second visit with this object. However, he has done a great deal more than anybody before him: he has ascertained the precise localities of nearly all the birds hitherto known, and added to them not inconsiderably—*fourteen* new species or local forms of *Passeres*, *two* of which required generic acknowledgment—all, it needs not to say, being peculiar to the islands, and mostly to one particular island only. It can scarcely fail to be interesting that the distribution in the group of the different genera and local forms of *Passeres* should be shown, and this is best done by the accompanying table.[1]

But Mr. Wilson was not content, as so many collectors in foreign countries are, with preserving only the skins of the birds he procured. He was careful to obtain specimens in spirit of all the important existing types; and these, having been properly subjected to examination by Dr. Gadow, have led to some remarkable results—the most remarkable that have been as yet made known in regard to the birds of the Sandwich Islands, and perhaps the most remarkable of those published during the past year in regard to Ornithology at large. They are contained in the dissertation I have already mentioned as being contributed by Dr. Gadow to Mr. Wilson's work. Most of the land-birds of the Sandwich Islands had been at one time thought to belong to the *Meliphagidæ*, or Honey-suckers—a Family very characteristic of the Australian Region, and known to be very polymorphic. It was thought to be still more so; and the surmise had been acted upon, so that some Finch looking birds, *Psittacirostra* and *Loxioides* had been supposed to be Honey-suckers in disguise, and

[1] Many of its worst errors are doubtless due to the loss, before mentioned, of the type specimens, which had been suffered by the Museum long before Mr. G. R. Gray was connected with it. Latham, in 1821, had already lamented their decay. It is almost needless to add that such a loss is not now, nor for many years has been, possible.

[2] Mr. Sclater was pleased to remark that this "memoir" had "escaped" my notice as editor of *The Ibis*. Herein he was in error. It certainly did not come to my knowledge while I was discharging that duty, and I doubt whether any copy reached England until after I had laid down my office.

[3] I do not venture to doubt the distinctness of this species, which had before been mentioned as *A. boscas*, var.; but its describer might have shown more clearly wherein it differs from the well-known American *A. obscura*, which seems its nearest ally.

[1] I have no desire to overlook the services of Mr. Valdemar Knudsen, of Kauai, who sent thence to the United States National Museum several collections, the most important of which was described by Dr. Stejneger in the Proceedings of that institution for 1887 (pp. 75-102), the year of Mr. Wilson's arrival in the islands. The Doctor's paper is of the exhaustive character to which one is accustomed in all his productions, and has been of considerable use in working out Mr. Wilson's collections, while these have enabled the latter to correct several mistakes—under the circumstances quite pardonable—made by the former, who subsequently described in the same Proceedings (xii. pp. 377-386) another collection from the same quarter.

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classed accordingly. Dr. Gadow has shown that this supposition is wholly erroneous, and that these last, together with another form, *Chloridops*—one of Mr. Wilson's discoveries—are true *Fringillidæ;* while out of the whole Hawaiian avifauna, only two genera can be referred to the *Meliphagidæ*—namely, *Acrulocercus* (*Moho* of some writers) and *Chætoptila*, the last being presumably

Table showing the Distribution of Birds of the Order PASSERES *in the Sandwich Islands.*

	Niihau.	Kauai.	Oahu.	Molokai.	Lanai.	Kaho'olawe.	Maui.	Hawaii.
CORVIDÆ.								
Corvus tropicus								*
FRINGILLIDÆ—								
Psittacirostra psittacea		*		*	*			*
Loxioides bailleui								*
Chloridops kona								*
"*Fringilla*" *anna*								?
DREPANIDIDÆ—								
Loxops coccinea								*
,, *flammea*				*				
Chrysomitridops cæruleirostris ...		*						
Oreomyza bairdi		*						
Himatione stejnegeri		*						
,, *parva*		*						
,, *chloris*			*					
,, *maculata*			*					
,, *chloridoides*					*			
,, *montana*					*			
,, *kalaana*				*				
,, *virens*								*
,, *mana*								*
,, *sanguinea*	*	*	*	*	*		*	*
,, *dolii*							*	
Vestiaria coccinea	*	*	*	*	*		*	*
Drepanis pacifica								†
Hemignathus procerus		*						
,, *lichtensteini*			*					
,, *obscurus*								*
,, *hanapepe*		*						
,, *lucidus*			*					
,, *olivaceus*								*
MELIPHAGIDÆ—								
Acrulocercus braccatus		*						
,, *apicalis*			?					
,, *nobilis*							*	*
Chætoptila angustipluma								†
TURDIDÆ (?)—								
Phæornis myiadestina		*						
,, *lanaiensis*				?	*			
,, *obscura*								*
MUSCICAPIDÆ—								
Chasiempis (quædem species non determinatæ)	*	*	*	*	*		*	*

All the species above-named are peculiar to the group, *i.e.* not found elsewhere. A * indicates that the species inhabits the island whose name heads the column. A † shows that the species is believed to be extinct.

extinct. All the other forms which had been accounted Meliphagine, present a peculiar structure of tongue, forbidding that alliance, or any affinity to the *Prionopidæ*, *Dicæidæ*, or *Nectariniidæ*, but revealing a distinct relationship to the *Cærebidæ*—now known as a Family characteristic of the Neotropical Region! Hereby a beam of light is thrown on the origin and derivation of the ornithic population of the Sandwich Islands. The distinct inference is that the first stock of their existing avifauna was received from America, in days when the range of the *Cærebidæ* extended further to the northward than it does at present, and that certain cognates or ancestors of the present *Cærebidæ* colonized the islands, there differentiating into the modern *Drepanididæ*. The importance of this inference on views which are held as to the Geographical Distribution of Birds in North America is a subject into which there is no need here to enter, for that would be a subject foreign to my present remarks; but I doubt not it will receive due attention from American ornithologists, whom it most nearly concerns. That these colonists from what I would venture to term a "Columbian" fauna—since it cannot be literally called a Neotropical one, and is certainly not "Nearctic"—were the earliest settlers which have left descendants one can hardly doubt, for they have existed in the Sandwich Islands long enough to undergo a great amount of change. Subsequently there has been a small infusion of blood from the "Australian Region." I say subsequently, because Dr. Gadow has shown that this immigration has undergone comparatively little modification. We have (or had) the two Meliphagine genera, *Acrulocercus* and *Chætoptila*—the latter, indeed, beyond anatomical examination, but showing no very great external deviation from well-known Australian types; while the former undoubtedly retains the normal Meliphagine tongue. To these may be added *Chasiempis*, a well-marked genus; but, without question, very nearly allied to the genus *Rhipidura*, so widely spread over the Australian Region, and found also in New Zealand. Thus three genera constitute, so far as I am able to see, the "Australian" element in the avifauna of the Sandwich Islands—and what are they among so many others?[1] More recently than this Australian infusion, has supervened an influx of Holarctic types, and especially of the *Fringillidæ*. Whether these have arrived from America or Asia, I do not pretend to say; but the long chain of islets running to the westward—one of which produces a remarkable form (*Telespiza cantans*), the knowledge of which we also owe to Mr. Wilson (*Ibis*, 1890, pp. 339–341, Plate ix.)—suggests the possibility of an Asiatic origin, a possibility confirmed by the consideration that his fine *Chloridops kona* may be the magnified descendant of the long-known *Chloris kawarahiba*, which has already an enterprising relative, *C. kittlitzi* (*Ibis*, 1890, p. 101), established in the Bonin Islands. Still later must have been the appearance on the scene of members of the genera *Corvus* and *Buteo*, both of which are, so far as is yet known, confined to Hawaii, the most eastern of the islands, and therefore suggest an emigration from the Nearctic area. These have been settled long enough to assume recognizable specific characters; but an apparently more modern colonist exists in *Asio accipitrinus*, the common Short-eared Owl of Asia, Europe, and North America, which extends its range over many islands in the Pacific Ocean, so far at least as the Galapagos, and has found a permanent home in the Sandwich Isles, breeding there, as it would seem, regularly—as it once did in England, and would again, if permitted by the gamekeepers. More than this, there is an indication that the tendency to colonization from the Holarctic region still continues. Within an hour or two of his leaving the islands, there was sent to Mr. Wilson a freshly-killed example of *Circus hudsonius*—the American Hen-Harrier—a species which he had already ascertained to have before occurred in the group; but, not being recognized by Judge Dole, it had been endowed with a new name, and figures in his second list as *Accipiter hawaii*. The existence in considerable numbers of a Californian species of *Carpodacus* is thought, and no doubt rightly, by Mr. Wilson to be

[1] In connexion herewith may be noticed the absence of Parrots, Kingfishers, and Doves—all Families that are very characteristic of an "Australian" Fauna.

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due to human agency, and accordingly I do not attach any importance to the fact; but there is one very puzzling species, of which only a few specimens seem to have been preserved, that needs particular attention. This was described by Judge Dole under the name of "*Fringilla anna*," but, of course, is no true *Fringilla*. Mr. Wilson brought home but a single specimen, which he owed to the kindness of the Hon. C. R. Bishop, it having been formerly in the Mills Collection; and, I believe, will establish for it a new genus, *Ciridops*—so named because its bright coloration recalls the well-known *Emberiza ciris* of Linnæus, the Painted Bunting of authors, or "Nonpareil" of bird-dealers. It is supposed to be now extinct, but it was a truly native species; it probably belongs to the fauna which I have above called "Columbian" (for want of a better name); but I cannot suppose it to have been so early a settler as the *Drepanididæ*, since it has changed so little. On the genus *Chasiempis* I would offer only one remark, and that is a word of caution to those who would, on the evidence of from a couple to half-a-dozen of specimens, or perhaps even on the evidence of a badly-coloured plate, attempt to break it up into definable "species." There remains of land-birds the genus *Phæornis*, which earlier systematists were inclined to put among the Fly-catchers (*Muscicapidæ*). The examples in spirit, placed by Mr. Wilson at Dr. Gadow's disposal, have enabled the latter to set aside that view, and to show that, of all the Families to which this genus has been supposed to be allied, "it differs least from the *Turdidæ*," and he would regard it "as a generalized or rather primitive Thrush."[1]

Of the water-birds I do not now propose to speak. Though possessing very many points of special interest to the ornithologist, so far as I understand them they throw no particular light on the general questions I have attempted to consider; and I would conclude this sketch of the Ornithology of the Sandwich Isles by referring to the unhappy fate of one of the most beautiful of their birds—the "Mamo," as I am told it was latterly called—*Drepanis pacifica*, one of the rarest species in collections, and apparently wholly extinct. Until Mr. Wilson brought the specimen which he has kindly given to this University, there seems not to have been one in the British Islands since the dispersal of the Leverian Museum, when two were bought by the Austrian agent, and are now at Vienna. How many other specimens may exist in the world I do not know, but the number can hardly exceed half-a-dozen. The bird was destroyed for the sake of its rich yellow feathers, used in former days to decorate the state robes of the chiefs, and according to all accounts a glorious sight one of those robes when in all its freshness must have been. As the species became scarce, recourse was had to the yellow tufts of *Acrulocercus nobilis*, which in depth of colour are very inferior; and when the *Drepanis* had ceased to exist, the name "O-o," which it seems to have borne in Cook's days, was transferred to the surviving species, according to a practice of which I have observed several instances in other nations. The general similarity of coloration in *Acrulocercus* and *Drepanis* is, indeed, obvious, and Dr. Gadow is inclined to consider the latter to have been the imitating form. If so, its mimicry has proved its destruction; but it clearly could not have foreseen that fashion should ordain its acquired yellow and black feathers to become desirable commodities among the human race, and it would be well to suspend judgment on this point. It had most likely a very limited range, which would, of course, hasten its end; and its two most conspicuous relatives, the scarlet *Vestiaria coccinea*, and the crimson *Himatione sanguinea*, though in equal request for their gaudy plumage, still exist, inhabiting (as will be seen by the table) all the islands that have been examined. How to account for the disappearance of *Chætoptila angustipluma* is beyond my power. It has no attractive colouring, and yet is declared to be extinct. The specimen given to us by Mr. Wilson is, I believe, the only one ever brought to Europe, and there seems to be but one (the type) in any American Museum. In mentioning the former I must acknowledge gratefully the generosity of Mr. Wilson, who promised a complete set of his bird-skins to the Museum of his old University on the completion of his work, a promise that he will doubtless perform.

Finally, I would point out that the conclusions established by Dr. Gadow's researches seem to coincide very much with those arrived at by Dr. David Sharp and Mr. Blackburn from their investigation of a small collection of Hawaiian *Coleoptera* (Trans. Roy. Dublin Society, series 2, vol. iii. Part 6). The entomological captures of Mr. Perkins are therefore awaited with considerable interest; and still more valuable, perhaps, may be his conchological collections, for it seems doubtful at present whether the Mollusks of the Sandwich Isles can be brought into line with their Birds and their Beetles. There is every chance of this question, among many others of importance, being solved if Mr. Perkins is enabled to prolong his stay for sufficient time; but that depends upon the financial support he may receive at home from the two learned bodies which I have mentioned, and from the Hawaiian Government and influential residents in the Islands.

ALFRED NEWTON.

Cambridge, 13 February, 1892.

[1] A minute comparison with the New-Zealand *Turnagra*, if that be still in the land of the living, would be desirable.

PROF. BUNSEN AND THE CHEMICAL SOCIETY.

IT was announced at the last meeting of the Chemical Society that it was proposed to present the following address to Prof. Bunsen, who has now been fifty years a Foreign Member of the Society; and the wish was expressed that, among those who sign it, all who have been his pupils should, as far as possible, be included. Fellows of the Society who have been pupils of Prof. Bunsen are requested to communicate with the Senior Secretary before March 19, in order that they may receive a form for signature.

To Privy Councillor Prof. Bunsen, Fellow of the Royal Society.

YOUR EXCELLENCY,—Fifty years have elapsed since the Chemical Society of London honoured itself by electing you one of its Foreign Members. Your name, and that of your illustrious fellow-countryman Liebig, are, in fact, first on a list which includes the most distinguished cultivators of chemical science in every civilized country of the world.

Our Society remembers with gratitude that you enriched the first volume of its Transactions by communicating to it the results of your ever-memorable investigation of cacodyl and its compounds. That you should have sent to us, in the first and most critical year of our existence, a memoir which the chemical world will ever regard as one of the classics of our science, is a significant proof of the beneficent interest with which you regarded our efforts to foster the growth of chemical learning in this country.

Your masterly investigations, in collaboration with our Fellow, Sir Lyon Playfair, on the gases evolved from iron furnaces, made by methods which you were the first to bring to perfection, greatly extended our knowledge of the theory of the smelting of iron. By the permanent benefit thus conferred on one of the most important of our industries, you have largely augmented our national wealth.

The half-century during which you have been associated with our Society has been fruitful in great dis-

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coveries and important inventions. It has witnessed the birth of new elements, the creation of new analytical methods, and an extraordinary development in the instrumental resources of our laboratories. Chemists will never forget that it is to your unwearied assiduity and single-minded devotion that science owes some of the most momentous of these discoveries, and some of the most valuable of these inventions. Your investigations will ever be regarded as models of the highest type of scientific research, and the memoirs in which you have embodied them shed an imperishable lustre on our literature. Your methods of analysis are among the most common of our manipulative operations, and the very furniture and instruments of our laboratories are an ever-present testimony to the obligations under which experimental chemistry will always remain to you.

Many of our members are proud to be numbered among your pupils, and those among them who have become teachers, have, we trust, caught and transmitted something, not only of the method, but also of the spirit, in which they themselves were taught. They have an abiding memory of your kindliness, of your constant and unselfish devotion to their interests, and of the generous sympathy and ready help which you extended to their efforts to enlarge the boundaries of our science.

We, the undersigned Fellows of the Chemical Society of London, now beg to offer you our heartfelt felicitations on the occasion of your Jubilee as a member of our body. It is our fervent hope that you may be able, for many years to come, to enjoy in health and happiness the leisure and repose which you have so justly and so honourably earned.

NOTES.

THE course taken by the Government with regard to a Teaching University for London has met with general approval. The proposed Charter, if it had been accepted, would have done almost irreparable injury to the cause of higher education in the capital. Now we have got rid of it, and the way is clear for new and more carefully considered schemes. The Royal Commission to which the question is to be referred will be free, if it chooses, to examine the question whether, after all, the institution needed by London might not be most readily and most effectually obtained by the development of the existing University. It may be hoped that on this and all other aspects of the question full evidence will be taken. What is wanted is that any recommendations which may be made by the Commission shall be based on extensive and accurate information as to the organization and the proper functions of Universities. On this subject some very crude notions are still current in England.

THE sixth annnal Photographic Conference in connection with the Camera Club will be held in the theatre of the Society of Arts, on Tuesday and Wednesday, March 22 and 23, under the presidency of Captain W. de W. Abney, F.R.S. All photographers are invited to take part in the Conference.

THE Botanical Society of France will hold an extraordinary meeting at Biskra, Algeria, during the first half of April.

A ROYAL COMMISSION has been appointed to consider the question of the water supply of London. Its task will be to inquire whether the present sources of supply are adequate in quantity and quality, and, if inadequate, whether such supply as may be required can be obtained within the watersheds of the Thames and the Lea, or must be obtained elsewhere. Among the members of the Commission are Sir Archibald Geikie and Prof. James Dewar.

A NUMBER of gentlemen, representing institutions interested in science-teaching, recently applied to the Vice-President of the Committee of Council on Education for permission to wait upon him with reference to the changes indicated in the circular issued by the Department of Science and Art, dated November 12, 1891. In answer to a question put by Mr. Schwann, Sir W. Hart Dyke stated in the House of Commons, on Tuesday evening, that he had informed the deputation that he did not think any good purpose would be served by his receiving them. He was, however, willing to reconsider the matter. Sir William expressed his belief that the changes announced in the circular would greatly stimulate more advanced scientific instruction. The minute had been well received in the better schools.

A NEW "Jahrbuch der Chemie" is to be issued by the German publisher, H. Bechhold, Frankfort. It will be edited by Prof. R. Meyer, who has secured the co-operation of many eminent men of science. The intention is that the progress of pure and applied chemistry shall be recorded every year in a connected series of articles.

DR. M. C. COOKE announces that, with the next number of *Grevillea*, his connection with it as editor and proprietor will come to a close, and it will rest with others whether the journal is to be continued. *Grevillea* is "A Quarterly Record of Cryptogamic Botany and its Literature." With the next number it will have completed its twentieth volume.

DR. B. ARTHUR WHITELEGGE will begin at the Royal Institution, on Thursday, March 24, a course of three lectures on epidemic waves. In the first lecture he will deal with cyclic waves; in the second (March 31), with superadded waves; in the third (April 7), with pandemic waves.

THE fifth ordinary meeting of the Egypt Exploration Fund was held last Friday, Sir John Fowler presiding. The balance-sheet was one of the most satisfactory ever presented to the members. The Fund, it was pointed out, was now used in two classes of operations—surveying and excavating; and both "seemed likely to be very successful." In the course of a brief discussion on the report, reference was made to "the generous contributions which had been handed to the Fund from America, in comparison with those received from England."

THE half-yearly general meeting of the Scottish Meteorological Society was held at Edinburgh on Monday, March 7. The following was the business:—(1) Report from the Council of the Society; (2) changes in the temperature of Scotland since 1764, by Dr. Buchan; (3) on the squall of February 1, 1892, by R. C. Mossman; (4) sunshine values at the Ben Nevis Observatory, by R. C. Mossman.

MR. E. J. STONE, F.R.S., and Mr. A. G. Vernon Harcourt, F.R.S., have been elected members of the Athenæum Club, under the rule which provides for "the annual introduction of a certain number of persons of distinguished eminence in science, literature, or the arts, or for public services."

THE fifteenth Convention of the National Electric Light Association, lately held at Buffalo, U.S., seems to have been unusually successful. This is attributed by the American journal *Electricity* to the fact that at previous meetings members attended "not for business but for pleasure." The counter-attractions were so varied and fascinating that it seems to have been difficult "to obtain a creditable attendance on the reading of the papers and the transaction of business." "The plan of doing away with these outside attractions," says *Electricity*, "was therefore an experiment, but so successful did it prove in this instance, that this last Convention will go down in history as 'the business Convention,' unless the increased success of succeeding meetings shall make it necessary to give this one the more specific title of 'the *first* business Convention.'"

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THE Natural History Society of Buda-Pesth is stated to number 7800 members. A special botanical meeting of the Society will in future be held monthly, under the presidency of Prof. Juranyi.

THE section of vegetable pathology of the botanical division, in the U.S. Department of Agriculture, was recently made a separate division by Act of Congress. The authorities of the new division decided to begin a fresh series of publications; and they have taken the first step towards the fulfilment of their purpose by the publication of an important *Bulletin*, by Dr. E. F. Smith, presenting "additional evidence on the communicability of peach yellows and peach rosettes."

JAPAN has no fewer than 700 earthquake-observing stations scattered over the Empire, and the Tokio correspondent of the *Times* is of opinion that they are all needed. He points out that not only are the Japanese shaken up by fully 500 earthquakes every year—some of them more or less destructive—but at intervals there comes a great disaster, amounting, as in the earthquake of October 28, 1891, to a national calamity. Japanese annals record twenty-nine such disasters during the last 1200 years.

A SEVERE earthquake shock, lasting twelve seconds, was felt at Napa, California, on March 13, at 8.30 a.m. The direction of the vibration was from north to south.

A CORRESPONDENT at Leon writes to us of an earthquake which was felt in Nicaragua on February 6. He speaks of a connected series of longitudinally oscillating progressive seismic waves, which lasted about ninety-two seconds. They were parallel with, and near, the cones and masses of volcanic ejecta which extend, with some interruptions, between the volcanic groups in the States of Salvador and Costa Rica. The earthquake began at 10.10 p.m.

PROF. HELLMANN, of Berlin, to whom we are indebted for many painstaking investigations into the origin of meteorological instruments and observations, has contributed to the *Zeitschrift für Luftschiffahrt* for January an article on the first balloon voyage made for scientific purposes. The works on the subject of ballooning, of which there are many, state that the first was by Robertson and Lhoëst in 1803, and the next in the following year, by Biot and Gay-Lussac. But this is not correct; the honour undoubtedly belongs to Dr. John Jeffries, of Boston (Mass.), who had for some years lived in this country. In 1786 he published a book (60 quarto pages and 2 plates), entitled, "A Narrative of the Two Aërial Voyages of Dr. Jeffries with Mons. Blanchard; with Meteorological Observations and Remarks." The first voyage was on November 30, 1784, from London to Dartford (Kent), and the second, on January 7, 1785, across the English Channel. A paper containing the results was read before the Royal Society in January 1786. The barometer taken was made by Jones, of Holborn, and read to 18 inches. The heights reached in the two voyages were about 9200 feet and 4500 feet respectively. The latter height was obtained trigonometrically by an officer at Calais, while the balloon lay stationary over the mid-Channel.

THE *Record of Technical and Secondary Education*, published monthly, can scarcely fail to be of service to all who interest themselves in educational progress. The number for March includes, besides editorial notes, accounts of County Council schemes and reports, scholarship schemes, recent progress in various districts, agricultural college for the south-eastern counties, and the financial management of the technical instruction fund. There are also instructive "miscellanea."

A WRITER in the *Mediterranean Naturalist* for March notes that no attention has hitherto been given to the fact that certain species of birds prefer certain trees. He says:—"It is a remarkable fact that, notwithstanding the voluminous literature that has been written on birds and their habits, no writer has noticed the preference that certain species of birds give to certain trees. Jays and rooks are found in the greatest number in oak-trees; finches, in lime-trees; and blackcaps among laurels. The nightingale is always found in the greatest numbers in nut-groves, while the thrush evinces a decided preference for the birch and ash. The beech is the favourite tree of the woodpecker; and the numerous families of tits are generally found in the greatest abundance among the blackthorn."

MR. W. M. GOLDTHWAITE, New York, is publishing a new monthly magazine called *Minerals*. The second number has been sent to us. It contains many short papers, in which interesting facts relating to various classes of minerals are presented in a bright and popular style.

MESSRS. J. AND A. CHURCHILL have issued a fifth edition of Dr. A. Tucker Wise's "Alpine Winter in its Medical Aspects." The work has been condensed and rewritten in many places.

THE U.S. Commissioner of Education has issued his Report for the year 1888–89, and, if it cannot be described as light reading, it is certainly a most instructive and useful work. It consists of two large volumes, and includes a number of chapters in which education in the United States is compared with that of England, France, Germany, and other countries. A full account is also given of normal schools, manual training schools, courses of study, &c. The second volume consists of "detailed statistics of educational systems and institutions, with comments and discussions."

THE peculiar milk-ferment known as "kefyr" or "kephir" has been supposed to be peculiar to the Caucasus and other parts of Eastern Europe and Western Asia. Mr. C. L. Mix has found a yeast apparently identical with it in use in Canada and the United States. It occurs in the form of small granules of a dirty brown colour, which retain their vitality for a long period, and consist of a small proportion of yeast-cells embedded in zoogloea-like masses of rod-shaped bacteria. The yeast-cells increase by budding, and no formation of spores has been detected in them. They do not invert cane-sugar like ordinary beer-yeast, but they cause alcoholic fermentation in milk-sugar or lactose and in dextrine, not in cane-sugar or saccharose. The bacteria are short cylindrical rods with homogeneous protoplasm, developing under cultivation into leptothrix-like filaments in which spores are formed. They appear to take no part in the fermentation, remaining almost entirely embedded in the zoogloea-masses during the process.

HERR W. BELAJEFF has communicated to the *Berichte* of the German Botanical Society a paper on the "Pollen-tube of Gymnosperms," which, if his observations—made on *Taxus* and *Juniperus*—are confirmed with regard to other members of the class, will greatly modify the accepted view as to the morphology of the different parts of the pollen-grain. Hitherto, the two or three small cells in the pollen-grain have been regarded as a survival of the male prothallium of the microspore. Belajeff shows that this cannot be the case, as they are cut off in succession from the large cell. Moreover, he states that it is not, as is usually supposed, the nucleus of the large cell which fertilizes the oosphere in the archegone, but the nucleus of one of the small cells. When the pollen-tube begins to develop, one of these small cells becomes detached and wanders down the tube. Its membrane becomes absorbed; its nucleus overtakes that of the large cell and divides into two; and it is one of these two daughter-nuclei of the wandering small cell, together with

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the protoplasm which surrounds it, that fuses with the nucleus of the oosphere in the archegone. The other small cell becomes entirely disorganized.

IT sometimes happens that peat bogs swell and burst, giving out a stream of dark mud. Herr Klinge has made a study of this rare phenomenon (*Bot. Jahrb.*), of which he has found only nine instances, in Europe, between 1745 and 1883 (seven of these being in Ireland). Heavy rains generally occur before the phenomenon, and detonations and earth vibrations precede and accompany it. The muddy stream which issues, of various fluidity, rolls along lumps of peat, and moves now more quickly, now more slowly. After the outbreak, the mud quickly hardens, and the bog sinks at the place it appeared, forming a funnel-shaped pool. The bogs considered by Herr Klinge have been almost all on high ground, not in valleys. He rejects the idea that the effects are due to excessive absorption of water by the bog. The peat layers, which often vary much in consistency, have each a certain power of imbibition, and the water absorbed does not exceed this limit. Excessive rain affects chiefly the upper layer not yet turned into peat and the cover of live vegetation, which get saturated like a sponge, after which the water collects in pools, and runs off in streams. The theory of gas explosions is also rejected; and the author considers the real cause to lie in land-slips, collapses, &c., of ground under the bog, permitting water or liquid mud to enter. This breaks up the bog mechanically, mixes with it and fluidifies it, and an outburst at the surface is the result. The limestone formations in Ireland, with their large caverns and masses of water, are naturally subject to those collapses, which, with the vibrations they induce, are more frequent in wet years. The heavy rains preceding the bog eruptions are thus to be regarded as only an indirect cause of these. Herr Klinge supposes that similar eruptions occurred in past geological periods, *e.g.* the Carboniferous, in some cases where fossil tree-stems are found in upright position.

THE geographical position of Mount St. Elias is of considerable popular interest in connection with the boundaries of Alaska. Mr. Israel C. Russell refers to the subject in a report published in the new number of the American *National Geographic Magazine*. In the convention between Great Britain and Russia, wherein the boundaries of Alaska are supposed to be defined, it is stated that the boundary, beginning at the south, after leaving Portland Channel, shall follow the summit of the mountains situated parallel to the coast as far as the 141st meridian, and from there northward the said meridian shall be the boundary to the Arctic Ocean. Whenever the summit of the mountains between Portland Channel and the 141st meridian "shall prove to be at the distance of more than ten marine leagues from the ocean, the limit between the British possessions and the line of coast which is to belong to Russia, above mentioned, shall be formed by a line parallel to the windings of the coast, and which shall never exceed the distance of ten marine leagues therefrom." As Mount St. Elias is approximately in longitude 140° 55′ 30″ west from Greenwich, it is therefore only 4′ 30″ of longitude, or 2½ statute miles, east of the boundary of the main portion of Alaska. Its distance from the nearest point on the coast is 33 statute miles. There is no coast range in South-Eastern Alaska parallel with the coast within the limits specified by the treaty, and the boundary must therefore be considered as a line parallel with the coast, and ten marine leagues, or 34½ statute miles, inland. The mountain is thus 1½ miles south of the boundary, and within the territory of the United States. Its position is so near the junction of the boundary separating South-Eastern Alaska from the North-West Territory with the 141st meridian, that it is practically a corner monument of the American national domain.

A NEW and very simple mode of synthesizing tartaric acid has been discovered by M. Genvresse, and is described by him in the current number of the *Comptes rendus*. It will doubtless be remembered that, some years ago, Dr. Perkin and Mr. Duppa prepared tartaric acid artificially by treating di-brom-succinic acid with hydrated oxide of silver, and this operation became the final stage of a complete synthesis from the elementary constituents, when, a short time afterwards, Prof. Maxwell Simpson succeeded in preparing succinic acid by the action of caustic potash upon the di-cyanide of ethylene. M. Genvresse now shows that tartaric acid may be directly synthesized by the action of nascent hydrogen upon glyoxylic acid, CHO—COOH, the curious compound, half aldehyde, half acid, derived from glycol, and hence directly from ethylene. If we double the formula of this acid, and add two atoms of hydrogen, we arrive at tartaric acid, COOH—CHOH—CHOH—COOH, and this is found to be capable of realization by reacting upon glyoxylic acid with nascent hydrogen liberated in its midst by the action of acetic acid upon zinc dust. A mixture of glyoxylic and acetic acids, the latter diluted with an equal weight of water, in the proportion of one molecule of glyoxylic to three molecules of acetic acid, was treated in small quantities at a time with zinc dust, at first at the ordinary temperature, and finally over the water-bath. The liquid was then filtered, and the zinc in solution removed by means of potassium carbonate. The clear liquid was then mixed with calcium chloride solution, and after removal of any calcium carbonate precipitate, a white crystalline precipitate commenced to separate. This precipitate was found to yield all the reactions of a tartrate, such as silvering glass when gently warmed with ammonia and silver nitrate. Its analysis gave numbers indicating the formula $C_4H_4CaO_6 + 4H_2O$, which is the composition of ordinary tartrate of lime. By treating this salt with the calculated quantity of sulphuric acid diluted with twenty times its volume of water, filtering off the precipitated calcium sulphate and evaporating the filtrate over oil of vitriol, the acid itself was obtained in large crystals. It is interesting to find that the tartaric acid obtained by this mode of synthesis is the optically inactive variety known as racemic acid, there being apparently equal numbers of molecules of both the dextro and lævo varieties produced. The crystals consequently do not show hemihedral faces; the angles observed corresponded with those observed by Provostaye and by Rammelsberg in the case of racemic acid. It may be remarked that, as the product of the synthesis of Dr. Perkin and Mr. Duppa, a mixture of racemic acid with the truly inactive tartaric acid, in which neutralization within the molecules themselves occurs, was obtained. This new synthesis of tartaric acid from glyoxylic acid would appear to throw some light upon the natural formation of tartaric acid. For, remembering the close relationship between glyoxylic and oxalic acids, which latter we know to be one most readily formed in vegetable tissues, and the reducing agencies which appear to be connected with chlorophyll, we have all the means at hand to account, in view of the work of M. Genvresse, for the natural synthesis of tartaric acid.

THE additions to the Zoological Society's Gardens during the past week include a Common Squirrel (*Sciurus vulgaris*), British, presented by Mrs. Crick; a Merlin (*Falco æsalon*), European, presented by Mr. T. A. Cotton; a Blue Titmouse (*Parus cæruleus*), British, presented by Captain Salvin; two Blossom-headed Parrakeets (*Palæornis cyanocephalus*) from India, presented by La Comtesse Cottrell; a Green Monkey (*Cercopithecus callitrichus* δ) from West Africa, deposited; a —— Hawk (*Asturina* sp. inc.) from South America, purchased; four Yellow-bellied Liothrix (*Liothrix luteus*) from India, received in exchange; sixteen Puff Adders (*Vipera arietans*), born in the Gardens.

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OUR ASTRONOMICAL COLUMN.

SOLAR INVESTIGATIONS.—*Astronomy and Astro-Physics* for February contains a short note by Prof. Hale, to the effect that photographs have been taken at Kenwood Observatory, showing the H and K lines reversed in regions widely distributed over the sun's disk. These regions closely resemble faculæ in appearance.

At the meeting of the Paris Academy on March 7, Prof. Tacchini communicated a paper on the distribution in latitude of the solar phenomena observed at the Royal Observatory of the Roman College during the second half of last year. Prominences have been more frequent in the northern hemisphere than in the southern, although in the preceding half-year, and in 1889 and 1890, they were more frequently observed in the southern solar hemisphere. The zones of maximum frequency occurred between latitudes 40° and 60°. Faculæ also have been most numerous north of the equator, and the zone of maximum frequency of these phenomena appears to be between latitudes 10° and 30°. Spots have been most abundant north of the equator, with a maximum frequency in the same zones as faculæ.

In the *Comptes rendus* containing Prof. Tacchini's results, occurs also a note by J. Fényi, on a remarkable prominence observed at Kalocsa, on February 19, as the recent large spot-group was passing over the sun's limb.

NEW DOUBLE STAR, 26 AURIGÆ.—In a communication to the *Astronomical Journal*, No. 256, Mr. S. W. Burnham records the discovery that 26 Aurigæ is a close double star, made up of two nearly equal components. His measures of position-angle and distance for 1892·0 are 344°·4 and 0″·15; and of magnitudes, 5·6 and 6·0. The distance very probably never exceeds a quarter of a second, or the duplicity of the star would have been noticed by many observers of the distant companion discovered by Herschel in 1783.

ROTATION OF JUPITER.—Writing in the March number of the *Observatory*, Mr. Denning notes that his observations of one of the chief dark spots in Jupiter's north temperate belt, for the period from August 21 to November 3 (180 rotations), gave the mean period of rotation 9h. 49m. 36·9s. Observations of the red spot, from August 7 to February 2 (432 rotations), indicate a mean period of 9h. 55m. 42·2s. The value obtained during the opposition of 1890 was 9h. 55m. 40·2s., so that the motion of rotation of the red spot would appear to have slackened by two seconds. Since the period of rotation derived from this spot is now 6m. 5s. longer than that given by the dark spots on the north temperate belt, the latter revolves around Jupiter, relatively to the former, in 40½ days.

THE NEW STAR IN AURIGA.—In No. 3078 of the *Astronomische Nachrichten* are recorded three communications relative to this Nova, two of which refer to its position, while the third deals with its spectrum. The observations of the last-named were made at the Astro-physical Observatory in Héreny, Ungarn, by Herrn Eugen and Gothard, previous to February 15. On the 10th and 13th of the same month the following lines were observed:—

Bright lines.	February 10. Wave-lengths in micro-millimetres.	February 13. Wave-lengths in micro-millimetres.
I.	654·2	—
II.	532·2	530·3
III.	513·2	516·8
IV.	501·9	501·9
V.	492·3	492·3
VI.	486·6	486·2
VII.	439·0	—
End of spectrum	412·0	—

In the first series of observations, the authors give suggestions for some of the lines, and for comparison's sake we have added Mr. Lockyer's also, both of which are included in the following table:—

Wave-lengths.	Origin.	Lockyer.
532·2	Chromosphere line (531·7)	— (531·3)
501·9	Nebula-line (500·5)	Mg (500·6)
486·6	Hydrogen line Hβ	H (486·1)
439·0	,, ,, Hγ	— —

In the second series comparisons were made with Geissler's tubes, and the following measurements were made:—

	Geissler Tubes.	Nova.
IV.	500·6	501·9
V.	492·6	492·3
VI.	486·2	486·2

THE LICK SPECTROSCOPE.—The February number of *Astronomy and Astro Physics* contains an excellent plate, taken from a photograph by Mr. Barnard, of the spectroscope on the great 36-inch refractor of the Lick Observatory. In the description of the instrument it is stated that the spectroscope itself is no less than 130 pounds in weight, while the two brass rods which connect it to the telescope form an extra addition of 75 to 80 pounds. Accompanying the plate, which shows the general arrangement of its parts, is a plan of the instrument which is completely described in the text. Many ingenious ideas have been displayed in the completion of the instrument as regards accessories, such as that of the lighting up of the pointers and production of the comparison-spark. Owing to the great focal length of the telescope, only 1·06 inch of the full aperture of the spectroscope can be used, but when it is dismounted, it rests on a truck, and its full aperture, 1·50 inch, is then available for laboratory work.

A BRIGHT COMET.—A circular from the Royal Observatory, Edinburgh, communicated by Dr. Ralph Copeland, and dated March 11, contains the following information of the appearance of a bright comet:—

Dr. L. Swift discovered a bright comet at Rochester, N.Y., at 16h. 50·1m. local mean time, on March 6, its place then being R.A. 18h. 59m., South Declination 31° 20′. It was moving eastwards.

The exact place of the comet was observed at the Royal Observatory, Cape of Good Hope, on the 8th inst. to be:—

Cape of Good Hope Mean Time	16h. 58m. 36s.
Right Ascension	19h. 2m. 27·8s.
South Declination	30° 2′ 54″

Astronomische Nachrichten, No. 3079, also contains some information with regard to this comet. A telegram from Boston contained the following: "Comet Swift was observed by Barnard, March 8·0399 G.M.T.; R.A. app. = 285° 51′ 20″, Polar Distance = 120° 32′ 53″. Comet is visible to the naked eye."

Another telegram from Capetown read: "Comet was observed March 9·6024 G.M.T.; Right Ascension = 287° 45′ 50″, P.D. app. = 119° 16′ 12″. R.A. March 8 read, 286° 36′ 57″ instead of 285° 36′ 57″."

Prof. H. Kreutz in the same number gives the following elements and ephemeris:—

Elements.

T = 1892 March 26·8545 Berlin M.T.

$\omega = 9\ 57'21$
$\Omega = 239\ 33'76$ } M. Eq. 1892·0
$i = 33\ 38'44$
log. q = 0·02208.

Ephemeris for 12h. Berlin M.T.

1892.		α h. m. s.	δ. ° ′
March	9	19 10 25	− 29 22·9
	13	29 13	25 57 8
	17	46 57	22 22·0
	21	20 3 41	18 39·0
	25	19 26	14 52·6
	29	34 17	11 5·9
April	2	20 48 19	− 7 21·9

CALCULATION OF TRAJECTORIES OF ELONGATED PROJECTILES.

AS the correct determination of the law of resistance of the air to the motion of elongated projectiles is a matter of considerable national importance, I hope you will allow me to offer a few concluding remarks explanatory of the present state of the case. Some results of my first systematic experiments were published in 1868, and General Mayevski, after applying a few tests of his own, adopted my results in his "Traité de Balistique Extérieure," 1872. Siacci made use of these results in

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his ballistic tables (1881). But Mayevski appears to have recently become a disciple of Krupp, from the diagram in NATURE, August 28, 1890 (p. 411), where the dotted line (1) represents roughly the resistance of the air to ogival-headed projectiles given in my "Final Report," 1880; line (2) represents the law of resistance deduced from these results by Major Ingalls, of the United States Artillery, which is similar to the law deduced by me (NATURE, April 29, 1886, p. 605); and line (3) represents the results Mayevski professes to have deduced from Krupp's Meppen experiments. My law of resistance has been very closely followed throughout by Mayevski, as is evident from the diagram above referred to, which is suggestive of a free use of the parallel ruler. The main object of these proceedings seems to have been to persuade the world, and the Americans especially, that Krupp guns are far superior to English guns, regard being had to the initial steadiness imparted by them to their projectiles. But this claim is unworthy of notice so far as it depends upon the Meppen experiments with chronoscopes, the patent defects of some of which were pointed out in NATURE, April 29, 1886 (p. 606). If, however, Government consider this matter worthy of investigation, there are simple practical methods of determining the comparative steadiness of projectiles fired from two or more guns.

At present, my concern is with English guns only, and I wish to point out, as briefly as possible, (1) that my results obtained from English guns are quite correct; (2) that the coefficients of resistance for each round are expressed by such a short unit of time that they are made to appear more irregular than they are in reality, while the variation in their value is just what experiment leads us to expect; and (3) that when my *mean* coefficients are *fairly* used to calculate results of *good* experiments made with *recent* English guns, in *calm* weather, the agreement between calculation and experiment is perfectly satisfactory.

My chronometric arrangements were made with a view to guard against the errors of remaining magnetism, which is the chief source of error in the measurement of extremely short intervals of time by the help of electro-magnetism. All the time-records were made by one electro-magnet, whose galvanic current was interrupted once a second by the swing of a half-seconds clock pendulum; and all the screen records were made by another electro-magnet, whose galvanic current was being rapidly interrupted by a self-acting contact-breaker, till the pull of the lanyard turned off the contact-breaker, and then fired the gun, after which the shot momentarily interrupted the galvanic current as it passed each of ten or more equi-distant screens. Also care was taken to reduce the strength of the galvanic currents, so as to leave each electro-magnet only just sufficient power for the performance of its appointed work. Under these circumstances it may be safely assumed that, if there were any errors arising from remaining magnetism, in either clock or screen records, they would be *constant* in each case, and therefore they would have no injurious effect on the result obtained. The records on a 4-inch cylinder were read off by a vernier to the 1/3000 of its circumference; but as the scale of time was in general only 9 or 10 inches to the second, it may be concluded that the records were read off to the 1/2000 of a second at least.

The accuracy of the time and of the screen records was tested by differencing, when slight adjustments were applied to render the first and second order of differences regular to an *additional place of decimals*. The following is a list of the adjustments so applied in seven successive rounds, 146–152, which are fair samples of those applied in the other rounds (1867–68). They are expressed in decimals of the unit read off by the vernier, or of the 1/2000 of a second. In round 258 an example is given of the correction of an occasional erroneous record at screen 6:—

Round	146.	147.	148.	149.	150.	151.	152.	258.
Screen								
1	+0·3	+0·2	−0·2	0·0	+0·2	0·0	0·0	− 0·1
2	−0·5	−0·1	+0·5	0·0	0·0	+0·2	+0·6	+ 0·1
3	−0·3	−0·2	−0·1	0·0	−0·7	−0·3	−0·1	+ 0·2
4	−0·1	−0·1	−1·0	0·0	+0·1	+0·4	−0·1	− 0·6
5	+0·1	+0·2	0·2	0·0	+1·4	+0·3	+0·7	− 0·1
6	+0·3	−0·2	+0·3	0·0	+0·3	+0·5	+0·3	+32·9
7	−0·4	+1·6	+0·4	0·0	−0·2	0·0	−0·4	+ 0·6
8	−0·1	+0·7	−0·7	−1·0	−0·1		+0·7	+ 0·2
9	+0·2	0·0	+0·1	0·0	+0·6		−0·4	− 0·1
10	−1·5	−0·4	−0·2	0·0	−0·1		+0·2	− 0·1

Here is conclusive evidence of the perfect trustworthiness of the observations made, such as no other ballistic experiments have afforded to my knowledge. When the readings of the screen records required only such slight adjustments as those above indicated, there could be no reasonable doubt about the perfect accuracy of the experiment, and the round was accordingly adopted as good in all cases, unless there was some known disturbing cause, as when the bronze gun expanded, or where the gas check left the shot, &c.

Although the records are read off only to the 1/2000 of a second, we are able to express the coefficients of resistance with much greater exactness through the employment of a long range (1350 feet) where the only absolute errors in time possible are at the two extremities of the range, and the accuracy of each of these readings is tested by the differencing. Supposing the retarding force of the air, acting upon an ogival-headed projectile moving in the direction of its axis with a velocity v f.s., to be expressed by $-2bv^3$, the values of $2000bw/d^2$ corresponding to all velocities from 900 to 1700 f.s. were found by experiment in 1867–68, where w denotes the weight of the projectile in pounds, and d its diameter in inches. Corresponding to a velocity of 1200 f.s., the mean value of $2000bw/d^2$ was found to be 0·00010089. But to avoid the use of so many decimal places, K was subsequently employed to denote $(1000)^3 2bw/d^2 = (1000)^3 w\Delta^2 t/d^2 l^2 = 108·9$, where $\Delta^2 t$ is the second difference of the times at which the shot passed successive equi-distant screens l feet apart, with a velocity 1200 f.s. more or less, and, in the case of the solid 5-inch shot it equals 0″·00124. From this it appears that for the specified shot the time by which the unit of K is expressed is 0″·0000112 in $\Delta^2 t$. Considering the shortness of this unit of time, it seems very natural that some variations should have been found in the experimental values of K for any specified velocity, derived as they have been from both *hollow* and *solid* projectiles fired with *various charges* from 3, 5, 7, and 9-inch guns. If we turn to actual experiment, it is plain that the coefficients of resistance for any given velocity cannot practically retain a constant value for all rounds. For do not we frequently read that shot are "noisy," or "unsteady" in their flight? There was much unsteadiness in the Jubilee rounds; and Captain May, R.N., in speaking of experiments with *recent* guns, remarked, "the range of 500 yards is selected, because at this range shell which *start unsteadily* will have steadied (that is if they ever do so), &c." It is, therefore, quite natural that exact experiment should afford evidence of this unsteadiness.

It now remains to test the value of my mean results by making use of them to calculate the ranges and times of flight of projectiles for comparison with the results of experiments made with *recent* guns. In 1879 some range tables of the 6·3-inch howitzer were forwarded to me to show that my coefficients for *low* velocities did *not* give satisfactory results. As the muzzle velocities in these tables were 332, 507, 628, 697, 740, and 751 f.s., and the elevations varied from 5° to 40°, the trajectories were much curved, so that my general tables were not applicable in these cases. But when the ranges and times of flight were properly calculated by Bernoulli's method, experiment and calculation were found to agree remarkably well. In the same way numerous German range tables (Krupp guns?) were calculated for muzzle velocities varying from 380 to 774 f.s., which gave very satisfactory results in general. Although there was no allowance for jump or vertical drift in these calculations for *low* muzzle velocities, the calculated often exceeded slightly the experimental ranges, showing that my resistances were perhaps a little too low. The results of each of these comparisons—32 English and 82 German—will be found in my "Final Report," 1880, pp. 45–47. For specimens of the best and worst results of each kind, see NATURE, April 29, 1886 (p. 606). Now Mayevski proposes to reduce these coefficients of resistance, already rather too low, by 20 per cent. more! (Ingalls, pp. 29, 36).

In consequence of the Krupp scare, the authorities desired to have the accuracy of my results tested by practice on a *long* range, with a *recent* gun, and for this purpose *they selected* the 4-inch B.L. gun. Careful experiments were subsequently carried out with this gun (1887), which showed that my coefficients of resistance were perfectly satisfactory. But there was no real necessity for any special experiments to be made with this gun, as its own range table was afterwards found to be abundantly sufficient for the purpose of testing my results. By calculating trajectories carefully by Bernoulli's method, and then recalcu-

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lating by the general tables the time of flight over the range already obtained, and also the striking velocity, it is found that the general tables may be used for elevations of the 4-inch gun as high as 15°, or even more, with a muzzle velocity of 1900 f.s. In this way the merest tyro may test my coefficients for his own satisfaction by calculating the times of flight over ranges of two or three miles given in any good range table for a high muzzle velocity. The following are the results of such testing, using the full extent of the range table of the 4-inch B.L. gun, chosen by the authorities. Muzzle velocity, 1900 f.s.; weight of ogival-headed shot (two diameters), 25 pounds :—

Range	2000.	3000.	4000.	5000.	6000.	7500 yds.
Horizontal muzzle vel.	1898·5	1895·0	1888·0	1875·0	1853·5	1796·8 f.s.
Horizontal striking vel.	1124·9	955·4	843·4	751·2	667·6	556·5 f.s.
Exp. time	4·21	7·20	10·49	14·3	18·4	26·2
Calc. time	4·17	7·10	10·48	14·30	18 66	26·43
Difference	−0·04	−0·10	−0·01	0·00	+0·26	+0·23

Here, as before, the calculated time is rather too short for velocities 1900 to 751 f.s. And if we allowed for a slight diminution of the density of the air for the higher elevations, as we ought to do, the calculated would throughout fall very slightly short of the experimental times of flight. Thus it is clear that my coefficients of resistance give perfectly satisfactory results when fairly tested by *recent* guns, chosen by Government, for velocities 330 to 751 f.s., and from 751 to 1900 f.s., or from 330 to 1900 f.s.

In the same way we may use the model range table, carefully prepared for the 12-inch B.L. gun by Captain May, R.N., for a muzzle velocity 1892 f.s., and weight of shot 714 pounds (Proc. R.A. Inst., 1886, p. 356) :—

Range	1000.	2000.	3000.	4000 yards.
Experimental time ...	1·66	3·47	5·44	7·61
Calculated time ...	1·654	3·457	5·428	7·591
Difference	−0·006	−0·013	−0·012	−0·019

Here, again, the calculated times of flight, being a trifle too short, show that my coefficients of resistance are very slightly too low.

When coefficients tested in this manner give calculated times of flight accurately over ranges gradually increasing up to two or three miles, those coefficients must be correct for all practical purposes, and they will give correctly the striking velocity and time of flight for any other reasonable distance from the gun.

The tables of "Mayevski nach Siacci," printed by Krupp, 1883, may be used to calculate the times of flight of the shot fired from the 4-inch gun as above :—

Range ...	2000.	3000.	4000.	5000.	6000.	7500 yds.
Experimental time	4·21	7·20	10·49	14·3	18·4	26·2
Calculated time ...	4·10	6·91	10·11	13·69	17·67	24·58
Difference ...	−0·11	−0·29	−0·38	−0·61	−0·73	−1·62

From this it is evident that the reduction of my coefficients proposed by Mayevski on the strength of Krupp's experiments is uncalled for.

Again, it has been urged that my resistances ought to be reduced in order to adapt them to *recent* guns, which, it is assumed, impart an increased degree of steadiness to their projectiles. But that assumption requires proof. After most carefully testing the admirable range tables of the 4-inch and 12-inch B.L. guns, I have failed to find any indication whatever of increased steadiness in their projectiles. Besides, Admiral Robert A. E. Scott wrote to the *Morning Post* (November 9, 1889), condemning the system of rifling the 110-ton gun, which he blamed for causing the projectiles to "issue from their guns with a *very unsteady motion*." He then went on to notice the large number of unsteady shot fired from the 9·2-inch gun in 1888. I would also remind my critics that my coefficients of resistance for velocities 1000 to 1700 f.s. were derived from experiments made in 1867–68, while all those for velocities less than 1000 f.s., and greater than 1700 f.s., were derived from experiments in 1878–80, carried out with some of the newest and best guns of the time. As conclusive evidence of the excellence of the 3, 5, and 7-inch guns used in the early experiments, reference may be made to the fact that, from the results of the experiments of 1867–68, I was able to deduce the Newtonian law of resistance for velocities 1350 to 1700 f.s. (Proc. R.A. Inst., 1871); and using the mean of the eight numerical coefficients there given for velocities 1350, 1400, . . . 1700 f.s., the numerical value of k will be found to be 143·9.

In 1879 experiments were made with a *new* Armstrong 6-inch B.L. gun, with velocities 1700 to 2250 f.s. (Reports, &c., Part ii., 48); and again, in 1880, further experiments were carried out with a *new* Armstrong 8-inch B.L. gun, with velocities 2250 to 2800 f.s. (Final Report, 56). Combining these three sets of experiments, Major Ingalls found that the Newtonian law of resistance held good for velocities 1330 to 2800 f.s., where $k = 142·1$ (Ext. Bal., 36). I also deduced the same law for velocities 1300 to 2800 f.s., where $k = 141·5$ (NATURE, 1886, p. 606). And lastly, after a thorough revision of the reduction of every round, I finally adopted the same law for velocities above 1300 f.s., where $k = 141·2$.

Hence it appears that the early experiments of 1867–68 were so accurate that they gave a correct law of resistance for velocities 1350 to 1700 f.s., which has since been found to hold good for velocities 1300 to 2800 f.s.; and they also gave the coefficient $k = 143·9$ (with *studded* shot) sufficiently accurate for all practical purposes up to a velocity 2800 f.s. This is conclusive evidence of the steadiness of the shot in the early experiments, and of the accuracy of the method of reduction of those experiments.

But when those coefficients, which have been found correct by the use of the general tables, are employed to calculate trajectories of elongated shot moving with high velocities, the calculated ranges and times of flight gradually fall more and more below those quantities given in the range tables, as the elevation increases beyond 4° or 5°. These defects are generally only small when the variation in the density of the air is taken into account; but their presence indicates some slight disturbing cause independent of the coefficients of resistance. We can now make use of the exact method of calculating trajectories given by modern analysis, which was first published by J. Bernoulli. But this method applies with strictness only to the motion of a spherical projectile, whose centre of gravity coincides with its centre of figure. Many years ago Count St. Robert remarked: "On doit en conclure que les formules ordinaires de la balistique ne peuvent représenter la trajectoire décrite par les projectiles *allongés*" (Balistique, p. 183). Also Mayevski has published an elaborate paper, "De l'Influence du Mouvement de Rotation sur la Trajectoire des Projectiles oblongs dans l'Air" (Technologie Mil., 1866, pp. 1–150), which, however, leads to no useful result beyond showing that the author recognized the effect of drift on the form of the trajectory. The chief cause of the difficulty is this. For a short time after a steady elongated shot has left a rifled gun, the shot preserves the parallelism of its axis, and in consequence of the action of gravity the point of the shot gradually rises above its trajectory till the resistance of the air causes the axis of the shot to begin to describe a conical surface, with nearly constant vertical angle, about the moving tangent to the trajectory. Consequently, soon after a steady elongated shot leaves the muzzle of a rifled gun, the resistance of the air acting on the inclined under side of the shot, begins to raise the shot bodily, and continues to do so until its axis has made one-fourth of a revolution about the tangent to the trajectory. This vertical drift, near the gun, causes the shot to move in its path as if it had been fired at a slightly increased elevation. Consequently, the observed range and time of flight are each somewhat greater than that due to the elevation at which the gun was laid.

Another difficulty, common, however, to both spherical and elongated shot, is caused by the jump of the gun. In the range tables of the 4 and 12-inch guns above considered, six minutes were allowed for the effect of jump for all elevations. But Major Ingalls remarks that "it varies in value from an angle

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too small to be appreciable, to one of a degree of arc or even more. . . . It also varies somewhat with the angle of elevation." In one of his examples he supposed the jump to be 22 to 23 minutes. Although the jump and vertical drift are uncertain in amount, they have considerable influence on the range and time of flight, and on this account the calculation of trajectories is a decidedly unsatisfactory method of testing the coefficients of resistance of the air to elongated projectiles.

In the early days of elongated projectiles the vertical drift caused by the "kite-like action" of the shot was duly recognized, but of late this disturbing force has been commonly ignored. For now, when a calculated range is shorter than the experimental range, it is at once assumed that the theoretical resistance is too high. This resistance is forthwith reduced so as to make the calculated agree with the experimental ranges, but seldom is any care taken to compare the time of flight calculated with this reduced resistance with the experimental time. It ought, however, to be remembered that, while a reduction of resistance *increases* the range it *diminishes* the time of flight over a given range. Major Ingalls has given a complete example of this method of correcting my resistance (Problems, &c., p. 151). For elevations of

2°, 4°, 6°, 8°, and 10°,

he found it necessary to reduce my coefficients by

4·5 2·3 5·2 8·1 9.7 per cent.,

in order to obtain the experimental ranges by calculation; and these *reduced* resistances gave the calculated times of flight *too short* by

0″·09 0″·12 0″·13 0″·26 and 0″·42,

which proves clearly that the theoretical resistances had been too much reduced throughout. Also, if the method of correction pursued in the above example was correct, it would follow that the coefficient of resistance is a function of the elevation, which is simply absurd.

On the other hand, suppose we correct the elevation so as to make the calculated agree with the experimental range, which seems to me to be a satisfactory *approximate* correction in such cases. A careful calculation of the trajectory of an ogival-headed shot (two diameters) fired from the 4-inch B.L. gun, at an elevation of 15°, gave a range of 6448 yards (185 yards too short by the range table), and time of flight 20″·46 (1″·10 too short), the density of the air having been supposed to vary with the height of the shot. Now, corresponding to an elevation of 14° 16′, the range table gives a range of 6448 yards, and time of flight 20″·53. If we suppose that the elevation of the gun, 14° 16′, was practically increased by 44′ by jump and vertical drift, we obtain an elevation of 15° for the initial direction of the shot. But, according to calculation, for an elevation of 15° we have found the range 6448 yards exactly, and the time of flight 20″·46, which is only −0″·07 in error, and the calculated horizontal striking velocity is 646 f.s. This, I maintain, is the proper method of correction, because it corrects both *range* and *time* of flight, when there is no wind. Using the general tables and the horizontal muzzle velocity, the calculated time over 6448 yards is found to be 20″·53, and horizontal striking velocity 647 f.s., where $\tau = 0{\cdot}967$, the mean density of the air, as the projectile would rise to a height of 1800 feet.

If the above be a correct view of what takes place, it follows that the axis of the shot during its flight is inclined at such a small angle to the direction of its motion, that the resistance of the air to its forward motion is not sensibly greater than when it moves in the direction of its axis. But small as this angle must be, we find evidence of the marked effect of the lateral action of the air in causing the shot to drift to the right towards the end of the range. It is therefore to be expected that the resistance of the air, acting from below on the shot, soon after it leaves the gun will raise the shot upwards, and cause it to move as if it had been fired at an elevation a little greater than that at which the gun was laid.

The Ordnance Committee fired some ogival-headed projectiles from a 9·2-inch wire gun at high elevations in 1888, professedly to try whether calculations of trajectories at very long ranges are trustworthy. But before experimenting they invited calculators to furnish them with the calculated range and time of flight of a 380-pound elongated shot fired at an elevation of 40° with a muzzle velocity of 2360 f.s. My calculations for an ogival head, struck with a radius of one diameter and a half, gave a range of 19,436 yards, and time of flight 62″·15, which were sent in in March 1888. Allowing 300 yards for jump and vertical drift, I obtained a range of 19,736 yards. In April two rounds were fired with a velocity of 2375 f.s. at an elevation of 40°, which gave ranges of 21,048 and 21,358 yards. My reply to this announcement, by return of post, was that the ranges were about 1500 yards too great. In July the experiment was repeated, which gave ranges of 20,236 and 20,210 yards, being a reduction of near 1000 yards. Two rounds fired at an elevation of 30° gave ranges of 17,500 and 18,344 yards; two at 35° gave ranges of 19,420 and 18,963 yards; and one at 45° gave a range of 21,800 yards. The times of flight have not been published. These great variations in range were due in part to unsteadiness in the motion of the shot, but chiefly to the prevalence of high favourable winds.

In order to test guns or coefficients of resistance in a satisfactory manner, *calm weather* is absolutely necessary. And if the shot is expected to rise to a height of two or three miles, trial balloons ought to be sent up to test the state of the currents in the higher regions. Afterwards, when good mean results of experiments have been obtained, then, and not till then, may these mean results be used to test the results of calculation. Experiments ought not to be made at all unless precautions necessary to secure a correct result can be taken.

I have calculated a complete range table for the 9·2-inch gun for elevations 1° to 45°, according to the original programme of the Ordnance Committee (NATURE, September 13, 1888, p. 468), where the net results of calculation have been given. These ranges will require an addition perhaps of about 2 per cent. for jump, vertical drift, &c., and a further 1 per cent. if the ogival head be struck with a radius of two diameters instead of one and a half, and the increment of time must be ruled by this increment of range.

My sole object having been to obtain the correct law of resistance of the air to the motion of projectiles, I was always ready to consider any proposed correction. But my results, obtained from numerous and most exact experiments, could be changed only on perfectly satisfactory evidence. That evidence I have failed to obtain in any single case, so that my results remain practically the same as they were given in my original reports, 1868–80. I have recently published a revised account of all my experiments, accompanied by newly calculated general tables, for both English and French measures, and other tables required in the calculation of trajectories, according to the results of modern analysis. With these helps I have now thoroughly tested my final results by the use of range tables of the 4-inch, 6·3-inch, and 12-inch guns, with the most gratifying results. And Major McClintock, R.A., has tested my coefficients for small-arm bullets, with very satisfactory results (Proc. R.A. Inst., xii. 569). This evidence of the accuracy of my results is the more valuable because it is derived from Government experiments, made for other purposes, which have manifestly been carried out with great care and ability. Anyone so disposed has the means to re-examine the whole matter for himself. If there be not some error in my calculations, it appears that my results do not admit of any real improvement, and consequently my labours in this matter may be considered to have reached a satisfactory conclusion.

F. BASHFORTH.

FORTHCOMING SCIENTIFIC BOOKS.

THE following is a list of scientific works which will be issued by various publishers in the course of the spring:—

Messrs. Macmillan and Co.:—"Essays on some Controverted Questions," with a Prologue, by Prof. Huxley; "The Beauties of Nature," by Sir John Lubbock, F.R.S., illustrated; "Island Life; or, The Phenomena and Causes of Insular Faunas and Floras," including a revision and attempted solution of the problem of geological climates, by A. R. Wallace, with illustrations and maps, new and cheaper edition; "The Apodidæ," a morphological study, by Henry M. Bernard, illustrated (Nature Series); "Experimental Evolution," by Henry de Varigny; "The Diseases of Modern Life," by B. W. Richardson, F.R.S., new and cheaper edition; "The Geography of the British Colonies"—"Canada," by George M. Dawson, "Australia and New Zealand," by Alexander Sutherland (Macmillan's Geographical Series); "Scientific Papers," by Oliver Heaviside; "The Algebra of Co-Planar Vectors and Trigonometry," by R. B. Hayward, F.R.S., Assistant Master at Harrow; "Key and Students'

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Companion to Higher Arithmetic and Elementary Mensuration," by P. Goyen, Inspector of Schools, Dunedin, New Zealand; "Arithmetic for Schools," by Barnard Smith, late Fellow and Bursar of St. Peter's College, Cambridge; carefully revised in accordance with modern methods by W. H. H. Hudson, Professor of Mathematics, King's College, London; "Blowpipe Analysis," by J. Landauer, authorized English edition by J. Taylor and W. E. Kay, of the Owens College, Manchester, new edition, thoroughly revised with the assistance of Prof. Landauer; "Nature's Story Books," I. "Sunshine," by Amy Johnson, illustrated.

The Clarendon Press:—"Mathematical Papers of the late Henry J. S. Smith, Savilian Professor of Geometry in the University of Oxford," with portrait and memoir, two vols.; "Plane Trigonometry without Imaginaries," by R. C. J. Nixon; "A Treatise on Electricity and Magnetism," by J. Clerk Maxwell, new edition; "A Manual of Crystallography," by M. H. N. Story-Maskelyne; "Elementary Mechanics," by A. L. Selby; "Weismann's Lectures on Heredity," Vol. II., edited by E. B. Poulton, F.R.S.; "Epidemic Influenza," by F. A. Dixey.

The Cambridge University Press:—"A Treatise on the Mathematical Theory of Electricity," by A. E. H. Love, Fellow of St. John's College, Cambridge, two vols., Vol. I. in the press; "The Origin of Metallic Currency and Weight Standards," by W. Ridgeway, Professor of Greek, Queen's College, Cork, and late Fellow of Gonville and Caius College; "Solutions of the Examples in 'A Treatise on Elementary Dynamics,'" by S. L. Loney, formerly Fellow of Sidney Sussex College, Cambridge.

Messrs. Longmans and Co.:—"Darwin and after Darwin: an Exposition of the Darwinian Theory, and a Discussion of Post-Darwinian Questions," by George John Romanes, F.R.S., two vols.

Messrs. A. and C. Black:—"Life in Motion; or, Muscle and Nerve," a series of lectures delivered at the Royal Institution, Christmas, 1891, by John Gray McKendrick, F.R.S., illustrated.

Messrs. J. and A. Churchill:—"A Treatise on Hygiene, edited by Thomas Stephenson and Shirley F. Murphy, in two vols., with numerous illustrations, Vol. I. nearly ready; "Chemical Technology; or, Chemistry in its Applications to Arts and Manufactures," edited by Charles E. Groves, F.R.S., and William Thorp (with which is incorporated "Richardson and Watts' Chemical Technology"), Vol. II. "Lighting: Fats and Oils, Candles, Stearine, Gas, Electric Lighting"; "Materia Medica, Pharmacy, Pharmacology, and Therapeutics," by W. Hale White; "The Student's Guide to Diseases of the Nervous System," by J. A. Ormerod, with 66 illustrations; "A Dictionary of Psychological Medicine, giving the Definition, Etymology, and Synonyms of the Terms used in Medical Psychology, with the Symptoms, Pathology, and Treatment of the Recognized Forms of Mental Disorder, together with the Law of Lunacy in Great Britain and Ireland," in two vols., edited by D. Hack Tuke.

Messrs. Whittaker and Co.:—New volumes of the Specialists' Series—"Lightning Conductors and Guards," by Oliver J. Lodge, F.R.S., with numerous illustrations; "The Dynamo," by C. C. Hawkins and F. Wallis, with numerous original diagrams; "A Guide to Electric Lighting," by S. R. Bottone, for householders and amateurs, with 77 illustrations. Whittaker's Manual Instruction Series—"Manual Instruction: Woodwork," by S. Barter, Organizer and Instructor for the London School Board, and to the Joint Committee on Manual Training of the School Board for London, the City and Guilds of London Institute, and the Worshipful Company of Drapers, with over 300 illustrations; "Leather Work, Stamped, Moulded, and Cut, Cuir-Bouillé, Sewn, &c.," by Charles G. Leland, author of "Wood Carving," with numerous illustrations. Whittaker's Library of Popular Science—"Mineralogy," by Dr. F. Hatch, with numerous illustrations; "Chemistry," by T. Bolas, with many illustrations.

Messrs. Sampson Low and Co.:—"Answers to the Questions on Elementary Chemistry, Theoretical and Practical (Ordinary Course), set at the Examinations of the Science and Art Department, South Kensington, 1887 to 1891," by John Mills, two vols., fully illustrated; "Chemistry for Students, consisting of a Series of Lessons based on the Syllabus of the Science and Art Department, and specially designed to facilitate the experimental teaching of Elementary Chemistry in Schools and Evening Classes," by John Mills, numerous illustrations; "Decorative Electricity," by Mrs. J. E. H. Gordon, with a chapter on Fire Risks by J. E. H. Gordon, and numerous illustrations by Herbert Fell, engraved on wood by J. D. Cooper; "Examination of Soils," by W. T. Brannt.

Messrs. George Philip and Son:—"Makers of Modern Thought; or, Five Hundred Years' Struggle (A.D. 1200 to A.D. 1699) between Science, Ignorance, and Superstition," by David Nasmith, in two volumes; "Christopher Columbus," by Clements R. Markham, Vol. VII. of "The World's Great Explorers and Explorations"; "The Development of Africa," by Arthur Silva White, new and cheap edition, revised to date, with fourteen coloured maps; "Philips' General Atlas," entirely new and revised edition, with several additional maps; "Philips' Systematic Atlas," for higher schools and general use, a series of physical and political maps of all the countries of the world, with diagrams and illustrations of astronomy and physical geography, specially drawn by E. G. Ravenstein; "Philips' Atlas of Astronomy," a series of seventy-two plates, with notes and index by Sir Robert Stawell Ball, F.R.S., Royal Astronomer of Ireland; "Tourists' Handy Volume Atlas of Europe," a series of coloured maps, with notes, plans of cities, and complete consulting index, by J. G. Bartholomew.

Messrs. Swan Sonnenschein and Co.:—"Animal Colouration," by Frank Beddard, Prosector to the Zoological Society, with four coloured plates by P. J. Smit, and numerous woodcuts; "Text-book of Embryology: Man and Mammals," by Dr. Oscar Hertwig, of the University of Berlin, translated and edited from the third German edition by Dr. E. L. Mark, Professor of Anatomy in Harvard University, fully illustrated; "Text-book of Embryology: Invertebrates," by Drs. Korschelt and Heider, of the University of Berlin, translated and edited by Dr. E. L. Mark, Professor of Anatomy in Harvard University, and Dr. W. M. Woodworth, Assistant Professor in Harvard University, fully illustrated; "Text-book of Geology," adapted from the work of Dr. Kayser, Professor in the University of Marburg, by Philip Lake, of St. John's College, Cambridge, fully illustrated; "The Geographical Distribution of Disease in England and Wales," by Alfred Haviland, with several coloured maps; "A Treatise on Public Hygiene and its Applications in different European Countries," by Dr. Albert Palmerg, translated, and the English portion edited and revised, by Arthur Newsholme, fully illustrated; "The Photographer's Pocket-book," by Dr. E. Vogel. "Introductory Science Text-Books," additions—introductions to the study of "Zoology," by B. Lindsay, illustrated; "The Amphioxus," by Dr. B. Hatschek, of the University of Vienna, and James Tuckey, of the University of Durham, illustrated; "Geology," by Edward B. Aveling, Fellow of University College, London, illustrated; "Physiological Psychology," by Dr. Th. Ziehen, of the University of Jena, adapted by Dr. Otto Beyer, with twenty-two figures.

Messrs. Crosby Lockwood and Son:—"A Hand-book of Brewing, a Practical Treatise for the use of Brewers and their Pupils," by Herbert Edwards Wright; "A Treatise on Earthy and other Minerals and Mining," by the late D. C. Davies, third edition, revised and very considerably extended by his son, E. H. Davies; "Fuels: Solid, Liquid, and Gaseous, their Analysis and Valuation," for the use of chemists and engineers, by H. J. Phillips, second edition, revised and much enlarged.

SCIENTIFIC SERIALS.

THE most important article in the numbers of the *Journal of Botany* for January and February is one by the late Dr. A. Barclay on rust and mildews in India. He shows that the years in which the grain-crops were deficient have been those in which the climatal conditions were favourable to the growth of parasitic fungi. The chief enemy to wheat in India is *Puccinia rubigo-vera*; and it is an interesting fact that while the æcidioform of this Uredine occurs in Europe on Borraginaceous plants, no æcidium is known in India on any species of the order. Dr. Barclay believes that its life-history has a different course in India from that taken in Europe.—Mr. W. G. Smith reports the progress at present made in the commission which he has received from the Trustees of the British Museum to make a series of water-colour drawings of the whole of the British *Basidiomycetes* for the public gallery of the Department of Botany.—A series of papers is commenced in these numbers on the first records of British flowering plants, by Mr. W. A. Clarke.

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THE *Bullettino* of the Italian Botanical Society is now published apart from the *Nuovo Giornale*. The first number contains reports of the papers read at the annual meeting, held at Naples in August, and of the regular meetings held since till the end of the year, and of the discussions which followed. Among the more noteworthy papers may be mentioned the following:—On a new carpellary theory, by Signor F. Pasquale, who maintains that the carpel is not derived, as has been generally supposed, from the modification of a single leaf, but from the concrescence of two, or sometimes of three leaves, which unite in the formation and nutrition of the ovules and seeds.—On the floral structure and process of pollination in some species of *Nigella*, by Dr. A. Terracciano.—On the period of formation of the inflorescence within the bud of the vine, by Signor U. Martelli.—On the non-sexual propagation of *Cynomorium coccineum*, by the same writer, who has established its parasitism on *Atriplex nummularia*.—Prof. G. Arcangeli also describes the results of experiments on the cultivation of this plant, which he finds to be parasitic on many hosts.—On earthquakes and vegetation, by Signor A. Goiran. He finds the effects of seismic motions in the earth to be the more rapid germination of seeds, as well as a more rapid growth of the young plant.—Signor E. Tanfani has a paper on the teaching of botany in gymnasia, which he considers to be in a very backward state in Italy.

THE *Botanical Gazette* for January contains two interesting original papers:—Herr A. F. Foerste speaks of the relationship of autumn- to spring-blossoming plants, and concludes that late autumn-flowering plants may be divided into two classes—those which have developed from summer-flowering plants by the increase in the number of internodes with their appendages and the gradual retardation of growth, and those which have developed from spring-blossoming plants by the premature development of buds destined to flower during the ensuing spring.—Mr. H. L. Russell discourses on the effect of mechanical movement on the growth of certain plants. The experiments were made chiefly on certain yeast-fungi; and the general results were that the development of filaments was hindered by shaking; but that strong agitation greatly increases the activity of cell-division, while it diminishes the intensity of fermentation. This may be compared with the fact mentioned above relative to the effect of earthquakes on the growth of plants.

THE greater part of the number of the *Nuovo Giornale Botanico Italiano* for January is occupied by a paper by Signor A. Jatta, on the Lichens of Italy, accompanied by a very elaborate bibliography.—Signor C. Massolongo has a note on a floral monstrosity in *Jasminum grandiflorum*; and Dr. R. Cobelli a paper on the movements of the flower and fruit of *Erodium gruinum*. These movements belong to three organs—the calyx, the upper portion of the style, and the mericarp—and do not appear to be in any way connected with the pollination of the flower, since the species is apparently self-fertilized, and no pollinating insects were observed at any time upon it.

SOCIETIES AND ACADEMIES.

LONDON.

Physical Society, February 26.—Prof. W. E. Ayrton, F.R.S., Past President, in the chair.—Prof. S. P. Thompson, F.R.S., read a paper on modes of representing electromotive forces and currents in diagrams. The author said he had found it advantageous in some cases to depart from the usual methods of representation, and he now brought the subject before the Society in order to have it discussed and improvements suggested. To indicate the directions of currents in wires seen end-on, Mr. Swinburne had used circles with and without crosses, but no symbol had been suggested for wires not conveying currents. He (Prof. Thompson) thought the plain circle should be used for inactive wires. A circle with a dot in the middle could then be used to indicate that a current was flowing towards the observer, and a circle with a cross in it to represent a wire conveying a current away. These meanings could be recalled by considering the direction indicated by an arrow, the dot showing the tip of the arrow, and the cross the feathers. Some method of distinguishing between E.M.F. and current was required. For this he proposed to use thin-stemmed arrows with feathers for E.M.F.'s, and thick-stemmed ones without tails for currents. In the case of electrical transmission of energy, this convention had the important advantage that where the two arrows had the same direction, energy was being given to the system, and where the arrows were opposite, energy was leaving it. Mr. Maycock, he said, had recently published a simple rule for finding the direction of magnetic force due to a current of known direction in a wire. Grasp the wire with the right hand, the thumb pointing in the direction of the current; the fingers will then encircle the wire in the direction of the magnetic force. Dr. Fleming's well-known rule for induced currents was also a *right-hand* rule, but as it referred to the direction of *currents*, another rule was necessary when considering motors. By making the rule refer to E.M.F.'s, only one rule was required for generators and motors. For alternating currents the author found it convenient to draw polar curves analogous to Zeuner's valve diagrams. Suppose a line OP (Fig. 1), representing the maximum value of an E.M.F. or

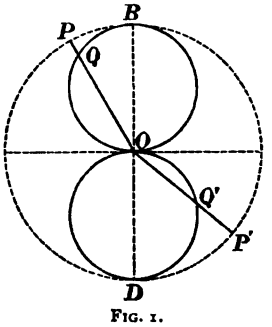

FIG. 1.

current whose magnitude is a sine function of the time, to revolve at uniform velocity about O; the intercepts OQ, OQ', &c., cut off by circles OQB, OQ'D, will represent the magnitudes at the times corresponding to the positions OP and OP'. The effect of lag can also be represented in such diagrams. In cases where the variables are not sine functions, the curves OQB and OQ'D are no longer circles. Polar diagrams representing the E.M.F. and current curves obtained by Prof. Ryan in his transformer experiments were exhibited, and a working diagram, illustrating the changes in three-phase currents, was shown. To show the directions of induced E.M.F.'s in diagrams of dynamos and motors, diagonal shading of the pole-faces was sometimes convenient; the lines over north poles being drawn from left to right downwards in the direction of the middle stroke of the letter N, and those over south poles from left to right upwards. A conductor passing over a north pole from left to right would have an E.M.F. induced in a downward direction, as indicated by the slope of the diagonal lines. This method of representation was used to show the ways of connecting up multipolar drum armatures, the winding being supposed cut along a generating line, unwrapped from the core, and laid out flat in the manner adopted by Fritsche. In connection with armatures, the author said a formula had been published by means of which the nature of a winding consisting of a given number of convolutions, and to be used with a given number of poles, could be predetermined. This, he thought, would be very useful in practice. Mr. Blakesley said the old method of representing alternate current magnitudes by means of the projections of revolving lines, seemed preferable, for it left no ambiguity as to the directions of the quantities. The method of shading the poles also required that the direction in which the diagram was to be viewed should be known before the direction of the E.M.F. could be determined. Mr. Swinburne suggested that the author might use a bow to represent E.M.F., and an arrow for current. He was glad to see that Prof. Thompson recognized the differences in dynamos and motors, and approved of the view that mnemonic rules should refer to E.M.F. and not to current. The diagrams of drum windings would be very useful, and he hoped the author would make the subject clear to ordinary workmen in the next edition of "Dynamo-Electric Machinery." Prof. Perry considered it undesirable to use polar curves for anything but circles. In his opinion it was not sufficiently known that any curve can be split up into a series of sine curves, and each component dealt with separately; the separate results being added together in the end. Mr. Swinburne pointed out that before one could analyze a curve in this way, the curve must be known, and would probably have to be determined ex-

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perimentally. If means for finding one curve are available, any other required curve could probably be found by the same apparatus; hence there was no need for analysis. Prof. Perry remarked that experiments could not be made on a machine before the machine was built; whereas the E.M.F. curve could be predetermined from its design. By analysis, its current curves, when working under various conditions, could be found. Prof. Ayrton, referring to the mnemonic character of the modes of representation described by Dr. Thompson, suggested that the symbols in the author's book should be more mnemonic. He himself was in the habit of using large letters for currents and small ones for resistances: A and a for the armature, S and s for series, and Z and z for the shunt, currents and resistances, respectively, and σ and ζ for the series and shunt turns. He also found the following "electromotive force" rule very convenient. Draw three rectangular axes, OM, OF, and OE, as shown in Fig. 2. If, then, OF represents the direction of the

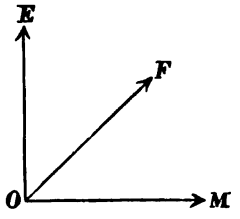

FIG. 2.

force (magnetic), OM that of the motion, then OE shows the direction of the induced E.M.F. Dr. Thompson, in replying, said he thought Mr. Blakesley had misunderstood what had been said, for no ambiguity existed. In describing the windings of armatures, difficulty arose from want of proper names for the various elements, and in his forthcoming work suitable names had been given. To Prof. Ayrton he pointed out that in his book he (Dr. Thompson) had used mnemonic characters, for r_a, r_s, and r_{sm} represented the resistances of armature, shunt, and series magnet coils respectively. The symbol I for current had also been recommended for adoption by the Frankfort Committee. He objected to Greek letters except for specific quantities, such as angles, specific inductive capacities, refractive indices, &c. He appreciated the simplicity of Prof. Ayrton's E.M.F. rule, but thought it would be better to rotate OE and OF through a right angle about OM, thus giving Fig. 3.—A paper on

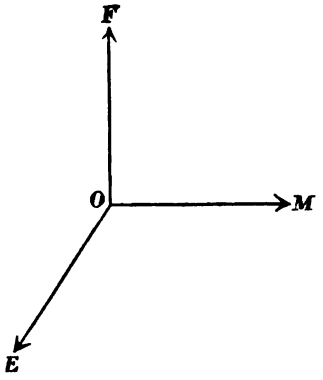

FIG. 3.

the flexure of long pillars under their own weight, by Prof. M. Fitzgerald, was read by Mr. Blakesley. The subject of upright pillars fixed at the base and free at the top is treated mathematically, the differential equation being integrated in two series, involving ascending powers of the variable. Putting L for the ratio of length to diameter, the results, when applied to thin steel tubes and rods, for which Young's modulus is taken as 12,000 tons per square inch, show that the limiting height (in feet) of pillars which can stand without bending is given by $H = \frac{15 \times 10^6}{L^2}$ for tubes; and $H = \frac{7{\cdot}5 \times 10^6}{L^2}$ for rods. If L = 100, the maximum height of a tube is 1500 feet, the diameter being 15 feet. For wires, L may have larger values, and the limiting length of a No. 28 B.W.A. steel wire, is about 10 feet. In the case of pillars whose neutral axes are constrained to be vertical both at top and bottom, the results show that a definite ratio must exist between the bending moments producing the constraints.—A paper on choking coils, by Prof. Perry, F.R.S., and a description of the uses of Rice's choking coils for regulating the brilliancy of incandescent lamps, by Mr. Hammer, were postponed until next meeting.

Linnean Society, March 3.—Prof. Stewart, President, in the chair.—A letter was read from the Home Secretary, conveying the thanks of Her Majesty the Queen for the address of condolence which had been forwarded on behalf of the Society on the death of H.R.H. the Duke of Clarence and Avondale.—The President announced the presentation by Sir Joseph Hooker to the Society of two medallion portraits of Sir James Ross and Dr. John Richardson, whose names are well known in connection with Arctic exploration. The medallions were executed in 1843 by the late Bernhard Smith. A vote of thanks to the donor was passed unanimously.—Mr. Clement Reid exhibited a collection of fossil plants and seeds which he had found associated with the bones of Rhinoceros and other mammals in the neighbourhood of Selsea, and West Wittering. By means of diagrams Mr. Reid showed the exact position of the bed, and described the condition in which the various specimens were deposited.—On behalf of Mr. W. E. Beckwith, of Shrewsbury, Mr. H. Seebohm exhibited a specimen of White's Thrush (*Turdus varius*) which had been shot near Shrewsbury on January 14 last. He pointed out that this species, which inhabits Eastern Asia, belongs to the sub-genus *Oreocincla*, an exclusively Eastern group of ground Thrushes, and is the only one which is Palæarctic and migratory. It does not breed anywhere west of the Yenisei, and its occurrence in Europe is accidental. Mr. Seebohm added that it had been met with twice in France, four times in Italy, three times in Belgium, once or twice in Austria and Prussia, once in Norway, thirteen times in Heligoland (between 1827 and 1884), and about a score of times in the British Islands, including three occurrences in Ireland, and one in the extreme south of Scotland.—On behalf of Mr. A. Craig Christie, the Secretary exhibited some specimens, as was supposed, of *Lycopodium complanatum*, collected in Scotland, on which it was suggested that the plant might be regarded as British. In the opinion, however, of Mr. James Groves, who had carefully examined the specimens, and other botanists present, they were referable to *L. alpinum*. Mr. Groves pointed out the distinctive characters of both. Mr. Carruthers was of opinion that *L. complanatum* had been met with in the south of England, but not within the last ten years. Mr. E. M. Holmes was under the impression he had seen it growing a few years ago near Stroud.—A paper was then read by Mr. A. D. Michael, on variations in the internal anatomy, and especially the genital organs, of the *Gamasinæ*, a typical sub-family of the Acari. In this paper the author gave the results of two years' research, including many hundreds of dissections and serial sections, with lengthy observations of the living creatures. The comparison of variable organs was worked out in numerous species, showing great specific differences. Four of the species were found to be previously undescribed, and for these the names *Hæmogamasus horridus*, *H. nidi*, *Lælaps oribatoides*, and *L. ligoniformis* were proposed.

Royal Microscopical Society, February 17.—Dr. R. Braithwaite, President, in the chair.—Prof. F. Jeffrey Bell said that he had, in accordance with the resolution passed at the last meeting, forwarded a copy of the message of condolence from the Society to the Prince of Wales, to General Sir Dighton Probyn, and he had received the following letter of acknowledgment:—"*Sandringham, Norfolk.*—General Sir Dighton Probyn, Comptroller and Treasurer of the Household, is desired to convey to the members of the 'Royal Microscopical Society' the heartfelt thanks of the Prince and Princess of Wales for the Society's kind resolution, expressing sympathy for their Royal Highnesses in their deep affliction.—*January* 25, 1891."—Mr. Watson exhibited and described a new vertical camera for photomicrography designed upon the same lines as that used by Dr. Van Heurck.—The President then read his annual address, postponed from the last meeting under the special circumstances then mentioned. The subject chosen was the impregnation and modes of reproduction in Ferns and Mosses; diagrams in illustration were exhibited and explained, and specimens were also shown under microscopes.—A cordial

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vote of thanks, proposed by the Rev. Canon Carr and seconded by Prof. Groves, was given to the President for his valuable address.—Mr. J. J. Vezey moved that the best thanks of the Society be given to its officers, and also to the auditors and scrutineers for their services during the year.—The President declared the motion to be carried by acclamation.—Prof. Bell thanked the Society on behalf of himself and the other officers, at the same time calling attention to the special services rendered by the Treasurer, Mr. Frank Crisp.—The following are the names of the members of the new Council, who met for the first time at this meeting:—President: Dr. R. Braithwaite. Vice-Presidents: Mr. A. W. Bennett, Prof. J. W. Groves, Mr. G. C. Karop, and Mr. A. D. Michael. Treasurer: Mr. Frank Crisp. Secretaries: Prof. F. Jeffrey Bell and the Rev. Dr. W. H. Dallinger. Ordinary members: Dr. Lionel S. Beale, Rev. E. Carr, Mr. James Glaisher, Dr. R. G. Hebb, Mr. E. M. Nelson, Mr. T. H. Powell, Prof. Urban Pritchard, Mr. W. W. Reeves, Prof. C. Stewart, Mr. W. T. Suffolk, Mr. C. Tyler, and Mr. F. H. Ward.

PARIS.

Academy of Sciences, March 7.—M. d'Abbadie in the chair.—Fermentation of blood, by MM. Berthelot and G. André. The blood of cattle, defibrinated, was fermented for 130 days in a water-bath at 35°. The paper contains an account of the products of the fermentation. It will be published in greater detail in the *Annales de Chimie et de Physique*.—On the distribution in latitude of solar phenomena observed at the Royal Observatory of the Roman College during the second half of 1891, by M. P. Tacchini. (See Our Astronomical Column.)—Phenomena observed at Kalocsa, on the large group of spots of February 1892, by M. J. Fényi. A prominence, 124″ high, was observed in the position 220°–230°, as the recent large spot-group was crossing the limb.—On the impossibility of certain movements, by MM. A. de Saint-Germain and L. Lecornu.—On the movement of a conical pendulum, by M. de Sparre.—On electro-capillary phenomena, by M. Alphonse Berget.—On the co-existence of dielectric power and electrolytic conductivity, by M. E. Bouty. It appears from the experiments described that the dielectric constant only varies slightly under conditions which produce an enormous increase of conductivity. Thus, water and ice have sensibly the same dielectric constant, whilst the conductivity may vary from 1 to 10^5 or 10^6.—On the thermal conductivity of crystalline bodies, by M. Charles Soret.—Rule for finding the number and nature of accidentals of the gamut in a tone and a given mode, by M. Pierre Lefebvre.—On the density of aqueous solutions, by M. Georges Charpy. The author concludes from his results that the variation of the density of a solution, as a function of the concentration, is a complex phenomenon, and cannot be used in studying the state of the dissolved body. There is no reason why the solution at which the maximum density is reached should be regarded as corresponding to a definite hydrate.—Compounds of gaseous ammonia with boron iodide and bromide, by M. A. Besson.—Synthesis of the minerals crocoisite and phœnicite (*phornicochroïte*), by M. C. Luedeking.—On the value of the primary alcoholic function, by M. de Forcrand.—On the production of quinine di-iodomethoxide from cupreïne, by MM. E. Grimaux and A. Arnaud.—A study of the velocity of decomposition of diazo-compounds by water, by MM. P. Th. Muller and J. Hausser. The law according to which sulphanilic acid is decomposed is expressed by the formula $C = \frac{1}{\theta} \log. \text{nat.} \frac{A}{A - x}$, in which θ is the time, C a constant, A the total nitrogen that can be evolved, and x the amount of nitrogen evolved. C is independent of the concentration.—Action of capryl iodide on trimethylamine in aqueous solution, in equimolecular proportions; formation, when heated, of dimethylcaprylamine; production in the cold of caprylene, by MM. H. and A. Malbot.—New synthesis of tartaric acid, by M. P. Genvresse. (See Notes.)—On the pyloric secretion of the dog, by M. Ch. Contejean.—New rings or intercalary rings of nerve-ducts (*tubes nerveux*), produced by the impregnation of silver, by M. Benjamin Ségall.—On two new species of *Streptothrix*, Cohn, and on the place of this genus in the classification, by MM. C. Sauvageau and M. Radais.—History of the *Garcinia* of the sub-group *Xanthochymus*, by M. J. Vesque.—On the magnetic disturbance and the aurora borealis of March 6, 1892, by M. Th. Moureaux. Disturbances similar to those of February 13–14, but less violent, were registered by the Parc Saint-Maur instruments on March 6–7.—On the magnetic storm of February 13–14, by M. H. Wild. A comparison of the records made by instruments at Pawlowsk with those obtained at Parc Saint-Maur shows that, although the recent magnetic storm commenced at approximately the same time, the variations at the two places were in the opposite directions. Other differences have been observed in the two records.—On the atmospheric, magnetic, and seismic disturbances of February 1892, by M. Ch. V. Zenger. The author has marshalled facts to show that magnetic storms, cyclones, snow-storms, discharges of atmospheric electricity, earthquakes, and volcanic eruptions occur simultaneously.—On three fossil human skeletons found in the Baousse-Roussé grottos, in Italy, by M. Émile Rivière.

BOOKS RECEIVED.

BOOKS.—Phases of Animal Life: R. Lydekker (Longmans).—Meteorological Observations made at the Adelaide Observatory, &c., during the Year 1889 (Adelaide).—The Rationale of Mesmerism: A. P. Sinnett (Kegan Paul).—Zoological Record, 1890 (Gurney and Jackson).—Elements of Economics of Industry: Prof. A. Marshall (Macmillan).—Elementary Mathematical Astronomy: C. W. C. Barlow and G. H. Bryan (Clive).—The Dietetic Value of Bread: J. Goodfellow (Macmillan).—Air and Water: Prof. V. B. Lewes (Methuen).—Le Climat de la Belgique, 1891: A. Lancaster (Bruxelles, Hayez).—Soils and Manures: Dr. J. M. H. Munro (Cassell).—Longmans' School Geography for North America: G. G. Chisholm and C. H. Leete, 2nd edition (Longmans).—Precious Stones and Gems: E. W. Streeter, 5th edition (Bell).—The Oak: Prof. H. M. Ward (Kegan Paul).—The Labrador Coast: Dr. A. S. Packard (Kegan Paul).—Contribution à l'Étude de la Morphologie et du Developpement des Bactériacées: A. Billet (Dulau).—Bateaux et Navires: Marquis de Folin (Paris, Baillière).—Diseases of the Nose: Dr. G. Macdonald, 2nd edition (Watt).—Annals of the Royal Botanic Garden, Calcutta, vol. iii. (Calcutta).—A Short Text-book of Inorganic Chemistry: Dr. H. Kolbe, translated and edited by Dr. T. S. Humpidge, 3rd edition (Longmans).—Laboratory Practice: Dr. J. P. Cooke (Kegan Paul).—Catalogue of the Specimens illustrating the Osteology of Vertebrated Animals recent and extinct, contained in the Museum of the Royal College of Surgeons of England, Part 3, Class Aves: Dr. R. B. Sharpe (Taylor and Francis).

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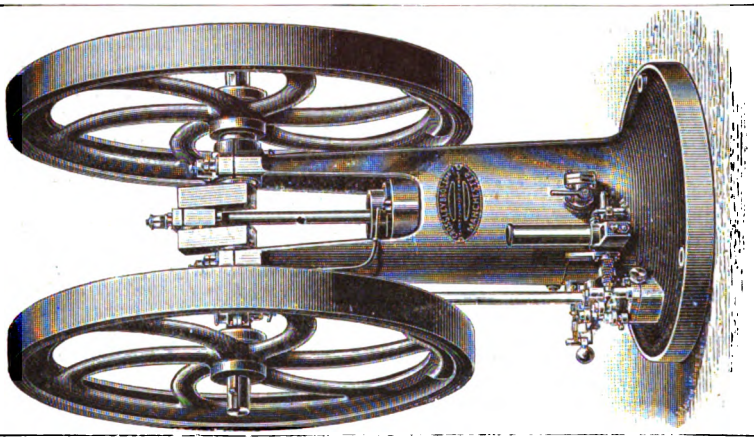

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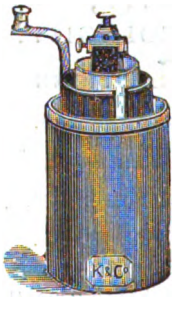

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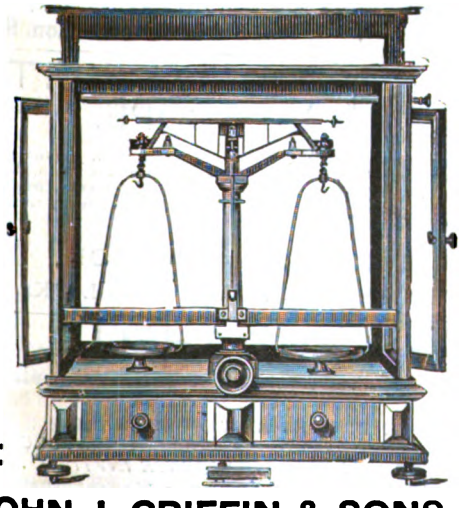

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THE HISTORY OF DETERMINANTS.

The Theory of Determinants in the Historical Order of its Development. Part I. Determinants in General. By Thomas Muir, M.A., LL.D., F.R.S.E. (London: Macmillan and Co., 1890.)

THE theory of determinants is in that borderland which separates the "pass" from the "honour" student of pure mathematics. In elementary text-books the subject is rarely more than introduced for the purpose of representing some result of geometry or analysis in a convenient, beautiful, and suggestive form. The essential properties of a determinant are not set forth, but the student is perhaps referred for further information to one or other of the two excellent treatises which are already at our disposal in the English language, viz. those of Mr. Muir and of Mr. R. F. Scott. The value of the idea thus given to a student of the shape and convenient use of a great analytical implement is beyond all question. His imagination and curiosity are alike excited, and the "trick" possibly prevents his passage through life under the delusion that all mathematics are comprised within the covers of the school-books. The honour student, as a matter of course, reads some work on the subject, and is as surely enchanted. He cannot fail to recognize the power and beauty of the notation. He observes that the object of his study is constructive in its nature. He becomes convinced that pure mathematics is one of the fine arts, and just as a beautiful picture gives pleasure to one who understands painting, just as a fine piece of sculpture delights one who understands modelling, so he sees unfolded to his intellectual eye an exquisite example of constructive art, which his previous mathematical reading has fitted him to understand and appreciate, and to regard as a beautiful object of contemplation. The theory of determinants is one of the most artistic subdivisions of mathematical science, and accordingly has never wanted enthusiastic admirers. It is most gratifying to find such an authority as Mr. Muir devoting his leisure to its historical development. Any mathematician taking up this volume would anticipate a treat, and he would not be disappointed. In this first instalment the reader is taken from Leibnitz (1693) to Cauchy (1841). Mr. Muir assigns a chief place of honour to Vandermonde (1771), who, in his "Mémoire sur l'Élimination" (Hist. de l'Acad. Roy. des Sciences), denoted a function formed from the coefficients of a set of linear equations by a symbolism which is at once recognized as a condensed form of the determinant matrix of the present day. He was the first to give a connected exposition of the theory, and to give the true fundamental properties of the new functions. His notation, moreover, was exceedingly good, and much superior to that adopted by some subsequent writers who overlooked or neglected his important work.

Vandermonde has also recently received justice, long denied him, in other branches of analysis, and there is now no doubt that the value and originality of his work entitle him to a place in the ranks of the mathematical pioneers of his time. Up to the close of the eighteenth century the most noteworthy additions were made by Laplace, Lagrange, and Bezont. We find that Lagrange knew that the discriminant of a binary quantic of the second order is an invariant of the linear transformation. He did not, however, express either the discriminant or the determinant of transformation in a determinant form. The author critically examines the claim of Laplace to be the discoverer of the expansion theorem. He finds that although the case in which as many as possible of the factors of the terms of the expansion are of the second degree had already been given by Vandermonde, and Laplace himself did not give a statement of the rule suited for general application, the claim can in the main be upheld. Hindenburg (1784) and Rothe (1800) took up the subject in Germany, and between them constructed an elementary theory of permutations. Rothe, it is interesting to observe, employed a graphical method which will remind the reader of Prof. Sylvester's modern constructive theory of partitions. Gauss (1801) followed, and then we come to the important papers of Binet (1811) and Cauchy (1812). These memoirs establish the multiplication theorem in its full generalization. The method adopted by Binet may be described as that of symmetric functions, which he uses freely. He employs identities of the type

$$\Sigma ab'c'' = \Sigma a \Sigma b \Sigma c + 2\Sigma abc - \Sigma a \Sigma bc - \Sigma b \Sigma ca - \Sigma c \Sigma ab,$$

having reference to several systems of quantities. He was not, however, sufficiently acquainted with the theory of such functions; and was unable to supply rigid proofs of the theorems in determinants which, from his point of view, rested upon these identities. Nowadays, the identity in question will be recognized as the expression of an "elementary" symmetric function (single unitary, and having three parts in its partition) by means of symmetric functions each of which is expressible symbolically by a partition composed of one tripartite part. The law of the coefficients, undivulged by Binet, is now perfectly well known. It is, in fact, an easy generalization of the law by which, in the case of a single system of quantities, the elementary symmetric functions are expressed as functions of the sums of powers of the quantities. Cauchy at the same date (1812) introduced the idea of "fonctions symétriques alternées," which led him to a new symbolic definition of a determinant and to many valuable results. Mr. Muir devotes several pages to an examination of Cauchy's title to share with Binet the credit of the generalized multiplication theorem. He gives his decision against Cauchy, and probably the reader who closely follows the argument will find himself in accord with the historian. Notwithstanding this conclusion, Cauchy's memoir is excellent in quality and abundant in quantity; he "opened up a whole avenue of fresh investigation, and one cannot but assign to him the place of highest honour among all the workers from 1693 to 1812."

A retrospect is given of the period 1693–1812 accompanied by an interesting tabular record. As a means of reference the work appears to be absolutely perfect. Each new result as it appears is marked in Roman figures; and if the same result be obtained differently, or be generalized by a subsequent investigator, the same Roman figure is employed, followed by an Arabic numeral. It is found that to this point nearly every important advance is due to French mathematicians.

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During the second period (1813–1841) the chief names are Cauchy, Schweins, Jacobi, and Sylvester; to these may be added Desnanot and Scherk, to whom fresh departures, of less extent, are due. Schweins himself may be said to have been discovered by Mr. Muir. This author (1825) deals with the subject under the title "Theorie der Producte mit Versetzungen." He made a notable generalization described as the transformation of an aggregate of products of pairs of determinants into another aggregate of similar kind. He further discussed special forms, and, it is clear, possessed a firm grasp of his subject.

The work of Jacobi and Sylvester, and the further work of Cauchy will be more or less familiar to mathematicians. Germany has passed to the first place; and the occurrence of Sylvester's name in the history marks a revival of learning in pure mathematics in England.

The volume is remarkable for the study it presents in nomenclature and notation. There is an extraordinary variety in the symbolism. It is easy to observe the distinctive characters of French and German notation that are so marked at the present day. It is well known that much lies in an appropriate notation. Every young mathematician with a predilection for original work should read this book, to note the power of suitable symbols, to grasp the reason of their power, and, above all, to see what to avoid.

The book also points a moral which is not far to seek. It would be easy to pick out many such phrases as, "was acquainted with the writings of very few of his predecessors"; "was unaware apparently of the existence of a theory of determinants"; "hasty, if not contemptuous, disregard of historical research." To this tendency to work on without proper attention to previous work is doubtless due in some measure the unfortunate multiplication of names and symbolisms which is so perplexing and irritating to a reader. This failure to collaborate with others can only retard the progress of the science. It is perfectly true that great original thinkers, like Gauss, Cauchy, Jacobi, and Sylvester, may take liberties of this kind; and the fact of their doing so may even be beneficial to the subject, as resulting in memoirs of more unfettered originality. But this is not so in the case of lesser men. In taking leave of this fascinating history one can look forward to Part II. with sincere pleasure, which is not diminished by the knowledge that the later developments have been largely due to our countrymen. We have yet to see Sylvester's most powerful investigations, and all Cayley's researches; and, finally, the successive steps by which the lofty heights of the theory of matrices and the theory of multiple algebra (involving the generalization of quaternions) have been attained. P. A. M.

THE EVOLUTION OF MAN.

Anthropogenie, oder Entwickelungsgeschichte des Menschen. By Prof. Ernst Haeckel. Fourth Edition, revised and enlarged. Pp. i.–xxvi. and 1–906. (Leipzig: W. Engelmann, 1891.)

THE importance of the subject-matter of the book, the length of time that has elapsed since the appearance of the former editions, and the prominent position held by the author, seem to call for more detailed notice of this work than is usually accorded to a new edition.

The "Anthropogenie," which was first published in 1874, is the third of the series of books in which Prof. Haeckel has attempted to determine the laws governing the form, structure, and mutual relations of living things, and to establish the general principles of biological science. The first of these, under the title "Generelle Morphologie," appeared in 1866, almost simultaneously with the completion of Herbert Spencer's "Principles of Biology." It is a comprehensive and ambitious work, which, in its author's words,

"constituted the first attempt to apply the general doctrine of development to the whole range of organic morphology, . . . and to introduce the Darwinian theory of descent into the systematic classification of animals and plants, and to found a 'natural system' on the basis of genealogy; that is, to construct hypothetical pedigrees for the various species of organisms."

It contains also the first systematic attempt to deal in detail with the ancestry of man, as regards the groups of animals lower than mammals. This is, perhaps, the most solid piece of work Prof. Haeckel has done; it contains much matter of great value, and discussions and speculations of extreme ingenuity and interest. Later discoveries have rendered much of it obsolete, but it still remains the most important work of its kind; and but for its somewhat cumbersome terminology, would be widely read even now. The "Natural History of Creation," first published in 1868, goes over a good deal of the same ground as the earlier work, but is written in a much more popular style, and aims at presenting in a form suited to the general reader the main arguments on which the Darwinian theory is based, together with a detailed application of the theory to the principal groups of animals, and an attempt to determine their mutual relations and lines of descent. The "Anthropogenie," the book now before us, is a more elaborate application of the same principles to the special problem of the evolution of man.

In the new edition the general arrangement remains much the same as before; but, in order to include the results of more recent investigations, a great part of the book has been re-written, and new chapters have been added on subjects that have attracted especial attention of late years, such as the Gastræa theory, the Cœlom theory, and the nature and origin of segmentation. A large number of new figures have been inserted, and the genealogical tables, for which Prof. Haeckel has a special fondness, have been greatly increased in number, and in elaboration of detail.

The book, which is adapted rather for the general reader than the scientific student, is written in an attractive and popular style, and presents the main facts of vertebrate embryology in an intelligible and well illustrated form. As might be expected from his former writings, the main feature of Prof. Haeckel's work is a detailed exposition and vigorous defence of what he has named "the fundamental law of biogenesis," better known in this country as the recapitulation theory, according to which the actual or ontogenetic development of a animal is a repetition of the ancestral or phylogenetic

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development of the species; or, to put it more simply, animals in their development climb up their own genealogical trees. This is now generally accepted by embryologists, but it has not always been so, and Haeckel reminds us, with justice, that he was one of the first to realize and teach the doctrine.

Prof. Haeckel has much to say on other points of theoretical interest. He protests strongly against Weismann's views with regard to heredity, pointing out that the very existence of germ-plasma is as yet a mere assumption, and maintaining that acquired characters may be and are actually transmitted. He objects equally strongly to the views as to the widespread occurrence of degeneration, which were first put forward by Dr. Dohrn; and on the much-debated question of the origin of Vertebrates he sides with those who fully accept the evidence afforded by the anatomy and development of Amphioxus; he admits the Vertebrate affinities of Balanoglossus, and looks for the ancestors of Vertebrates among the unsegmented Turbellarian worms. As a controversialist Prof. Haeckel is impressive rather than convincing. He hits hard and with effect, but prefers to counter rather than to parry the blows of his opponent. It is impossible to pass over without protest the terms in which he writes—it must be admitted under provocation—of the opinions and work of other investigators.

Prof. Haeckel's fondness for genealogical trees, and his facility in constructing them, are well known and have been much criticized, perhaps a little unfairly. Acceptance of the doctrine of evolution involves the recognition of a blood-relationship, near or remote, between any one animal and any other; and the only true classification is one which places this fact in the forefront, and adopts it as the basis on which the scheme is to rest. Genealogical tables undoubtedly stimulate inquiry, and so long as it is realized that they are necessarily in great part tentative or provisional, they probably will do more good than harm. It would be easy to take exception to many points in Prof. Haeckel's numerous and elaborate pedigrees, but it will be generally admitted that they are instructive, and often extremely suggestive, even though the conclusions may not meet with general acceptance.

The least satisfactory part of the book is that which deals with human embryology. No attempt whatever is made to explain the earlier stages of development; the special difficulties of the problem are absolutely ignored; the human gastrula is spoken of in a confident way, as though such a stage really existed; and the accounts of the development of the several organs and systems are too often taken from other animals. A student who relied on Prof. Haeckel's descriptions would obtain an entirely erroneous idea of the actual course of development of the human embryo.

Owing to the difficulty of obtaining material in proper condition for microscopical examination, our acquaintance with human embryology long remained imperfect; and the descriptions in text-books were largely based on our knowledge of other Vertebrates, and illustrated by figures from embryos of dogs, pigs, rabbits, or even chicken and dogfish. The time for all this is now past. During the last ten years our knowledge has advanced wonderfully; and although the earliest stages are still unknown, it is not too much to say that our knowledge of the development of the human embryo, from a stage corresponding to a chick embryo at the commencement of the second day onwards, is as satisfactory, as complete, and as well illustrated as that of any other mammal.

For this great advance we are indebted almost entirely to the labours of German embryologists, and notably to the splendid work of Prof. His. Prof. Haeckel has in his volume many hard things to say of Prof. His, but is indebted to him for the only really good figures of human embryos which he gives, and would have materially improved his book had he studied more carefully the admirable descriptions of the Leipzig Professor. It is a matter for great regret that a book of 900 pages, having for its title, "Anthropogenie, oder Entwickelungsgeschichte des Menschen," should be allowed to appear in which the account of the actual development of the human embryo is so inadequate or even erroneous.

A. M. M.

OUR BOOK SHELF.

Philosophical Notes on Botanical Subjects. By E. Bonavia, M.D. With 160 Illustrations. 368 pages. (London: Eyre and Spottiswoode, 1892.)

DR. BONAVIA'S philosophy is concerned with the evolution of vegetable organisms, and the gist of it is that all land-plants have descended from sea-weeds. He sees modifications and traces developments not obvious to ordinary observers, and he is prepared for derisive criticism. To quote from his preface:—"The fact is that, in this stage of existence, certain thoughts are often a great worry. One often cannot get rid of them. They turn up by day, they turn up by night, they turn up in the morning, and they haunt one at all times, and the only remedy for mitigating this worry of civilization is to commit them to paper. This done, there are several ways of disposing of your written thoughts. You can burn the papers they are written upon or otherwise destroy them, or you can leave them in a drawer as a legacy to your heirs! If by neither of these processes can you entirely give yourself repose, then the most effective way of ridding yourself of the worry of such thoughts is to have them published (if any publisher will perform this kind office), and to see them adversely criticized if anyone will even take so much trouble."

As the book before us testifies, Dr. Bonavia's worry reached the acute stage, and he is so far relieved as to have found a publisher; but we do not propose to gratify him by adverse criticism. We prefer giving one short extract from his sixteen "general conclusions":—

"Fifteenth:—The fig is obviously a further development of a conceptacle of a *Fucus* or other sea-weed. And there is every reason to believe that the oil-glands of the bark, leaves, and peel of the *Citrus*, and similar glands in other plants, are mere remnants of sea-weed conceptacles—that is, persistent features turned to other uses."

W. B. H.

The Zoological Record for 1890. Edited by Frank E. Beddard. (London: Gurney and Jackson, 1892.)

THIS is the twenty-seventh volume of the Record of Zoological Literature, and it has been prepared on the same plan as the volume published last year. First of all there is a list of works on general subjects, by J. A. Thomson. Then come the titles of writings on the following:—Mammalia, by R. Lydekker; Aves, by R. Bowdler Sharpe; Reptilia, Batrachia, and Pisces, by G. A. Boulenger; Tunicata, by W. A. Herdman; Mollusca, Brachiopoda, and Polyzoa, by P. C. Mitchell;

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Crustacea, by C. Warburton; Arachnida and Myriopoda, by R. Innes Pocock; Insecta, by D. Sharp; Echinodermata, by E. A. Minchin; Vermes, by P. C. Mitchell; Cœlenterata, by S. J. Hickson; Spongiæ, by E. A. Minchin; Protozoa, by C. Warburton. The utmost pains have been taken to make the lists complete and accurate, and to students of zoology they are practically indispensable. In the introduction to Mammalia, Mr. Lydekker notes that the number of new recent species is extraordinarily large. He adds, however, that this is "due to the elevation to specific rank of a host of North American forms which would be regarded by most zoologists as varieties." No fewer than forty of the "new species" belong to this category.

LETTERS TO THE EDITOR.

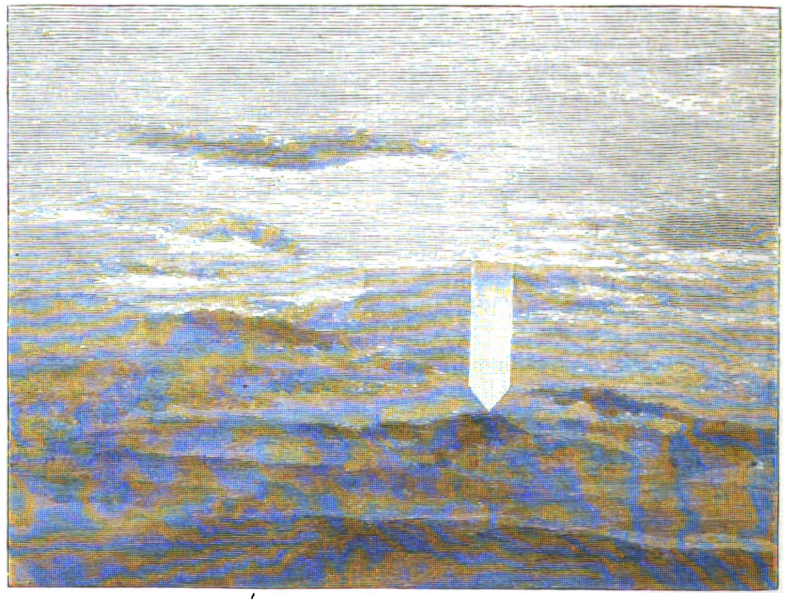
[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Sun Pillar.

A REMARKABLY well-defined instance of this phenomenon was seen by me at this place (460 feet above mean sea-level) this afternoon. At 5.32 p.m. the sun was sinking behind a thick layer of stratus cloud. There was a bank of dust haze, so defined as almost to resemble cirrus, which apparently formed a background to the clouds. When the phenomenon was first noticed, about three-quarters of the sun's disk was below the edge of the cloud bank; and from the centre of that portion of the disk visible there rose a tall column of brilliant light, extending upwards to about 5°, of the same width as the apparent diameter of the sun, and narrowing almost to a point as it touched the sun's rim. This convergence became more marked as the rest of the disk disappeared, until at the point at which the latter was finally lost to sight the apex appeared to rest on the edge of the cloud bank. The cone-shaped part at the base of the pillar was the most luminous portion, and glowed with a brilliant orange-red tint, which gradually merged into the yellow-white of the upper part of the column. The effect lasted for some minutes after the sun's disappearance, but the pillar lost its conical base and became less defined, while the clouds receding gave the appearance of the base of the pillar having risen in the sky.

I had noticed, a quarter of an hour previously, that the rays of the sun, when behind a cloud, stood out in an unusually solid and clearly defined manner. There was a good deal of anticyclonic stratus (about 5000 feet) at the time, and the upper part of the atmosphere was more hazy than is usual with a north-east wind at this period of the year. At the earth's surface the wind had dropped to an almost perfect calm.

Lutterworth, March 5. ANNIE LEY.



Sun pillar observed at Lutterworth.

New Comet.

THE comet discovered here on the evening of Friday, March 18, is extremely small, though not very faint, and it has a decided central condensation or nucleus. Its position at about 8h. 30m., March 18, was roughly determined as R.A. 341°, Decl. N. 59°. The comet was therefore situated in Cepheus, and about 3° east-north-east of the star Delta in that constellation.

On March 19, at 8h., I reobserved the comet, and found its rate and direction of motion to be 47′ of arc east, and 12′ north. It will therefore shortly traverse Cassiopeia.

The comet was discovered with a 10-inch reflecting telescope, with eye-piece magnifying 40 times, and having a field of 65′ of arc. W. F. DENNING.

Bristol, March 21.

[This is stated to be Winnecke's comet.—ED.]

First Visible Colour of Incandescent Iron.

DURING the discussion which followed the reading of the paper on "Colour Photometry" by Captain Abney and General Festing at the Royal Society on January 28, some interesting remarks were made by Lord Rayleigh as to the colour exhibited by heated iron when raised to such a temperature as only to be just visible in a dark room.

Lord Rayleigh stated that Weber, who, so far as I know, first drew attention to this subject, described the first visible light as a greenish-grey. Lord Rayleigh himself repeated the experiment by making a piece of thin iron part of the wall of a very dark room, and heating the iron gradually by a Bunsen burner upon the other side. Lord Rayleigh could not satisfy himself as to the greenish tint, but was satisfied that no redness was apparent.

It struck me that a very convenient method of trying this experiment would be to introduce a round bar of heated iron into a thin sleeve, as shown in the annexed sketch, the sleeve being closed with a cover lined with asbestos. In this way the heat would slowly penetrate the sleeve, and the observers could note the first appearance of visibility and the changes of colour that followed.

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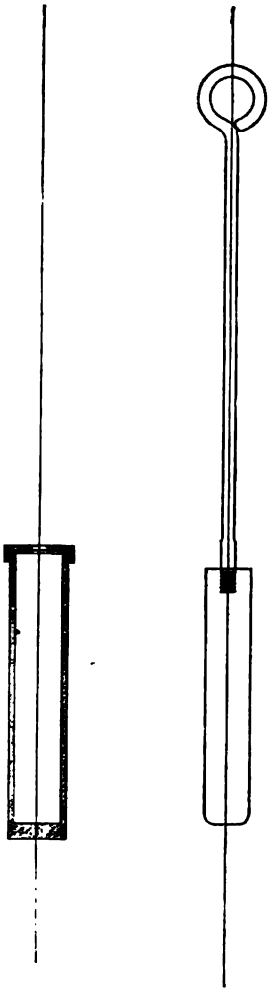
I accordingly had two sleeves prepared, one of turned and polished iron, the other left with a thick coating of oxide. Two sets of experiments, in each of which six observers took part, were made. In each set of experiments three observations were made with the polished, and three with the oxidized sleeve. In each case the observers were in the dark room for some minutes before the experiments began.

In the first set of experiments the observers gave their opinion, at the conclusion of the experiments, as a body, that the first appearance of colour was a greyish-white; as the sleeve became hotter the colour was yellow, and gradually changed into orange. There was little or no difference between the observers as to the instant of visibility; it was generally over a minute before the sleeve became visible, the light generally showing first on the generating line of the cylinder between the eye and the axis.



There was no difference in colour between the bright and the oxidized sleeves.

In the second set of experiments, the observers had no communication with one another, had no idea what colour they were expected to see, and their impressions were written down separately and independently. Their impressions were as follow, the observers being designated by α, β, &c. :—

(α) First colour visible, grey white, second colour white with a little mauve, third pale rose, fourth orange. The above was the first experiment (polished metal). The other experiments showed same colour, but no mauve seen. In the last experiment (a very low heat) the colour never passed beyond a pale yellow.

(β) For all experiments, first grey white, second yellow, third orange. Last experiment, no orange.

(γ) For all experiments except last, first white, second yellow, third orange.

(δ) For all experiments except last, first grey white, gradually becoming warmer till it reached orange.

(ϵ) First white like phosphorus in the dark, gradually getting to rose, and winding up with a reddish-orange not reached in the last experiment.

(ζ) First white with a dark shade, second yellow, third orange; no difference in any of the experiments except the last, where the temperature was lower, and the orange was not reached.

I may add that the temperature of the heating bar was a little reduced each experiment, the colours changed very slowly, and gave ample time for observation. A. NOBLE.

Poincaré's "Thermodynamics."

M. TAIT ne répond pas à mon objection sous prétexte qu'elle est sans importance. Je maintiens que nous n'avons aucun moyen non seulement d'assigner l'origine des forces électromotrices Thomson, mais encore d'en constater l'existence. Si M. Tait veut répondre, et s'il connait ce moyen, qu'il l'indique. Dans le cas contraire, s'il n'est pas en mesure de soutenir une quelconque de ses critiques, et s'il préfère un autre terrain de discussion, je suis prêt à l'y suivre.

Seulement je serai forcé d'être un peu plus long, car il me faudra passer en revue les trois reproches de M. Tait.

(1) La forme de mon ouvrage est trop mathématique.

C'est là une appréciation personnelle dont il n'y a pas à disputer. Je veux bien d'ailleurs d'une polémique sur une question de doctrine, mais non d'un procès de tendance où je jouerais le rôle d'accusé.

Toutefois il est certain que je consacre relativement peu de place à la description des expériences, et on aurait le droit de s'en étonner si je n'en donnais l'explication. Mon livre est la reproduction textuelle de mon cours; or mes auditeurs avaient tous suivi déjà un cours de physique expérimentale, où ces expériences leur étaient décrites en détail. Je n'avais donc qu'à leur en rappeler brièvement les résultats.

(2) J'ai mal parlé de la définition de la température absolue.

Autant que je puis comprendre, M. Tait ne trouve pas ma définition mauvaise, et n'en propose pas une autre; mais, dit-il, j'aurais dû parler des expériences de Joule et Thomson, qui permettent de mesurer la température absolue.

Or j'ai décrit ces expériences à la page 164, et j'ai montré à la page 169 comment elles permettent de déterminer la température absolue.

(3) J'ai laissé complètement de côté une explication mécanique du principe de Clausius que M. Tait appelle "the true (*i.e.* the statistical) basis of the second Law of Thermodynamics."

Je n'ai pas parlé de cette explication, qui me paraît d'ailleurs assez peu satisfaisante, parce que je désirais rester complètement en dehors de toutes les hypothèses moléculaires quelque ingénieuses qu'elles puissent être; et en particulier j'ai passé sous silence la théorie cinétique des gaz. H. POINCARÉ.

Ornithology of the Sandwich Islands.

HAVING just returned from an exploring expedition into the interior of Australia, on my way home I lingered in this group of islands, and was sorry to find that some species which have been obtained here are now no longer to be found.

My attention has been called to an interesting paper by Prof. Newton in your last issue (p. 465), on this subject, which seems to imply that the ornithological collection made by Sir Joseph Banks during his voyage in the *Endeavour* with Captain Cook no longer exists, which I beg to be allowed to make a few remarks upon. After the return of Sir Joseph Banks he had several cases of birds carefully mounted and arranged according to the localities in which they were collected. In one group of land birds from Owhyhee, another case contained a number of specimens from Botany Bay, conspicuous in the centre of which was a very fine specimen of the Black Swan, which was shot by Captain Cook himself.

These cases were in the custody of the Linnean Society of London until 1863, when they formed part of their Natural History sale.

These cases have been carefully preserved, and are now in the museum of my ancestor, Mr. John Calvert, together with a number of cases of birds which formed part of Sir Ashton Lever's collection, amongst which are a few from the Pacific Islands. These last cases were purchased from the executors of the late Mr. M. Armfield, of Catherine Street, Macclesfield,

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about twenty-five years ago, being a very fine collection of many thousand ornithological specimens, with a quantity of interesting correspondence with Mr. J. Thompson, of Knowsley Aviary, Mr. Reid, of Doncaster, R. Dunn, of Hull, and many other naturalists of that period; these explain upon what terms he obtained the egg and a very fine specimen of the Great Auk.

ALBERT F. CALVERT.

63, Patshull Road, Kentish Town, March 19.

Superheated Steam.

LORD RAYLEIGH (p. 438) rebuts my objection to the statement regarding the efficiency of a vapour-engine in which pure water is replaced by a saline solution, pointing out "that Maxwell's exposition of Carnot's engine applies *without the change of a single word*, whether the substance *in the cylinder* be water, mercury, or an aqueous solution of chloride of calcium." The latter italics are mine. In the statement objected to by me the aqueous solution of chloride of calcium was *in the boiler*, and what was *in the cylinder* was *superheated steam*, which is not included in the above list, so that the application of Maxwell's exposition is somewhat difficult. The greater part of the fresh water supplied to passengers in steamships is now produced by condensing the *superheated vapour of a saline solution*, and the culinary experience is that the substance which was in solution has all been left in the boiler. My contention, therefore, still stands—the saline mixture is not the working substance, and Carnot's law refers to the working substance only, and not to anything left in the boiler.

"In each case there is a definite relation between pressure and temperature." This is evidently merely a slip of the pen, the writer having for the moment forgotten that he was dealing with *superheated* steam, for which there is not a definite relation between pressure and temperature. The condition of superheated steam is completely defined when *both* pressure and temperature are given; but pressure is here a function of temperature-and-something-else, and temperature is here a function of pressure-and-something-else. That something-else may be volume or it may be energy, or, preferably, it may be entropy, but it must be something which cannot be predicated from pressure alone or from temperature alone.

"(So far as the substance is concerned), all that is necessary for the reversible operation of the engine is that the various parts of the working substance should be in equilibrium with one another throughout." No; for, in addition, it is necessary that the working substance should have only one pressure consistent with any given temperature. For this reason, superheated steam, however it may have been produced, can never be the working substance in a Carnot's engine. In the reversed cycle, when the steam is raised from a saline solution, from the beginning of the higher isothermal, the pressure would go on increasing until it became that due to saturated steam at the temperature of the superheat. This might be double the maximum pressure in the original cycle.

"The various parts of the working substance should be in equilibrium with one another throughout." The writer seems to say that the steam of a saline solution is a stable saturated vapour. It is H_2O at a given pressure and temperature, and the condition of the substance is by this definition completely determined, and there is no alternative; but it is not stable. Say that the steam-space of the boiler is increased by adding a vertical cylinder alongside the boiler, open to it. On the bottom of that vessel the steam might condense—pure water—and the temperature of the steam immediately over this water would be that of saturated vapour at the same temperature, and from there all through the steam-space to the surface of the saline solution in the boiler the temperature would increase, and all would have the same pressure. There would be mechanical equilibrium, but not thermal equilibrium.

"At the upper limit, *all* the heat is received at the highest point of temperature,"—but just as it would be if the evaporation were from a film of water upon a nearly bare combustion chamber crown. The plate is left in the boiler, and so is the salt, and in neither case would the steam exhibit a "state of things strongly contrasted with that which obtains when vapour rising from pure water is afterwards superheated." I have stated in my previous letter that the heat of evaporation is all *received* at identically the same temperatures as when it is raised from pure water at the same pressure, and the contrast is only as strong as that between occult and obvious. I have now shown that the vapours must be identical from either beginning; and unless each carried a certificate of birth, I do not now see how it would be possible to tell one from the other.

London, March 12. J. MACFARLANE GRAY.

Phoronomy.

I THINK it will be admitted by all, that precision of language is of great importance in scientific terminology; and the letter of Dr. Besant, which appeared in your issue of last week (p. 462), certainly suggests strong reasons for employing the word *phoronomy* in the place of *kinematics*.

The word may at first sight appear strange to the present generation of mathematicians; but if it becomes acclimatized, its employment will appear as natural as the phrases *kinetic* and *potential* energy, in the place of such meaningless phrases as *vis-viva* and *force-function*.

When the medical profession require a new word, they almost always have recourse to the Greek language; and mathematicians and physicists would do well to follow their example, and in cases of doubt or difficulty to consult some eminent classical scholar. I must confess, that I have no sympathy with the attempts, which have occasionally been made, to introduce short words of Teutonic origin into scientific nomenclature, as such words have always appeared to me to be singularly deficient in point. A good example of this is furnished by the word *spin*, which Clifford attempted to introduce in the place of the phrase *molecular rotation*. The latter phrase, although a little long, exactly expresses the idea which it is intended to convey, viz. that the molecules of the fluid possess a motion of rotation as well as a motion of translation. The word *spin*, on the other hand, does not express any such idea, but is strongly suggestive of the juvenile, though not altogether unscientific, pastime of spinning peg-tops.

A. B. BASSET.

322 Oxford Street, W., March 18.

I HAVE before me the second edition of F. Redtenbacher's "Principien der Mechanik und des Maschinenbaues" (Mannheim, 1859), of which the first section is entitled "Die Bewegung als Erscheinung (Phoronomie)." Whether the term occurs already in the first edition (1852), I cannot affirm, but I remember very well that Redtenbacher, in his lectures in Carlsruhe, in 1858, insisted upon that term being distinct from "Dynamik" and "Kinematik." I conclude, therefore, that the majority of the 786 students of that year—among them many foreigners—as also those of other years, were conversant with the term.

M. AM ENDE.

Westminster Chambers, 5 Victoria Street,
London, S.W., March 19.

SOME other correspondent is pretty sure to be mentioning that Mr. W. H. Besant will find in Kant's *Metaph. Anfangsgründe der Naturwissenschaft* all the authority he could desire for his proposed use of the word "phoronomy." Kant regularly uses the word in the sense of the later "kinematic"; and he was man of science enough to justify anyone in following his lead.

G. C. R.

The Tudor Specimen of Eozoon.

IN reference to the remarks made by Sir J. W. Dawson (NATURE, March 17, p. 461) on my paper on the Tudor specimen of Eozoon (*Quart. Journ. Geol. Soc.*, vol. xlvii. pp. 348–55), I should like to say that the whole point of that paper was that it was based on Sir J. W. Dawson's original type. The figure of this specimen has been repeatedly republished by Sir J. W. Dawson, and, in the absence of illustrations or details of other specimens from Tudor, upon its evidence alone rests the asserted occurrence of Eozoon in the Tudor limestone, and the great claims based thereupon. The value of other specimens from this locality was not rated very highly by Sir J. W. Dawson so recently as September 1888, when he remarked, "*Without additional specimens*,[1] and in the case of creatures so variable as the Foraminifera, it would be rash to decide whether

[1] And he previously refers only to "*the* specimen," "this very interesting specimen," "the fine specimen from Tudor," &c.

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the differences above noted[1] are of specific value."[2] I may add that I have recently seen the specimens of Tudor limestone exhibited in the Peter Redpath Museum, and my estimate of their value coincides exactly with that of Sir J. W. Dawson in 1888. As Sir J. W. Dawson most kindly promises his assistance to other workers, perhaps he would submit to some of them any specimens from Tudor which he regards as more conclusive than his original type.

It would seem rather unnecessary for anyone to trouble to infer from my paper that Sir J. W. Dawson has "regarded the Madoc and Tudor specimens as 'Lower Laurentian,'" when that is so directly stated by Sir J. W. Dawson in his description of his figure; viz. "Specimen of *Eozoon canadense* embedded in a dark-coloured homogeneous limestone occurring in the Lower Laurentian series at Tudor, Canada West" (*Quart. Journ. Geol. Soc.*, vol. xxiii. p. 265). J. W. GREGORY.

British Museum (Natural History), S.W.

The Theory of Solutions.

IN his last letter (NATURE, March 3, p. 415) Prof. Ostwald repeats his opinion that a theory is "a complex of laws, grouped around and derived from a main law," and infers from my letter that what I term a *theory* he would term an *hypothesis*.

If this were the whole point at issue, I could meet it in no better way perhaps than by referring Prof. Ostwald to his own works. For example, in his "Outlines of General Chemistry," are to be found not only numerous instances of the use of the word theory in its ordinary and accepted sense (*e.g.*, p. 58) but also cases in which it is employed as synonymous with hypothesis (*e.g.*, p. 187).

With regard to the definition of solutions as mixtures, Prof. Ostwald maintains that even if hydrates are formed in a solution, the solution is finally a mixture of the hydrates and the remaining solvent. The real question involved is unaffected by this explanation. There is no doubt whatever that to the majority of readers the definition, without any qualifying clause, that solutions are mixtures leads to one conclusion and no other—namely, that between solvent and dissolved substance there is no interaction of a chemical nature. Prof. Ostwald has in his letters stated that in some cases he considers such interactions occur; he has also stated that between chemical and physical processes he knows of no distinction. The definition is at variance with both these views, and it seems but fair to conclude that such discordant statements tend in no way to obviate that misconception which Prof. Ostwald so often deplores.

In defence of the application of van der Waals's equation to solutions, a process questioned by me in my letter, Prof. Ostwald states that van der Waals himself has taken up this very question. The method by which van der Waals approaches the subject, curiously enough, furnished the main grounds for my objections. The most superficial comparison of the complex formula which van der Waals deduces for a mixture of two substances, with such an application of his simple gas equation to a solution as that given in Prof. Ostwald's book, is ample justification for my strictures. But apart even from such evidence as to the inadequacy of the application, the form which it is finally made to assume is in itself a proof of its incompleteness. By judicious simplification the application is made to take the shape of a linear equation in which "pressure forces due to the interactions of molecules are absent." That is to say, the cohesion of solvent and dissolved substance, and the mutual reactions of both, are alike ignored. Further comment on such a method of accounting for the phenomena of solutions appears to me to be superfluous. J. W. RODGER.

London, March 7.

The Limpet's Strength.

THE limpet experiments of your esteemed correspondent, Mr. Percy Aubin, as reported in NATURE of March 17 (p. 464) would have been still more interesting and instructive had he weighed the animals deprived of their shells.

On April 10, 1890, I published my experiments showing that the shell-less limpet pulls 1984 times in the air its own weight, and about double when immersed in water.

Fasting fleas on an average pull 1493 times their own dead weight.

Other experimenters give the pulling power of the shell-deprived *Venus verrucosa* of the Mediterranean, a cockle-like creature, at 2071 times the weight of its own body.

The force required to open an oyster appears to be 1319·5 times the weight of the shell-less oyster.

J. LAWRENCE-HAMILTON, M.R.C.S.

30 Sussex Square, Brighton, March 19.

Technical Education for Novelists.

AMIDST the many schemes for technical education, could you not put in a plea for the "author of the popular novel"? Perhaps the need will best appear from these illustrations taken from the first 100 pages of a recently published and loudly heralded work.

(1) Scene—Kinder Scout, Derbyshire. Date—"after the snows and rains of early April," 1864. Time—*after* 8 *p.m.* "It was a clear, frosty night, *promising* a *full* moon."

(2) Same place, *Easter Eve*, 1864. "The wooded sides of the great moor were fading into dimness, and to the east a *young* moon was *rising*." W.

March 12.

THE ORIGIN OF THE YEAR.

I.

IT would seem that in the dawn of civilization it was not at all a matter of course that the sun should be taken as the measurer of time, as it is now with us; and in this connection it is worth while to note how very various the treatment of this subject was among the early peoples. Thus, for instance, it was different in Egypt from what it was in Chaldæa and Babylonia, and later among the Jews. In the Egyptian inscriptions we find references to the moon, but they prove that she occupied quite a subordinate position to the sun; while in Chaldæa it would seem that the moon was the chief thing worshipped, and it was thus naturally the chief means used for measuring time, and, so far as months were concerned, this, of course, was quite right. In Chaldæa, too, where much desert travel had to be undertaken at night, the movement of the moon would be naturally watched with great care.

An interesting point connected with this is that, among these ancient peoples, the celestial bodies which gave them the unit period of time by which they reckoned were practically looked upon in the same category. Thus, for instance, in Egypt the sun being used, the unit of time was a year; but in Chaldæa the unit of time was a month, for the reason that the standard of time was the moon. Hence, when periods of time were in question it was quite easy for one nation to conceive that the period of time used in another was a year when really it was a month, and *vice versâ*. It has been suggested that the years of Methuselah and other persons who are stated to have lived a considerable number of years were not solar years but lunar years—that is, properly, lunar months. This is reasonable, since if we divide the numbers by 12 we find that they come out very much the same length as lives are in the present day.

There seems little doubt that the country in which the sun was first definitely accepted as the most accurate measurer of time was Egypt.

"The Egyptians," says Ranke in the first chapter of his "Universal History," which is devoted to Egypt, "have determined the motion of the sun as seen on earth, and according to this the year was divided, in comparison with Babylon in a scientific and practically useful way, so that Julius Cæsar adopted the calendar from the Egyptians and introduced it into the Roman Empire; the other nations followed suit, and since then it has been in general use for seventeen centuries. The calendar may be considered the noblest relic of the most ancient times which has influenced the world."

[1] *i.e.* between *the* specimen from Tudor and those from other localities.

[2] "Specimens of *Eozoon canadense*," Mem. Peter Redpath Mus., 1888, p. 43.

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A study of the Egyptian monuments has shown most conclusively that towards the end of the ancient empire the Egyptians possessed a year as accurate for calendar purposes as our own, and that they had been led up to the knowledge of its true length by successive steps.

As we shall show further on, this earliest of all years that we know of in history began at the summer solstice. Since one of the oldest temples at Thebes is oriented to sunset at the summer solstice, we should be not at all surprised if investigation shows that when that temple was built, more than 3000 years B.C., the Egyptian year really began in what we should call our summer. We have ample evidence of this. And I think there is little doubt also that when Stonehenge was built it certainly was built by people who began their year with the summer solstice, which is the time of the year in which in many countries it is the habit still to light fires upon hills and so on. If we look up the records of the peoples that lived, say, during the 1000 years preceding the birth of Christ, we find that the different races began their year at different times, and even that the same race at different times began their year differently; the choice lay among the equinoxes and the solstices.

Wherever the ancient Egyptians came from, whether from a region where the moon was the time-measurer of not, so soon as they settled in the valley where the Nile then as now like a pendulum slowly beat the years by its annual overflows at the summer solstice, the solar basis of their calendar was settled.

We can well understand, therefore, since the whole life of the country depends upon the river, and all the energies of the inhabitants are connected with the work to be done during its rise and fall, that the moment of the commencement of the inundation, about the time of the summer solstice, should be chosen as the beginning of the year. Hence the perpetual reference to Solstice and Nile flood in the Egyptian annals.

It might be imagined at first sight that, as the year was thus determined, so to speak, by natural local causes, the divisions or seasons would be the same as those which Nature has given *us*. This is not so. Egypt is too near the tropics, and the local conditions are too different from our own, to permit of the application of our seasonal divisions of the year.

As Egypt, in the description quoted by Krall, "first appears like a dusty plain, then as a fresh-water sea, and finally as a bed of flowers," so the year is divided into three seasons instead of four.

Beginning with the inundation (summer solstice) we have—

(1) The season or *tetraemne* of the inundation,
(2) ,, ,, ,, sowing,
(3) ,, ,, ,, harvest.

From the earliest times the year was divided into twelve months, as follows:—

Inundation ...	Thoth	End of June (Gregorian).	
	Phaophi	,,	July.
	Athyr	,,	August.
	Choiak	,,	September.
Seed time ...	Tybi	,,	October.
	Menchir	,,	November.
	Phamenoth	,,	December.
	Pharmouthi	,,	January.
Harvest ...	Pachons	,,	February.
	Payni	,,	March.
	Epiphi	,,	April.
	Mesori	,,	May.

Now whether the Egyptians brought their year with them or invented it in the Nile valley, there is a belief that it at first consisted of 360 days only, that is $5\frac{1}{4}$ days too little. It is more likely that they brought the lunar month with them, taking it roughly as 30 days ($30 \times 12 = 360$), than that they began with such an erroneous notion of the true length of the solar year, seeing that in Egypt, above all countries in the world, owing to the regularity of the inundation, the true length could have been so easily determined, so soon as that regularity was recognized. We must not in these questions forget to put ourselves in the place of these pioneers of astronomy and civilization: if we do this, we shall soon see how many difficulties were involved in determining the true length of such a cycle as a year, when not only modern appliances, but all just ideas too, were of necessity lacking.

Still it is right that I should state that all authorities are not agreed as to the use of this year of 360 days. Ideler[1] considers it very doubtful. Krall,[2] however, urges that a certain inscription (the trilingual inscription of Tanis) expressly refers to it.

He adds to this some evidence, which he considers confirmatory, from religious usages. Thus at Philoe, in the temple of Osiris, there were 360 bowls for sacrifice, which were filled daily with milk by a specified rotation of priests. At Acanthus there was a perforated cask

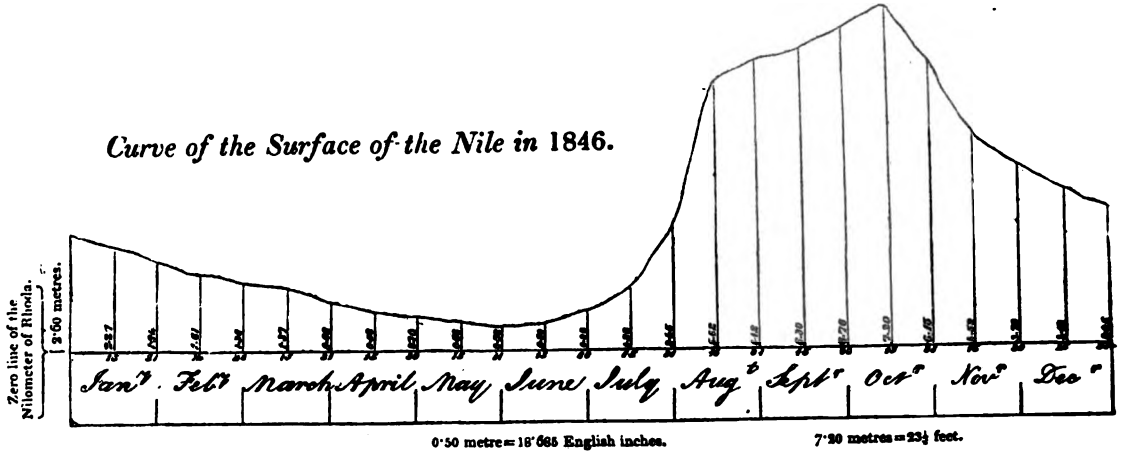

FIG. 1.—The annual rise and fall of the Nile (from Horner).

[1] "Chronologie," i. p. 70.
[2] "Studien zur Geschichte des alten Aegypten," i. p. 16.

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into which one of the 360 priests poured water from the Nile daily. Krall adds :—[1]

"It is probable that the year of 360 days dates from the time before the immigration into the Nile valley, when the Egyptians were unguided by the regular recurrence of the Nile flood. In any case, this must soon have convinced the priests that the 360-days year did not agree with the facts. But it is well known to everybody familiar with these things how long a period may be required before such determinations are practically realized, especially with a people so conservative of ancient usages as the Egyptians."

Supposing the use of this 360-day year to have been universal, it is perfectly certain that the Egyptians, now in this part of the Nile valley, now in that, must have got their calendar into the most hopeless confusion, compared with which "the year of confusion" was mere child's-play, and that the exact determination of the times of sowing, reaping, &c., by means of such a calendar would have been next to impossible.

As each year dropped 5¼ days, it is evident that in about seventy years $\left(\frac{365{\cdot}25}{5{\cdot}25}\right)$ a cycle was accomplished, in which new year's day swept through all the months. The same month (so far as its name was concerned) was now in the inundation time, now in the sowing time, and so on. Of fixed agricultural work for such months as these there could be none.

It must have been, then, that there were local attempts to retain the coincidences between the beginning of the year and the Nile flood and solstice; intercalation of days or even of months being introduced, now in one place, now in another; and these attempts, of course, would make confusion worse confounded, as the months might vary with the district, and not with the time of the year.

That this is what really happened is, no doubt, the origin of the stringent oath required of the Pharaohs in after times, to which I shall subsequently refer.

This year of 360 days had naturally to give way, and it ultimately did so in favour of one of 365. The precise date of the change is not known, but it is referred to in inscriptions of the time of Amenemha I. (*circ.* 2400 B.C.). This, of course, does not exclude the possibility, indeed even the probability, that it was introduced much earlier. The five days were added as epacts or epagomena; the original months were not altered, but a "little month" of five days was interpolated at the end of the year between Mesori of one year and Thoth of the next.

When the year of 365 days was established, it was evidently imagined that finality had been reached; and mindful of the confusion which, as we have shown, must have resulted from the attempt to keep up a year of 360 days by intercalations, each Egyptian king on his accession to the throne bound himself by oath before the priest of Isis, in the temple of Ptah at Memphis, not to intercalate either days or months, but to retain the year of 365 days as established by the Antiqui.[2] The text of the Latin translation preserved by Nigidius Figulus, cannot be accurately restored. Only thus much can be seen with certainty.

To retain this year of 365 days then became the first law for the king, and indeed the Pharaohs thenceforth throughout the whole course of Egyptian history adhered to this year, in spite of their being subsequently convinced, as we shall see, of its inadequacy for a long period. It was a Macedonian king who later made an attempt to replace it by a better one

The years of 360 and 365 days to which we have so far referred are termed in the inscriptions the "little" and "great" years respectively.

[1] *Loc. cit.*, p. 20.
[2] Mommsen, "Chronologie," p. 258.

How, then, was this 365-day year, which had been introduced with such pomp and circumstance, regulated? This brings us to a new point.

The Heliacal Rising of Sirius.

I have insisted upon the perfect regularity of the rise of the Nile affording the ancient Egyptians, so soon as this regularity had been established, a nearly perfect way of determining the length of the year.

It is also clear that so soon as the greatest northing and southing of the sun rising or setting at the solstices had been recognized, and that the intervals between them in days had been counted, a still more accurate way would be open to them, especially if, as I believe, the observations of the solsticial risings or settings were made in temples (or observatories) accurately oriented to the proper amplitude.

In this way, then, the great natural festival of the year would be the nearly coincident commencement of the inundation and the summer solstice.

As I have said, the solstice might have, one may say *must* have, occurred with greater regularity than the rise of the river, so that as accuracy of definition became more necessary the solstice would be preferred. The solstice was common to all Egypt, the commencement of the inundation was later as the place of observation was nearer the mouth of the river.

Now it seems as if among all ancient peoples each sunrise, each return of the sun—or of the sun-god—was hailed, and most naturally, as a resurrection from the sleep—the death—of night: with the returning sun, man found himself again in full possession of his powers of living, of doing, of enjoying. The sun-god had conquered death, man was again alive. Light and warmth returned with the dawn in those favoured eastern climes where man then was, and the dawn itself was a sight, a sensation, in which everything conspired to suggest awe and gratitude, and to thrill the emotions of even uncivilized man.

What wonder, then, that sunrise was the chief time of prayer and thankfulness? But prayer to the sun-god meant, then, sacrifice, and here a practical detail comes in, apparently a note of discord, but really the true germ of our present knowledge of the starry heavens which surround us.

To make the sacrifice at the instant of sunrise, preparations had to be made, beasts had to be slaughtered, and a ritual had to be followed; this required time, and a certain definite quantity of it; to measure this, the only means available then was to watch the rising of a star, the first glimmer of which past experience had shown preceded sunrise by just that amount of time which the ritual demanded for the various functions connected with the sunrise sacrifice.

This, perhaps, went on every morning, but beyond all question the most solemn ceremonial of this nature in the whole year was that which took place on New Year's morning, or the great festival of the Nile-rising and summer solstice, the 1st of Thoth.

How long these morning and special yearly ceremonials went on before the dawn of history we, of course, have no knowledge. Nor are the stars thus used certainly known to us; of course any star would do which rose at the appropriate time before the sun itself, whether the star was located either in the northern or southern heavens. But in historic times there is no doubt whatever about the star so used. The warning-star watched by the Egyptians at Thebes, certainly 3000 B.C., was Sirius, the brightest of them all, and there is much evidence that Sirius was not the star first used.

"Besides the solstice and the beginning of the Nile flood, there was an event in the sky which was too striking not to excite the general attention of the Egyptian priesthood. We also know from the newly-discovered

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inscriptions from the ancient empire that the risings of Orion and Sirius were already attentively followed and mythologically utilized at the time of the building of the pyramids."[1]

J. NORMAN LOCKYER.

(*To be continued.*)

THE WINTER STORMS OF NORTHERN INDIA.

THE physical constitution and history of the storms of India and Indian Seas is a subject which, almost from his first association with the Indian Meteorological Department, Mr. Eliot has made peculiarly his own. Besides nine elaborate, and, as far as possible, exhaustive, memoirs and reports on the history of particular storms, two on the tracks and periodicity of the cyclones of the Bay of Bengal during the ten years 1877–1886, and an admirable hand-book, in which he has summarized, for the guidance of seamen, the characteristic features and behaviour of these storms, his annual reports on the meteorology of India have always been replete with the results of his studies of the storms of the current year; and to him is mainly due that development which has been effected in the system of storm warnings for the coasts of India in recent years, and has rendered it one of the most efficient and comprehensive organizations for that purpose now in operation in any part of the world.

Like most other features of the climate, the storms of India differ very greatly in their leading characteristics at different seasons of the year. We have, in the first place, the well-known cyclones and cyclonic storms of the Bay of Bengal and the Arabian Sea, which are most frequent when the summer monsoon is at its height, and most severe at its commencement and termination. These are generated over some part of the tropical sea north of latitude 6°, and travel, as is now well known, on tracks between west and north—most frequently north-west or west-north-west; sometimes, however, in the spring and later months of the year, recurving to north-north-east or even north-east, as they approach the tropic. In virtue of their severity and destructiveness, these storms have attracted far more attention than any others, and not only Piddington's, but also the writings of most of his successors, have been almost exclusively devoted to them.

Of a very different type are the storms that bring the rainfall of the cold season and the earlier spring months to Northern India. It would be incorrect to speak of these as the storms of the winter monsoon (unless the term be understood, in its strictly etymological sense, as merely the name for a season of the year), for during their passage the northerly winds are suspended over a great part of India, and, with the rarest exceptions, they never penetrate far into the tropics. These storms, if they travel, always move from west to east. They have the usual cyclonic constitution, but the winds have but little force; and it was not until the preparation of daily weather charts for the first time showed their true nature, that this fact was even suspected.

Many of the features of these cold-weather storms are very striking and characteristic, and, as has been remarked by Mr. Eliot in his reports for 1888 and 1889, the temperature of Northern India in the cold season is chiefly determined by their number and character. Each of them is preceded by a wave of high temperature, and followed by a cold wave; except, indeed—and the exception is instructive—when the course of the storm is so far south of the Himalayas that little or no snow falls on the mountains (see "Climates and Weather of India," p. 206).

[1] Krall, *op. cit.*, p. 45. See also Brugsch, "Aeg. Zeit.," 1881, p. 1, *seqq.*

In these cases, which are, however, exceptional, the cold wave is sometimes evanescent. On the other hand, as Mr. Eliot remarks, "the intensity and period [of the cold wave] largely depend on the amount of rainfall in Northern India and of the snowfall on the Himalayan mountain regions, and the height to which the snow-line has descended." As a rule, therefore, the cold wave follows the storm. Mr. Eliot gives, in his report, a table of the changes of temperature from day to day from January 20, to February 7, 1889, which includes two very characteristic illustrations of this phenomenon, and which therefore we extract. The figures show the variations of the observed mean temperature of each day from the average of many years for the same day. The crest of each warm wave is emphasized by strong type and the trough of each cold wave by italics.

The history of these two storms is as follows, and it exhibits one or two remarkable and suggestive features, which will presently be noticed more particularly. The first disturbance originated (or made its first appearance in India) on January 22. There were two separate centres and areas of disturbance, one of which covered the Punjab Himalaya and adjacent plains from Sialkote to Roorkee. This filled up [apparently] on the 23rd, after giving moderate snow on the hills and light showers on the adjacent plains. The other originated in Rajputana, and advanced, on the 23rd and 24th, in an easterly direction, across Northern India into Burma. It gave moderate general rain to the North-West Provinces and Central India, and light showers to Behar, Bengal, and Assam. This first storm was therefore of very moderate intensity. The snowfall and rainfall were but slight, and in the Punjab, Bengal, and Burma insufficient to bring down the temperature below the normal average.

The second disturbance was one of greater intensity, but like its predecessor had a double centre. One part consisted of a shallow depression, which passed into Sind from Baluchistan on the 28th, advanced through Central India, Behar, and Bengal, on the three following days, and into Burma on February 1, where it slowly filled up during the next three days. The other part was a deep depression, which formed in the Northern Punjab on the evening of the 28th, and during the next thirty-six hours marched slowly to the south-east along the southern face of the Punjab Himalayas. It filled up very rapidly on the evening of the 30th, and morning of the 31st of January, in the South-Eastern Punjab. The double disturbance gave a very heavy fall of snow over the whole of the Western Himalaya, bringing the snow-line down to 3500 or 4000 feet, and also general rain to nearly the whole of Northern and Central India, which was greatest in amount in the Punjab, the North-West Provinces, and Behar. As is shown by the table (see next page), the fall of temperature after this storm was proportionately great, amounting to 9½° in the Punjab, and to 14° or 15° on the mean of the day in Guzerat, Central India, and the Central Provinces. It continued three or four days after the weather had cleared up, so that the trough of the cold wave followed the crest of the warm wave after an interval of five or six days, and each occupied three or four days in passing from the Punjab to Burma. Mr. Eliot gives eight charts in illustration of these waves, of which we reproduce those for January 30 and 31 and February 1. They are projected for the observed temperatures at 8 a.m. of those days, and show, not the temperatures themselves, but the amounts by which these deviate in excess or defect from the averages of many years at the same hour. The isabnormals of deficient temperature are represented by broken, those of excessive temperature by continuous lines.

Mr. Eliot remarks that in the warm waves the greatest excess is generally exhibited by the night temperatures; and in a table which he gives of the deviations of the daily maxima and minima from their respective normal

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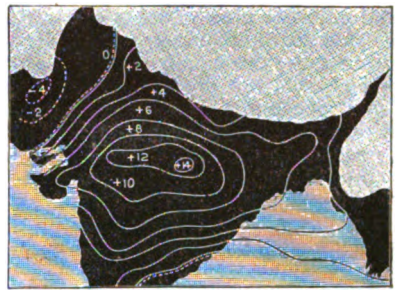
DAILY TEMPERATURE ANOMALIES (DEG. FAHR.).

PROVINCES.	January													February						
	20th.	21st.	22nd.	23rd.	24th.	25th.	26th.	27th.	28th.	29th.	30th.	31st.	1st.	2nd.	3rd.	4th.	5th.	6th.	7th.
Punjab	-4·2	-0·5	+4·0	+2·3	+0·1	+0·2	+2·2	+2·9	+3·2	0	0	-2·2	-3·4	-3·5	-6·1	-4·3	-1·8	-0·6	+3·5
Sind and Rajputana	-4·1	-2·1	+2·3	+5·2	-0·6	-0·9	+3·2	+4·6	+4·1	+4·4	+3·6	-3·8	-1·6	-2·6	-6·4	-5·7	-0·7	+4·9	+5·4
Guzerat and Central India ...	-0·2	+1·4	+4·8	+7·9	-1·8	-0·3	+1·8	+4·8	+6·6	+9·5	+10·9	+2·6	-3·3	-2·3	+0·4	-4·4	-2·9	+1·8	+4·0
North-West Provinces	+2·5	+0·2	+0·9	+5·3	+4·3	-3·4	-0·8	-1·5	+1·7	+5·0	+6·4	+0·5	-1·5	-2·5	-4·1	-5·2	-5·0	-2·3	+1·0
Central Provinces...	+5·1	+4·7	+6·5	+8·6	+3·2	-0·2	+3·0	+5·6	+7·3	+9·7	+10·4	+6·0	-3·2	-3·1	+1·5	-1·1	-4·1	-0·7	+4·0
Bengal	+6·8	+2·9	+0·9	+1·3	+3·4	+1·8	+1·6	+0·6	+2·3	+3·9	+6·1	+4·2	+3·9	-2·4	-3·2	-2·4	-5·0	-4·2	-2·6
Burma	+2·0	+1·5	+1·4	+1·8	+2·5	+2·6	+2·5	+1·2	+1·7	+2·2	+2·6	+3·1	+4·0	+3·7	+1·3	+0·6	+0·4	-1·5	-0·1

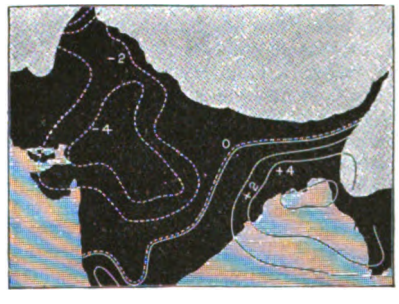
values, it appears that those of the latter are from one and a half times to nearly twice as great as those of the former. On the other hand, in the cold waves which follow the storm, the day temperatures frequently show a greater relative depression than those of the night-time, indicating therefore that the nocturnal radiation under a clear sky is far from being the only cooling agency operative.

Notwithstanding this latter feature, the cold waves often bring about an inversion of the normal temperature relations between the hills and the plains, and that in a very remarkable degree. This was observed on no less than eleven nights in January 1889; and a very interesting paper on the subject was contributed by Mr.



January 30.

January 31.



February 1.

Temperature isabnormals of twenty-four hours preceding 8 a.m.

Eliot to vol. lix., Part ii., of the Journal of the Asiatic Society of Bengal. On one occasion, the figures of which are given, not only was the minimum night temperature of the hill stations (at elevations between 6000 and 8000 feet) from 8° to 13° higher than on the adjacent plains, but actually higher than anywhere on the Indo-Gangetic plain except only the coast districts of Bengal.

Before noticing the barometric features of these storms, let us see what is the probable explanation of their striking vicissitudes of temperature. The weather charts and reports

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show that for a day or two previously to the appearance of the storm-centre in North-Western India light southerly or easterly winds prevail over Northern India, accompanied with increasing but not dense cloud, and a steady rise of vapour tension, which, as Mr. Eliot points out, is only in part explained by the increase of temperature. The distribution of the cloud is well marked and characteristic. "An examination of a large number of these storms shows that they invariably give light cloud to the south-east of their tracks, moderate cloud to the east, and overcast skies to the north. In fact the largest development of cloud usually occurs in the northern and north-eastern portion of the depression, and at a considerable distance to the north of the depression, as indicated by the isobars, . . . and usually in the hill districts, and adjacent plain districts of Northern India. Hence the cloud distribution appears to indicate that the ascensional motion accompanying these storms commences to the south, and proceeds slowly in the eastern quadrant, and is completed in the northern quadrant. The cloud thins off rapidly in the north-west quadrant, and in the west and south-west portions of the depression the skies are clear." These observations are very important; they show that the damp, warm air of southerly origin is absolutely restricted to the eastern side of the depression, for it is, so to speak, hall-marked by its freight of vapour, and this manifests its presence, and also indicates the region of its ascent and withdrawal from the earth's surface, by forming a thick cloud canopy, on the north and north-east chiefly. This too, of course, is the region of heaviest rain and snow fall.

The wave of warmth that precedes the depression is thus clearly explained by the unseasonable replacement of northerly by southerly winds; and since the warmth depends not only on the derived temperature of the air, but also, and in a very great measure, on the check given to nocturnal radiation by cloud and vapour, while it is not dense enough greatly to obstruct the solar rays, it is sufficiently obvious why the night temperatures should show a greater excess than those of the day-time.

For the reduction of the temperature in the cold wave that follows the depression, three distinct causes may be assigned: viz. the contact of a snow surface down to low levels on the Himalaya and Afghan mountains, from which are drawn the north-west winds of the western half of the storm; the evaporation of the rain that has fallen on the plains, and is rapidly taken up by the very dry air that has descended from the mountains; and lastly, the increased nocturnal radiation in this dry atmosphere under a cloudless sky. Of these it would seem that the first and second are the most influential. This seems to be indicated by the facts already quoted—that the intensity of the cold largely depends on the rain and snow fall, and the level to which the latter descends,[1] also that when the storm centre lies so far south of the mountains that little or no snow is precipitated on them, the cold wave does not always follow. It is further supported by the fact noticed by Mr Eliot, that the relative depression of the day temperature in the cold wave is frequently greater than that of the night hours. This effect of the snow is the more remarkable, when we consider that, in descending to the level of the Indo-Gangetic plain, the air must, of course, be dynamically heated about $5\frac{1}{2}^{\circ}$ for each thousand feet of descent. Nearly all this heat must have been expended in melting the snow, since the air arrives at the level of the plains dry and cold; so much so, indeed, that the further cooling it undergoes, in consequence of the evaporation of the rainfall and nocturnal radiation, brings down the night temperature of the plains sometimes as much as 13° below that of mountain ridges at 7000 feet.

It remains to notice the barometric features of these storms, for, if less striking than the vicissitudes of temperature that accompany them, they afford matter of much interest, and suggest questions of a wider bearing than such as have reference merely to the local circumstances of the sub-tropical zone. The barometric depression of these winter storms is as a rule very small, and the gradients are very low. On the Indian weather charts the isobars are laid down for increments of one-twentieth of an inch; but it is seldom that the storm vortex, when launched on the plains of India, is encircled by more than one or two closed isobars, and sometimes its position can only be gathered from the figures in the accompanying table of the reduced barometric readings, or recognized by the oblique inflection of the wind arrows. Such was the case with both the January storms, noticed above, which travelled across India to Burma, and also the Punjab secondary depression of January 22, 1889. But that which formed in the Punjab on January 28, and which is described as a stationary storm, was of a much more pronounced character, and for three days determined the leading features of the distribution of pressure all over North-Western India, the isobars being only slightly modified by the travelling depression further south. Of a similar type was the storm at the end of January 1883, of which illustrations are given in the report on the meteorology of India for that year. This too appears to have lingered in the angle of the Punjab inclosed by the Himalaya and the Afghan mountains, and the question forces itself upon us, whether there is not something in the physical features of this part of the country that favours the development of such vortices, and detains them while the feebler and shallower depressions that form part of the same general disturbance, pursue their course eastward across the plains. Mr. Eliot remarks on the effect of the Himalaya in causing a forced ascent of the southerly vapour-bearing winds, thus localizing the maximum precipitation on the north and north-east of the depression; and it is possible also that the angle at which the mountain chains meet on the north of the Punjab, inclosing the plain of that province, may not be without its influence in favouring the development and detention of the vortex.

It is still very obscure what are the general causes that determine the appearance of these storms in India. At one time it seemed to me probable that their origin was to be sought for in the local conditions of India itself, and even now I see no reason to question that, as distinct vortices, very many, perhaps most, of them originate on the plains of India; especially in such cases as the succession of storms in the earlier part of January 1883, of which a brief description was given in the "Climates and Weather of India." Mr. Eliot, too, seems to hold a similar view, since he speaks sometimes of storms "forming" in Rajputana, &c., sometimes merely as "first appearing" in Sind, Rajputana, or the Punjab. But in some of these instances there is evidence that the appearance of the storm on the plains of the north-western frontier was preceded by stormy weather in Afghanistan, indicating that the disturbance had reached India from that country or Baluchistan; and Mr. Eliot distinctly identifies a storm that appeared in the Punjab on January 9, 1889, with one that had been experienced at Bushire at the head of the Persian Gulf, on the 7th and 8th.

In this matter, we must distinguish between the barometric depression, which appears like the trough of a great atmospheric wave of very great extent both in longitude and latitude, sweeping across the country from the westward, and the vortex, or in some cases vortices, which are, as a rule, merely local and subordinate features of the former. No doubt, it is the passage of one of these troughs that in all cases determines the formation of the

[1] Some of the lowest temperatures hitherto recorded in Northern India were those observed in the first week of February 1883, four or five days after a storm which covered with snow the plain around Rawulpindi, only 1700 feet above the sea; down to a level therefore unprecedented in the meteorological annals of India. See Report on the Meteorology of India in 1883; also "Climates and Weather of India," p. 204.

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storm; but, except when the advent of a vortex can be distinctly traced to the highlands of the western frontier, it seems very likely that its development and duration are in some degree influenced by the local conditions of the land surface, such as have been already noticed in the case of the Northern Punjab; and its intensity would seem to be mainly dependent on the amount of snow and rain that is precipitated.

From what has been said above, the general resemblance of the winter storms of Northern India to those of our own latitudes will be sufficiently obvious. In their eastward movements, the localization of the rainfall, the contrasted temperature conditions of the opposite quadrants, and many other particulars, we may recognize their essential identity. But certain features which are more or less blurred in our European storms, in those that we are now dealing with stand out with much clearer definition; and they seem calculated to throw not a little light on the still vexed question of storm generation, and perhaps to reconcile some of the very conflicting views that now prevail on this subject. As Mr. Lewis Morris says of the old Greek myths—

"These fair tales which we know so beautiful
Show only finer than our lives to-day
Because their voice was clearer and they found
A sacred hard to sing them"—

so may we say of these manifestations of aërial action in India. The phenomena are similar in kind to those that pass before our own eyes, but they stand out, accentuated by the circumstances of the climate and country, with a clearness and prerogative emphasis that we may seek for in vain in the confused and kaleidoscopic weather phases of these latitudes. We read their meaning almost at a glance, as we gather that of a printed page, and have not laboriously to pick out and piece together the obscure facts that express it, as with painful effort we might decipher the faded and half-concealed characters of a monkish palimpsest. And by good fortune, we have at the head of the Indian Meteorological Department an accomplished mathematician and physicist, who appreciates to the full the rich opportunities of his charge, and who knows how to marshal and interpret his facts as well as record them.

HENRY F. BLANFORD.

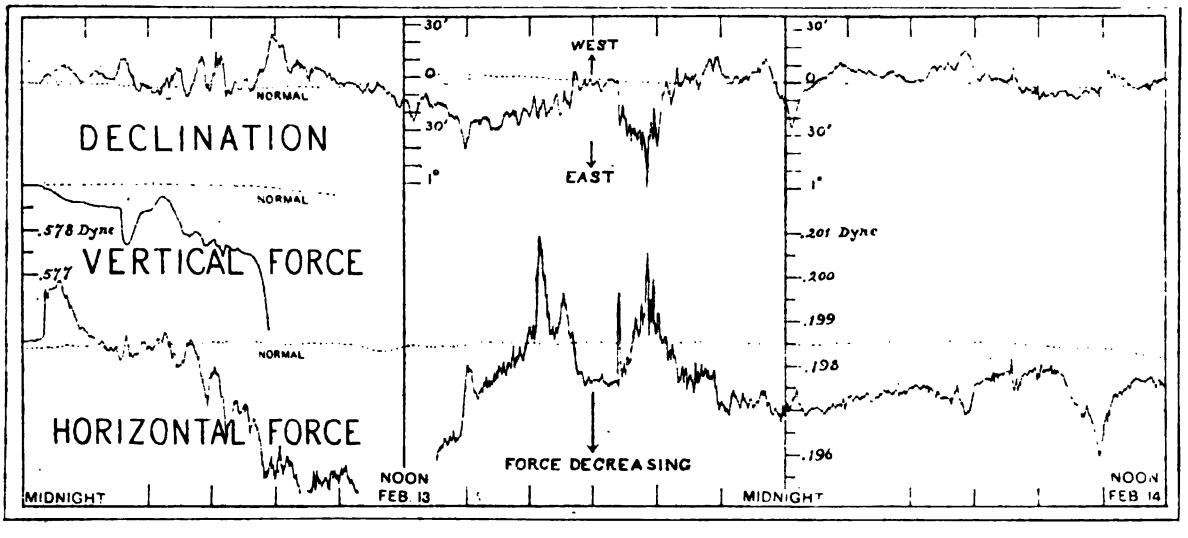
THE MAGNETIC STORM OF FEBRUARY 13-14, 1892.

THE Superintendent of the U.S. Naval Observatory sends us the following records of the magnetic storm of February 13:—

"The records of this unusually severe magnetic storm are of especial interest as occurring at the same time as the fine displays of auroræ and the appearance of a large group of sun-spots.

"The first increase in the horizontal force was followed by a rapid decrease, the force falling to much less than its usual strength, with rapid changes. Its change during the storm was 2½ per cent. of its mean strength. The vertical force decreased so much that the sensitive balanced magnet used to record it was upset at 8 p.m. of the 13th, and its further record lost.

"The auroræ were seen at Washington at about 2 a.m. and 7.30 p.m. of the 13th, the latter time being marked by an unusually disturbed condition of the magnets."



"The magnetic storm commenced suddenly at 12.40 a.m. (75th meridian time), February 13, with a movement of the north end of the declination magnet to the westward, and a rapid increase in the horizontal and decrease in the vertical components of the earth's magnetic force.

"The north end of the declination magnet remained to the westward of its normal position until 10.30 a.m., when it crossed to the eastward, all the time oscillating violently, and did not return to the normal until 8 p.m. of the 13th, after which it kept oscillating on each side of its mean position until the end of the storm. It registered a change of direction of 1½°.

WILLIAM DITTMAR.

ALL who take an interest in the progress of chemistry will regret the death of William Dittmar; a smaller circle feel that they have lost an invaluable friend. Born at Umstadt, near Darmstadt, April 15, 1833, he was the second son of Fritz Dittmar, then Assessor at Umstadt, afterwards Landrichter at Ulrichstein, in Ober-Hessen, where he took a part in the movements of 1848 displeasing to the Hessian Government, who removed him from office, allowing him to retire on a pension. At the age of sixteen, therefore, William became a resident in

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Darmstadt, where his father spent his enforced leisure. He was apprenticed to the Hof-Apotheker in Darmstadt, and in due time passed his "Gehülfe Examen" with distinction. He had access to a good collection of books on chemistry and physics, which he eagerly read. He went as Gehülfe to Mühlhausen in Alsace, where he spent several years, and returning to Darmstadt passed the "Staats Examen" in pharmacy, passing in the first class. But the attraction of pure chemistry prevailed, and in 1857 he went to Heidelberg. Bunsen soon saw what kind of student he had got, and appointed him assistant in the laboratory. There he met Sir Henry Roscoe, who invited him to Manchester as his private assistant. On Roscoe's appointment as Professor of Chemistry in the Owens College, Dittmar went with him as assistant. In 1861 he became chief assistant in the Edinburgh University Chemical Laboratory under Prof. Sir Lyon Playfair. In 1869 he went to Bonn, where he acted first as *Privatdocent* and afterwards as Lecturer on Meteorology at the Agricultural College at Poppelsdorf. In 1872 he declined the Chair of Chemistry in the Polytechnic School at Cassel, preferring to return to Edinburgh to his old post in the University. Here he remained only a few months, accepting in 1873 the Lectureship on Practical and Technical Chemistry in the Owens College. Thence he removed to Glasgow to succeed Prof. Thorpe in the Chair of Chemistry in the Andersonian College. This office he held till his death, February 9, 1892. He died literally in harness. He lectured in the morning, but not feeling very well, went home in the middle of the day, and after a few hours' illness died at 11.30.

He was a Fellow of the Royal Society and of the Royal Society of Edinburgh. In 1887 the University of Edinburgh conferred on him the degree of Doctor of Laws. The Philosophical Society of Glasgow awarded him last year the Graham Medal for his investigation into the composition of water.

Dittmar was a man of great intellectual energy, which always took a practical turn, indeed it is rare to see a man so truly scientific in all the operations of his mind so free from speculation. Not that his imagination was unused, but so prominent before him was the practical result to be obtained, that it gave a character of reality to the whole process by which he sought to reach it.

His most important work was analytical. His great investigation into the composition of the specimens of sea-water collected by the *Challenger* Expedition is a masterpiece of judgment and skill, important not only for its results, but perhaps even more for its methods. We may mention also his determination of the atomic weight of platinum, his method for the analysis of chrome iron ore, his examination of the alkaline hydrates and carbonates, and the gravimetric determination of the composition of water. But he did not confine himself to analytical work. He published along with Kekulé a paper on oxymethylbenzoic acid, the first aromatic alcohol acid; and also while at Bonn obtained glutaric acid by the reduction of Ritthausen's glutanic acid. He did much excellent work in physical chemistry. We may mention the determination of the vapour-pressures of formate of ethyl and acetate of methyl, his work on the dissociation of sulphuric acid and on the relation of the composition of acids of constant boiling-point to the pressure under which they are distilled. He made the construction of the balance a subject of special study, and the balances constructed for him by Oertling and by Staudinger are models of convenience and accuracy.

But Dittmar was greatest as a teacher. Patient and careful, he helped his students where they needed help, and led them to think and work for themselves. He had no ambition to make his pupils analyzing machines; they had to understand all that they did. Gradually his great power as a teacher came to be appreciated, and latterly his laboratory was filled with enthusiastic pupils. Those whom he has trained are his real works on analytical chemistry; but others can learn much of his method from his admirable treatise on qualitative analysis.

A. C. B.

SERENO WATSON.

THE last American mail brought the sad intelligence of the death of this indefatigable botanist, upon whom, in one sense, the mantle of Asa Gray fell barely four years ago. Early in the year he was seized with a bad attack of *grippe*, and although he rallied and was better for a time, he never recovered strength, and finally succumbed on the 9th inst., in the sixty-sixth year of his age. Of his early life we know nothing, but he appears to have published no botanical work previous to 1873, about the period that he was appointed Herbarium Assistant to Dr. A. Gray at Harvard. From that date, however, onward until within a few months of his death, he was, next to Gray, the most active writer on North American Phanerogams. Much of his work appeared originally in the Proceedings of the American Academy of Arts and Sciences, under the title of "Contributions to American Botany," numbered consecutively, the last being the eighteenth. These consist principally of monographs of North American genera and descriptions of novelties. He was also the principal author of the "Botany of California," the last volume of which appeared in 1880; and since the death of Dr. A. Gray, he in conjunction with Prof. J. M. Coulter has edited the sixth edition of the deceased author's valuable "Manual of the Northern United States." This work has been considerably decried by contemporary American botanists, because Watson did not introduce the changes in nomenclature consequent on a strict and unqualified observance of the law of priority. But in this conservatism he doubtless followed the wishes of his former master, and enjoyed the sympathies of those whose experience teaches them that it is much easier to make these changes in books than to carry them into practice. Watson had a still more important work in hand, for he had undertaken the continuation of Gray's "Synoptical Flora of North America." How far this is advanced we do not know, but it is not probable that it will see the light on the same lines as the published volumes, or as he would have continued it. Unfortunately, an exceedingly useful work, commenced during the early part of Watson's engagement at Harvard, was never completed. We allude to his "Bibliographical Index to North American Botany," which was only carried to the end of the Polypetalæ. To a great extent, Gray's "Synoptical Flora" takes its place, so far as the Gamopetalæ are concerned; but it is difficult to find one's way in the remaining groups. Though Sereno Watson was of a retiring disposition, and did not belong to the teaching body, nor take a prominent part in the gatherings of scientific men, yet the loss of him will be widely felt and deplored. He was elected a Foreign Member of the Linnean Society of London in 1890, but he was not a man who craved after honours and distinctions.

NOTES.

IT seems almost incredible that the Treasury should think of stopping the publication of the *Kew Bulletin* simply because it does not quite pay its own expenses. The periodical, as our readers know, is one of high value, both from the scientific and the industrial point of view, and, if the Treasury persists in the design attributed to it, something ought soon to be said on the subject in Parliament by the scientific members. The *Times* has argued strongly against the proposed step, and the view it has expressed will be shared by all who are capable of forming

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an intelligent opinion on the subject. It cannot be expected that a very large number of copies of the *Bulletin* will be sold, as it is really more useful to our colonial Governments than to individuals either here or in the colonies.

THE Council of the Royal Society of New South Wales has awarded to Mr. W. T. Thiselton-Dyer the Clarke Memorial Medal, in recognition of his distinguished services in the cause of botanical science, and especially on account of his labours in connection with the development and organization of the Botanical Departments for the Colonies and India, at the Royal Gardens, Kew. The medal has been forwarded with a letter, dated December 23, 1891, in which Mr. W. H. Warren, the Honorary Secretary, says:—"The Council fully appreciates the beneficial effects which this colony (in common with the other British possessions) has already derived and will continue to derive from the foresight and scientific zeal you have displayed in the building up of the Colonial Departments of your institution; the Council is also aware of the assistance which the Department under your direction has given to institutions in Sydney, and is not unmindful of the fact that the first collections obtained by the Sydney Technological Museum were received from the Museum of the Royal Gardens, Kew. The Council trusts that you will therefore accept the medal, as a token on the part of this Society of the appreciation in which your work is held in Australia." Mr. Warren's letter, with Mr. Thiselton-Dyer's answer, is published in the new number of the *Kew Bulletin*. In a prefatory note the Director of the Royal Gardens explains that in publishing the correspondence he feels "he is only putting on record a mark of appreciation as handsome as it is spontaneous, on the part of one of the most distinguished of the colonies of the Crown, of the usefulness of the official work which the Kew establishment could alone accomplish with the continuous and loyal assistance of every member of its staff."

STUDENTS of archæology will be glad to hear that Mr. F. C. Penrose has gone to Greece to carry on his investigation of the dates of Greek temples as derived from their orientation. He hopes to determine the orientation of many foundations not included in the list given in his recent paper on the subject. He will also verify, as far as possible, the approximate results at which he has already arrived.

ON March 28 many educational institutions in Austria and Germany will celebrate the three hundredth anniversary of the birth of Johann Amos Comenius, one of the most illustrious of pedagogues. The Austrian Government, however, has forbidden the proposed celebration in Bohemia. Comenius was by birth a Moravian. He anticipated many of the best ideas of our own time on education, and by his numerous writings and his great personal influence produced a profound impression on his contemporaries. Charles I. invited him to England to improve the organization of English schools; but the outbreak of the Civil War made it impossible for him to give effect to his ideas in this country.

PROF. GRIESBACH lately forwarded to Vienna various fossils which he had collected during his geological explorations in the Central Himalayas on behalf of the Government of India. They resemble so closely fossils found in corresponding Alpine strata, that they have excited much interest; and the Royal Imperial Academy of Science, Vienna, has determined, with the co-operation of the Indian Government, to send an exploring party to the Central Himalayas to compare their geological features with those of the Eastern Alps. The leader of the party will be Dr. Carl Diener, lecturer on geology at the University of Vienna. Dr. Diener is President of the Vienna Alpine Club, and is well known as the author of a work on the geological structure of the Western Alps. He will start for Brindisi on April 10, taking with him two Tyrol guides. The expedition will last six months.

THE anniversary meeting of the Chemical Society will be held on March 30 at 4 p.m.

DR. GEORGE BUCHANAN, F.R.S., who has long been known as one of the highest authorities on sanitary science, is about to erisgn the post of medical officer to the Local Government Board.

THE following are some of those who have consented to serve on the jury for the Crystal Palace Electrical Exhibition:—Prof. W. Grylls Adams, Prof. W. E. Ayrton, Mr. Shelford Bidwell, Prof. W. Crookes, Major-General Festing, Prof. George Forbes, Captain Sir Douglas Galton, Dr. J. H. Gladstone, Prof. D. E. Hughes, Mr. W. H. Preece, Prof. Silvanus Thompson.

ON Monday evening Mr. Kimber asked the Chancellor of the Exchequer why the British Government had not concurred with the other European Governments in joining the International Geodetic Bureau of Vienna. In reply, Mr. Goschen said the question of joining the reconstituted International Geodetic Bureau was raised just five years ago, the condition being an annual contribution of 2250f. a year for ten years, besides the expense of sending delegates to attend the meetings of the Bureau. "Our experience of the International Metric Bureau at that time," Mr. Goschen continued, "showed that the expenditure upon such undertakings tends to increase out of proportion to their actual utility, and it was considered that the practical advantages of joining the Geodetic Bureau were not sufficient to justify the guaranteeing of the sum named. So far as I am aware, the question has not been mooted since."

THE first annual meeting of the North-West of England Boulder Committee was held at the Technical School, Stockport, on the 19th inst. The year's work has been eminently practical and useful. The Committee, in addition to contributing a very large portion of the Report of the Erratic Block Committee of the British Association presented at Cardiff, have read and discussed more than forty papers and reports at their monthly meetings; these they now propose to print independently. Maps on the one-inch and six-inch scale have been acquired, partly by purchase and partly by presentation, including a valuable set from Sir A. Geikie, F.R.S., Director-General of the Geological Survey. On these good progress has been made, by a distinctive system of symbols, in showing the position and origin of the boulders over a large area. A thoroughly practical "Drift Primer" has been drawn up by the Secretary and approved by the Council of the Committee for the instruction of observers, and has circulated beyond the limits of the Committee. Boulder-forming rocks have been collected in England and Scotland, for reference purposes, and the nucleus of a Glacial Drift library formed. The Annual Report shows a large increase of members outside the original district of observation, and it was therefore decided that henceforth the title should be altered to "The Glacialists' Association," and that the rules be altered so as to include the whole of "the British Isles." The President, Mr. De Rance, and the Secretary, Mr. Percy Kendall, were re-elected, and the following Vice-Presidents were elected: Vice-Chancellor Sir Henry Bristowe, Mr. Brockbank, Mr. Gray, Alderman Kay, and Dr. Ricketts, and a Council of fifteen.

AMONG the contents of the new number of the *Kew Bulletin* is an interesting account of the Spanish Broom as a fibre plant. Some time ago a French scientific journal printed a notice respecting the use of the fibre of the Spanish Broom among the peasants in the neighbourhood of Lodéve, and in the remote hamlets in the mountains of Languedoc. An effort was made to secure speci-

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mens of the fibre, and of articles produced from it, for the collections in the Museums of Economic Botany at Kew. It was with the utmost difficulty that specimens were obtained; but ample material for arriving at a definite conclusion with regard to the origin and character of "Genista fibre" was at last received. There is now in the Kew Museums a complete set, consisting of twigs, fibre in various stages of preparation, as well as yarns and coarse cloths. These were sent by Mr. Consul Perceval. There is also a sample of coarse sheeting received from M. Geoffroy St. Hilaire through the English Embassy at Paris. These fully illustrate the fibre industry connected with *Spartium* (Genista) *junceum*. "It is evident," says the *Bulletin*, "that this interesting rural industry is fast dying out in France. It may be said to exist now only in very remote hamlets in the Cevennes. The inquiries made by Kew were therefore only just in time to secure the last specimens of cloth made in the laborious fashion before the days of rapid communication and the introduction of cheap cotton and other goods."

ONE of the most interesting of recent additions to the Museums of Economic Botany at Kew has recently been received from Sir John Kirk. It consists of a large sheet of bark cloth prepared by the natives of Uganda from the inner bark of a species of *Brachystegia*, a small genus of trees belonging to the *Cæsalpineæ* sub order of the natural order *Leguminosæ*. Various details relating to the use of *Brachystegia* as a source of bark cloth are given in the current number of the *Kew Bulletin*. The same number contains sections on oil palm fibre and the sources of rubber supply.

ANYONE who may desire to devote himself to the study of Finnish archæology and folk-lore will find ample material for study in the information collected by the late Dr. H. A. Reinholm. He was chaplain of the prison, and pastor of the Lutheran congregation, at the Fortress of Sveaborg, near Helsingfors, and for many years devoted the whole of his leisure time to the amassing and arrangement of facts relating to the life of the Finnish people in past times. He died in 1883. Only a few results of his researches have been published. By far the greater part of his work is preserved in manuscript in the Historical Museum at Helsingfors. An interesting account of the labours of this indefatigable investigator is given in the current number of *Globus*.

MR. R. H. SCOTT delivered a lecture at the Royal United Institution on March 18, on a subject of much importance to meteorologists in this country, viz. "Atlantic Weather and its connection with British Weather." He pointed out that less than a quarter of a century ago, before synchronous charts were in vogue, it would have been impossible to have traced a storm across America and the Atlantic to our own coasts; but this can now be done with considerable certainty. The broad principles which govern the weather system of the Atlantic were shown on two diagrams exhibiting the mean pressure, and the regions of greatest disturbance of temperature, on the globe, in our winter. The latter chart showed that, at that season, the relatively warmest district is near Iceland; and the barometer chart showed that close to the same region the barometer is lowest. The reasons of these relations, which involve the first principles of modern weather knowledge, were fully explained. The more northern part of the Atlantic area interests us the most. The whole region from 40° to 70° N. is constantly visited by cyclonic depressions, and in order to throw some light on the origin and history of these depressions, and of the storms which they at times bring with them, various institutions have published daily maps of the weather in the Atlantic. The most complete of these maps were published by the Meteorological Office for thirteen months, commencing with August 1882. The last twelve of these months have been carefully examined, and show no less than 264 depressions in various parts of the ocean. Of these, out of 62 which originated south of 40° N., only 16 had sufficient energy in them to cross the meridian of Greenwich, while out of 22 which originated further south only 11 crossed the Atlantic, and these were not all felt as actual storms in this country. The practical outcome of obtaining telegrams from America has not been satisfactory, but this failure has probably been mainly due to the fact that the reports "have been neither numerous nor full enough." This accurately represents the case at the present time; but we hope it is not too much to expect that, with our present knowledge of the paths taken by depressions with regard to areas of high pressure, some further advance may shortly be made in predicting storms by means of more numerous and fuller telegraphic reports both from outward and homeward bound ships.

THE following are among the lecture arrangements at the Royal Institution for the period after Easter:—Prof. T. G. Bonney, two lectures on "The Sculpturing of Britain—its Later Stages" (the Tyndall Lectures); Mr. Frederick E. Ives, two lectures on "Photography in the Colours of Nature"; Prof. Dewar, four lectures on "The Chemistry of Gases"; Prof. H. Marshall Ward, three lectures on "Some Modern Discoveries in Agricultural and Forest Botany" (illustrated by lantern). The Friday evening meetings will be resumed on April 29, when a discourse will be given by Dr. William Huggins on "The New Star in Auriga"; succeeding discourses will probably be given by Captain Abney, Dr. B. W. Richardson, Mr. J. Wilson Swan, Sir James Crichton-Browne, Mr. Ludwig Mond, Prof. Dewar, and other gentlemen.

A GOOD seam of coal from 7 feet to 8 feet thick has been discovered by Mr. Hughes, of the Indian Geological Survey, on the banks of the Tenasserim River, which is navigable to that point. The Government of India has sanctioned the grant of a large concession in Mergui to Ah Kwi, a wealthy Chinese resident of the Straits, to prospect for tin. According to the Calcutta correspondent of the *Times*, this is the first attempt to encourage on a large scale the mining industry in Mergui.

MESSRS. CROSBY LOCKWOOD AND CO. have issued the fourth edition of Mr. Primrose McConnell's "Note-book of Agricultural Facts and Figures for Farmers and Farm Students." The author was originally induced to prepare the volume by noticing the great value of Molesworth's "Pocket-book of Engineering Formulæ" to engineers, and of similar books to those engaged in other professions. It occurred to him that a book compiled in the same style, and devoted to farming matters, could not fail to be useful as a ready means of reference for refreshing the memory. The success of the "Note-book" has proved that he was right. The progress of agricultural practice and science has been so rapid that it has been necessary for him to rewrite the greater part of the book, and nearly twice as much matter is given in the present edition as was contained in the earlier issues. The use of a slightly longer page and thinner paper has prevented the size of the volume from being much increased.

THE Agricultural Research Association for the north-eastern counties of Scotland has issued its annual report for 1891. It includes a valuable paper on "Root Hairs," in which Mr. T. Jamieson presents the results of a laborious investigation he has carried on during the past three years. He also gives some hints on permanent pasture, and brings together various items of information which are likely to be of immediate benefit to farmers.

IF we may judge from its twenty-sixth annual report, the American Society for the Prevention of Cruelty to Animals is

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doing much good work. During the past year it prosecuted no fewer than 17,847 cases in the courts. Through its efforts 49,118 disabled animals were temporarily suspended from work; 34,264 horses, disabled past recovery, were humanely destroyed; 6444 disabled horses were removed from the streets in an ambulance. The number of prosecutions and other official interferences was larger than in previous years, but it does not follow that cruelty is more common than it was. The increase is due to the greater vigilance of the Society's officers.

IN a paper contributed to the current number of the Journal of the Franklin Institute, Prof. Lewis M. Haupt argues strongly in favour of the construction of a ship canal between New York and Philadelphia, connecting the Delaware and Raritan Rivers. Such a canal would, he maintained, extend the Erie Canal and its benefits to Philadelphia, and open to its manufacturers over 16,000 miles of waterways in the great basin of the Mississippi. It would reduce the distance by water to New York harbour from 240 to about 60 miles, would afford an inside and safe passage to Eastern, Sound, and Hudson River ports; would develop a large population along the entire route, and so benefit the railroads traversing the district. "In short," says Prof. Haupt, "the effect would be to reduce the rate per mile, as well as to shorten the distance between the two greatest centres of population on the American continent, or, we may say, in the world; for nowhere else on the globe is it possible by so short and inexpensive a waterway to connect such large populations and so many and valuable interests."

THE Bethlehem Iron Company, Pennsylvania, is to erect at the Chicago Exhibition a full-size model of its 125-ton steam hammer, said to be the largest in the world. It will span the main avenue of Machinery Hall, and will rise to a height of 90 feet. At the last Paris Exhibition great attention was attracted by a similar model shown by the Creusot works, but representing only a 100-ton hammer.

BARON VON MUELLER records, in the *Victorian Naturalist* for February, that, while elaborating diagnoses of new Papuan plants, he was pleasantly surprised to find among the novelties an Antholoma. This genus has hitherto been supposed to be restricted to New Caledonia. The Papuan species is dedicated to Prof. van Tieghem. The denticulation of the leaves, the elongation of the setule of the anthers and the three-celled ovulary already separate *A. Tieghemi* from *A. montanum*. Among the novelties are also *Oxalis* (*Biophytum*) *albiflora*, *Sloanea Forbesii*, which approaches *S. quadrivalvis* in many respects, but is petaliferous, and *Quintinia Macgregori* is particularly remarkable.

A "TREATISE on Physical Optics," by Mr. A. B. Basset, will be issued shortly by Messrs. Deighton, Bell, and Co.

THE proper title of Mr. A. E. H. Love's work (included in our list of forthcoming scientific books last week) is "A Treatise on the Mathematical Theory of Elasticity."

MESSRS. NALDER BROS. AND CO. have issued price lists, carefully illustrated, of their electrical testing and other scientific instruments, and of their ammeters and voltometers, resistance frames, &c.

MESSRS. DULAU AND CO. have issued Part xvii. of their "Catalogue of Zoological and Palæontological Books." It contains lists of works on Mollusca and Molluscoida.

IN Mr. George S. Carr's letter on the terms "centrifugal force" and "force of inertia" (NATURE, p. 463), in the second sentence of the second paragraph, read "in every case as the reaction to the normal component of the *centripetal* force" (not "*centrifugal*").

AT the meeting of the Belgian Academy of Sciences on March 6, Prof. Spring announced that the late Prof. Stas had left, in an almost completed condition, a long and important memoir describing the results of several further stöchiometrical investigations. It is entitled "Silver," and will forthwith be edited, presumably by Dr. Spring, and published. It may be remembered that, after the publication of Prof. Stas's classical memoir upon the preparation of absolutely pure silver and the atomic weight of that metal, doubts were thrown by Prof. Dumas on the validity of the work on the ground that the silver employed was not free from occluded atmospheric gases. Moreover, Prof. Dumas expressed doubts as to the bearing of the work upon the celebrated hypothesis of Prout, according to which the atomic weights of all the other elements are supposed to be multiples of that of hydrogen. For, if silver possessed the atomic weight attributed to it by Prof. Stas, the atomic weight of oxygen became 15·96 and not the whole number 16, and consequently Prout's hypothesis in its original form would be negatived. In order to set these doubts at rest, and to leave his work in a perfected condition, Prof. Stas has prepared a quantity of silver with such extreme precautions that he has succeeded in obtaining it entirely free from occluded gases, and from even the minutest traces of the materials of the vessels employed. So perfect is the purity of this silver that even when heated to the temperature of the melting-point of iridium not a trace of sodium can be detected in the spectrum of the vapour. With this silver he has repeated his former determinations of the atomic weight of the metal, and it is satisfactory to learn that the final number obtained is, as Prof. Stas himself expected it would be, identical with that formerly obtained. Hence, the objection of Prof. Dumas cannot longer be entertained, and the atomic weight of oxygen would indeed appear to be 15·96 and not 16, for the numbers obtained by Prof. Stas agree so remarkably that an error of four-hundredths of a unit would apparently be out of the question. In addition to this important memoir, Prof. Stas has also left the data of a series of twelve separate determinations of the stöchiometric relation of silver to potassium chloride, the materials for which were the pure silver just described, and a specimen of potassium chloride, also prepared with a care and precaution quite in keeping with the rest of the work of the great analyst. The results of these determinations are described by Prof. Spring as agreeing in a most wonderful manner, and will afford another valuable base to which the atomic weights of many other elements may be referred. Besides these two memoirs, a third is mentioned by Prof. Spring, relating to the spectra of several metals which Prof. Stas has obtained in the purest state in which these metals have ever probably been seen. The whole of these memoirs, consisting of about fifteen hundred pages of manuscript, it is intended to publish forthwith in three separate treatises.

THE additions to the Zoological Society's Gardens during the past week include a Ring-necked Parrakeet (*Palæornis torquatus* ♂) from India, presented by Mr. George H. Whitaker; a Grey-breasted Parrakeet (*Bolborhynchus monachus*) from Monte Video, presented by Miss Mildred Whitaker; a Roseate Cockatoo (*Cacatua roseicapilla*) from Australia, presented by Mr. J. S. Gibbons; a Nutmeg Fruit Pigeon (*Carpophaga bicolor*) from the Torres Straits, presented by Mrs. Fitzgerald; two Pike (*Esox lucius*) from British Fresh Waters, presented by Mr. P. F. Coggin; a Manchurian Crane (*Grus viridirostris*) from North China, deposited.

OUR ASTRONOMICAL COLUMN.

FUZZINESS OF SOME VARIABLE STARS.—Mr. Cuthbert G. Peek has, during the last six years, used his 6½-inch achromatic for the investigation of the light-curves of variable stars. In

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this month's *Knowledge* he describes some observations of changes in appearance of a few variable stars at different epochs. Three variables—T Cassiopeiæ, R Cassiopeiæ, and S Herculis—have been frequently observed as (*a*) remarkably well defined, almost planetary, disks; (*b*) well-defined stars, surrounded by a more or less dense, ruddy atmosphere; (*c*) large, woolly, ill-defined images, resembling a small but bright planetary nebula; (*d*) at minimum, in place of the variable, a slight bluish nebulosity. The changes appear to be real, for stars near the places of the variables have been seen clear and sharp when the haziness of the variables was unmistakable. Other stars with regard to which Mr. Peek has made similar observations are S Cassiopeiæ, R Tauri, R Aurigæ, V Cancri, R Ursæ Majoris, S Ursæ Majoris, R Camelopardi, R Boötis, S Coronæ, R Aquilæ, and S Cephei.

ASTRONOMICAL POSSIBILITIES AT CONSIDERABLE ALTITUDES.—Prof. Pickering, in No. 3079 of the *Astronomische Nachrichten*, relates some interesting facts in an article on "Astronomical Possibilities at Considerable Altitudes." They are gleaned from observations made at the Boyden Station of the Harvard College Observatory, which is situated two miles from the city of Arequipa, Peru, in latitude 16° 24′ S., and longitude 4h. 45m. 30s. W. of Greenwich, and at an altitude of 8060 feet above sea-level. The air there is so clear and steady that 6·5 magnitude stars are picked out by the naked eye with great ease, and, when the moon is not too bright, the eleven Pleiads can always be counted. The nebula in Andromeda forms also a very conspicuous object, "appearing larger than the moon," while, in the 13-inch Clark refractor, "the whole photographic region of the great Orion nebula, first shown in the Harvard photographs of 1887, is clearly visible to the eye," rendering it the "most splendid object in the stellar universe." The steadiness of the atmosphere is also very much remarked there, so much so that a scale of steadiness has been adopted. Some of the brightest stars have been noticed to have as many as six complete diffraction rings round them; while around these, when the seeing was denominated as "perfect," twelve rings have been counted. "Boiling" was also found to be sometimes completely eliminated, for, in observing bodies of the solar system with a 13-inch and a power of 400, "it was frequently impossible to detect any wavering of the edges of the disk."

The conclusion that Prof. Pickering comes to with regard to the position of future Observatories is that "moderate altitude is a most desirable qualification," while "for transparent skies one must approach the tropics, and for steady seeing one must have an extremely dry climate."

INCREASE OF THE EARTH'S SHADOW DURING LUNAR ECLIPSES.—In a memoir with the title "Die Vergrösserung des Erdschattens bei Mondfinsternissen" (*Abhandlungen der math. phys. Classe der k. Sachsischen Ges. d. Wissenschaften*, vol. xvii., Lepzig, 1891), Dr. Hartmann published the results of an investigation into the amount by which the earth's atmosphere increases the diameter of the section of the shadow during a lunar eclipse. An abstract of the memoir appears in the annual report of the Royal Astronomical Society, which has just been issued. Since the time of Tobias Mayer (1750) the coefficient $\frac{1}{50}$ has been assumed to represent this increase, although nothing is known as to the manner in which this quantity was determined. Dr. Hartmann has reduced all the observations of lunar eclipses observed independently by several astronomers during this century, and has deduced the increase of the diameter of the shadow from them. The result of a comprehensive discussion of 2920 observations of the contact of the shadow with well-defined lunar formations is, that the increase of the semi-diameter of the shadow is 48″·62 for mean lunar parallax. This corresponds to a coefficient of increase $= \frac{1}{50{\cdot}79}$. The result may perhaps be changed 2″ or 3″ by a discussion of new observations, but not more, so it seems desirable that the value of $\frac{1}{50}$ should be used, when required, instead of Mayer's value of $\frac{1}{50}$.

THE NEW STAR IN AURIGA.

THE new star is rapidly getting more and more difficult of observation in consequence of its waning light. There is very little change in its spectrum, and what change there is is not in the direction recorded of Nova Cygni, so it seems pretty clear that the new body was not a hitherto unobserved nebula to begin with.

The *Astronomische Nachrichten*, No. 3079, contains (at p. 109) the following communication from Mr. H. C. Vogel, Director of the Astro-physical Observatory at Potsdam, dated February 29:—

"Although the spectroscopic observations of the Nova in Auriga are not yet concluded—since the star will probably continue visible for some time—I consider it of importance, in the interest of the subject, to communicate my observations made hitherto, and the conclusions drawn therefrom, even though the latter should not in the future be confirmed in all points.

"Concerning, first, the direct spectroscopic observations, I have, on February 20, observed the Nova with a compound spectroscope of a dispersion sufficient just to show the nickel line between the D lines. The hydrogen lines C, F, and H_7 appeared bright. Their identification was easy by means of a hydrogen tube in front of the slit. These three lines did not exactly coincide with the lines of the comparison spectrum, but were displaced considerably towards the red, without, however, separating completely from the artificial lines, since they were very broad. The continuous spectrum appeared faint, owing to the comparatively high dispersion; and with certainty only the dark broad F line was recognizable, situate towards the more refrangible side, distinctly separated from the bright line in the spectrum.

"Between C and F, a large number of bright lines could be seen, but most of them were too faint to be fixed with certainty. In the case of two brighter lines near F, myself and Mr. Frost, who assisted in the observations, succeeded in making very certain wave-length determinations; we found 492·5 $\mu\mu$ for the fainter of the two lines, which appeared broad and fuzzy on both edges, and 501·6 $\mu\mu$ for the brighter line. The limit of error is to be taken at about $\pm$ ·3 $\mu\mu$, and it results from the observation with certainty that the brighter line is *not* identical with the double line of the air spectrum or with the brightest line of the nebula, and still less the other with the second nebula line. From Young's list of lines most frequent in the chromosphere, it follows that near F only the two groups of lines, 501·87, 501·59, and 493·44, 492·43, 492·24, 491·92 frequently appear bright. There is no doubt that both lines in the spectrum of the Nova are chromosphere lines, and this result appears to me of great importance, in so far as it is made probable that the line observed in Nova Cygni (1876)—W.L. 500 $\mu\mu$ $\pm$ 1 $\mu\mu$—which, during the gradual fading of the star, alone remained, was a chromosphere line, and not the nebula line.

"Further, both myself and Mr. Frost saw probably the magnesium lines, certainly the sodium lines bright, as also two lines between *b* and D, one of which probably was the well-known chromosphere line W.L. 531·72, also observed in Nova Cygni. By direct comparison with the hydrocarbon spectrum, the brightest band of which nearly coincides with the *b* group, and with the sodium flame, *b* and D were identified. Mr. Frost could see a displacement of the D lines in the star spectrum with respect to the comparison spectrum. There was no indication of hydrocarbon bands in the spectrum of the Nova.

"Up to the present eleven mostly very good spectrographic photographs have been taken; they were obtained by means of a small spectrograph connected to the photographic refractor of 34 cm. aperture. The dispersion is only small, but in the small spectrum of 10 mm. length, extending from F to H, much detail is discernible. The illuminating power of the apparatus is so great, in spite of the narrow slit employed, that even now an exposure of 40 minutes is sufficient to obtain an image suitable for measurement. The bright hydrogen lines F, $H\gamma$, *h*, H, and the calcium line H_2, are very broad; and, as already announced, the corresponding dark lines of a second spectrum are displaced with respect to the bright lines towards the violet, and in spite of the breadth of the latter, are almost entirely separated. There are still some of the hydrogen lines in the ultra-violet visible, but they are too faint for any approximately certain observation.

"In the last few days the spectrum has changed, inasmuch as in the broad bright lines $H\gamma$, *h*, H, and H_2 (F is only traced on plates which are over-exposed for the middle of the photographic spectrum), two maxima of intensity are plainly discernible, and, as in each of the corresponding dark lines, a narrow bright line has appeared. From the measurements, a connection between these and the hydrogen lines appears beyond doubt, and it is

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not improbable that these linear brightenings in the broad dark lines indicate eruptions of gases from the interior of the body possessing the continuous spectrum with the dark absorption lines. Such brightenings are occasionally seen in the spectra of sun-spots. On this supposition, the fine bright lines would indicate very nearly the middle of the dark lines.

"The appearance of two maxima of intensity in the broad bright lines admits of the conclusion that two bodies with different motions possess spectra with bright lines, and that therefore the spectrum of the Nova consists of at least three spectra superposed, from the measurement of which, in connection with the comparison spectra of β Aurigæ or β Tauri on the same plate, the relative motions of the three supposed bodies, as well as their motions with respect to the earth, can be determined. Denoting the body with the dark-line spectrum by *a*, the two others with bright-line spectra by *b* and *c*, measurements by myself and Dr. Scheiner have given the following results:

$$a - \tfrac{1}{2}(b + c) = 120 \text{ miles},^1$$

$$b - c = 70 \text{ miles}.$$

and further with respect to the earth—

$$a = -90 \text{ miles}, \; b = -5, \; c = +65 \text{ miles}.$$

"This result is still very uncertain, and must be regarded as quite preliminary, for it is evident that with the small size of the spectra the accuracy cannot be pushed very far—a displacement of ·01 mm. corresponds, for instance, to a motion of 8 to 12 miles, according to the situation of the line in the spectrum—and that the size of the silver grain in the photographs can exert a very marked influence on the measurements.

"In the photographic spectrum of the Nova, besides the broad lines mentioned, several more bright and mostly very broad lines can be seen, whose wave-lengths I intend to communicate later on."

Prof. Pickering communicates some valuable information to the same number of the *Astronomische Nachrichten* with reference to the visibility of the Nova before its discovery by Dr. Anderson. In eighteen photographs of this region, which were taken by the 8-inch photographic telescopes between the dates November 3, 1885, and November 2, 1891, no star in the Nova's place was visible, but in those taken from December 16, 1891, to January 31, 1891, there was a star of the fifth magnitude recorded. In another series of plates taken with the transit photometer, no record of the new star up to December 1, 1891, was obtained, although χ Aurigæ (mag. 5·0 m.) was always visible, but the plates taken on the nights of December 10, 1891, and ending January 20, 1892, indicated clearly the position of the new star.

Careful examination has been made on all the above-mentioned plates, and the following extract shows the series of magnitudes which have been deduced from the measurements:—

"It appears that the star was fainter than the eleventh magnitude on November 2, 1891, than the sixth magnitude on December 1, and that it was increasing rapidly on December 10. A graphical construction indicates that it had probably attained the seventh magnitude within a day or two of December 2, and the sixth magnitude December 7. The brightness increased rapidly until December 18, attaining its maximum about December 20, when its magnitude was 4·4m. It then began to decrease slowly, with slight fluctuations, until January 20, when it was slightly below the fifth magnitude."

From this it will be seen that two months' observations have been lost, owing to its late discovery.

ABERRATION.[2]

UNDER this head may conveniently be considered not only the apparent displacement of the stars discovered by Bradley, but other kindred phenomena dependent upon the velocity of light bearing but a finite ratio to that of the earth in its orbit round the sun, and to other astronomical velocities.

The explanation of stellar aberration, as usually given, proceeds rather upon the basis of the corpuscular than of the wave theory. In order to adapt it to the principles of the latter theory, Fresnel found it necessary to follow Young in assuming that the æther in any vacuous space connected with the earth (and therefore practically in the atmosphere) is undisturbed by the earth's motion of 19 miles per second. Consider for simplicity the case in which the direction of the star is at right angles to that of the earth's motion, and replace the telescope, which would be used in practice, by a pair of perforated screens, on which the light falls perpendicularly. We may further imagine the luminous disturbance to consist of a single plane pulse. When this reaches the anterior screen, so much of it as coincides with the momentary position of the aperture is transmitted, and the remainder is stopped. The part transmitted proceeds upon its course through the æther independently of the motion of the screens. In order, therefore, that the pulse may be transmitted by the aperture in the posterior screen, it is evident that the line joining the centres of the apertures must not be perpendicular to the screens and to the wave front, as would have been necessary in the case of rest. For in consequence of the motion of the posterior screen in its own plane the aperture will be carried forward during the time of passage of the light. By the amount of this motion the second aperture must be drawn backwards, in order that it may be in the place required when the light reaches it. If the velocity of light be V, and that of the earth be *v*, the line of apertures, giving the apparent direction of the star, must be directed forwards through an angle equal to v/V. More generally, if the angle between the star and the point of the heavens towards which the earth is moving be α, there will be an apparent displacement towards the latter point, expressed by $\sin \alpha \, . \, v/V$, and independent of the position upon the earth's surface where the observation is made. The ratio v/V is about $\frac{1}{10\frac{1}{100}}$.

The aperture in the anterior screen corresponds to the object-glass of the telescope with which the observation would actually be made, and which is necessary in order to produce agreement of phase of the various elementary waves at a moderately distant focal point. The introduction of a refracting medium would complicate the problem, and is not really necessary for our present purpose. As has been shown (*Philosophical Magazine*, March 1881, "On Images formed without Reflection or Refraction"), the only use of an object-glass is to shorten the focal length. Our imaginary screens may be as far apart as we please, and if the distance is sufficient, the definition, and consequently the accuracy of alignment, is as great as could be attained with the most perfect telescope whose aperture is equal to that in the anterior screen.

It appears, then, that stellar aberration in itself need present no particular difficulty on the wave theory, unless the hypothesis of a quiescent æther at the earth's surface be regarded as such. But there are a variety of allied phenomena, mostly of a negative kind, which require consideration before any judgment can be formed as to the degree of success with which the wave theory meets the demands made upon it. In the first place, the question arises whether terrestrial optical phenomena could remain unaffected by the supposed immense relative motion of our instruments and of the æther; whether reflection, diffraction, and refraction, as ordinarily observed by us, could be independent of the direction of the rays relatively to the earth's motion. It may be stated at once that no such influence has been detected, even in experiments carefully designed with this object in view.

Another class of experiments, with the results of which theory must be harmonized, are those of Fizeau and Michelson upon the velocity of light in ponderable refracting media which have a rapid motion (relatively to the instruments and other surrounding bodies) in the direction of propagation, or in the opposite direction. These very important researches have proved that in the case of water the velocity of the ponderable medium is not without effect; but that the increment or decrement of the velocity of propagation is very decidedly less than the velocity of the water. On the other hand, the motion of air, even at high velocities, has no perceptible influence upon the propagation of light through it.

Again, it has been found by Airy,[1] as the result of an experiment originally suggested by Boscovitch, that the constant of stellar aberration is the same, whether determined by means of a telescope of the ordinary kind, or by one of which the tube is filled with water. It is clear that, according to Fresnel's views

[1] = about 540 English miles.—*Tr.*

[2] This paper was written in 1887, when I was occupied with my article upon "Wave Theory" for the "Encyclopædia Britannica," and at a time when a more extensive treatment was contemplated than was afterwards found practicable. Friends on whom I can rely are of opinion that its publication may be useful; and, as I am not able to give it a complete revision, I prefer to let it stand under its original date, merely warning the reader that very important work has since been published by Michelson.—*January* 1892.

[1] Proc. Roy. Soc., xx., 1872, p. 35; xxi., 1873, p. 121.

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of the condition of the æther at the earth's surface, this agreement must involve some particular supposition as to the propagation of light in moving refracting media.

The theory of these phenomena must evidently turn upon the question whether the æther at the earth's surface is at rest, absolutely, or relatively to the earth;[1] and this fundamental question has not yet received a certain answer. The independence of terrestrial optical phenomena of the earth's motion in its orbit is, of course, more easily explained upon the latter alternative; or rather no explanation is required. But in that case the difficulty is thrown upon stellar aberration, which follows a more simple law than we should expect to apply in the case of an æther disturbed by the passage of a body in its neighbourhood. Prof. Stokes has, indeed, attempted a theory on these lines,[2] by supposing the ætherial motion to be what is called in hydrodynamics irrotational. In strictness there is, however, no such motion possible, subject to the condition of vanishing absolutely at a great distance, and relatively at the earth's surface; and it does not appear that the objection thus arising can be satisfactorily met.

If we start from the experimental facts which have the most direct bearing upon the question under discussion, we are led to regard Fresnel's views (doubtless in some generalized form) as the more plausible. From the results of Fizeau and Michelson relative to air, we may conclude with tolerable confidence that a small mass of ponderable matter, of very low refracting power, moving in space, would not appreciably carry the æther with it. The extension of the argument to a body as large as the earth is not unnatural, though it involves certainly an element of hypothesis. In like manner, if the globe were of water, we should expect the æther to be carried forward, but not to the full amount. The simplest supposition open to us is that, in any kind of ponderable matter, forming part of a complex mass, the æther is carried forward with a velocity dependent upon the local refracting power, but independent of the refracting power and velocity of other parts of the mass. In the earth's atmosphere, where the refracting power is negligible, the æther would be sensibly undisturbed.

If we agree to adopt this point of view provisionally, we have next to consider the relation between the velocity of luminous propagation in moving ponderable matter and the refractive index. The character of this relation was discovered by Fresnel, whose argument may be thrown into the following form.

Consider the behaviour of the æther when a plate of ponderable matter (index $= \mu$) is carried forward through vacuum with velocity v in a direction perpendicular to its plane. If D be the density of the æther in vacuum, and D_1 the density in the refracting medium, then, according to Fresnel's views as to the cause of refraction, $D_1 = \mu^2 D$. The æther is thus condensed as the plate reaches it; and if we assume that the whole quantity of æther is invariable, this consideration leads to the law giving the velocity (xv) with which the denser æther within the plate must be supposed to be carried forward. For conceive two ideal planes, one in the plate and one in the anterior vacuous region, to move forward with velocity v. The whole amount of æther between the planes must remain unchanged. Now, the quantity entering (per unit area and time) is Dv, and the quantity leaving is $D_1(v - xv)$. Hence,

$$x = 1 - \mu^{-2},$$

so that the velocity with which the æther in the plate is carried forward is $v(1 - \mu^{-2})$, tending to vanish as μ approaches unity. If V be the velocity of light in vacuum, and V/μ the velocity in the medium at rest, then the absolute velocity of light in the moving medium is

$$V/\mu \pm v(1 - \mu^{-2}). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Whatever may be thought of the means by which it is obtained, it is not a little remarkable that this formula, and no other, is consistent with the facts of terrestrial refraction, if we once admit that the æther in the atmosphere is at absolute rest. It is not probable that the æther, in moving refracting bodies, can properly be regarded as itself in motion; but if we knew more about the matter we might come to see that the objection is verbal rather than real. Perhaps the following illustration may assist the imagination. Compare the æther in vacuum to a stretched string, the transverse vibrations of which represent light. If the string is loaded, the velocity of propagation of waves is diminished. This represents the passage of light through stationary refracting matter. If now the loads be imagined to run along the string with a velocity not insensible in comparison with that of waves, the velocity of the latter is modified. The substitution of a membrane for a string will allow of a still closer parallel. It appears that the suggested model would lead to a somewhat different law of velocity from that of Fresnel; but in bringing it forward the object is merely to show that we need not interpret Fresnel's language too literally.

We will now consider a few examples of the application of the law of velocity in a moving medium; and first to the experiment of Boscovitch, in which stellar aberration is observed with a telescope filled with water. We have only to suppose the space between the two screens of our former explanation to be occupied by water, which is at rest relatively to the screens. In consequence of the movement of the water, the wave, after traversing the first aperture, is carried laterally with the velocity $v(1 - \mu^{-2})$, and this is to be subtracted from the actual velocity v of the aperture in the posterior screen. The difference is $\mu^{-2}v$. The ratio of this to the velocity of light in water (V/μ) gives the angular displacement of the second aperture necessary to compensate for the motion. We thus obtain $\mu^{-1}v/V$. This angle, being measured in water, corresponds to v/V in air; so that the result of the motion is to make the star appear as if it were in advance of its real place by the angle v/V, precisely as would have happened had the telescope contained air or vacuum instead of water.

We will now calculate the effect of the motion of a plate perpendicular to its own plane upon the retardation of luminous waves moving in the same (or in the opposite) direction. The velocity of the plate is v, its index is μ, and its thickness is a. Denoting, as before, the velocity of the æther within the plate by xv, and supposing, in the first place, that the signs of v and V are the same, we have, for the absolute velocity of the wave in the plate,

$$V/\mu + xv.$$

We have now to express the time (t) occupied by the wave in traversing the plate. This is not to be found by simply dividing a by the above written velocity; for during the time t the anterior face of the plate (which the wave reaches last) is carried forward through the distance vt. Thus, to determine t we have

$$(V/\mu + xv)t = a + vt,$$

whence

$$\frac{Vt}{a} = \frac{\mu}{1 + (x - 1)\mu v/V}. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

The time, t_0, which would have been occupied in traversing the same distance ($a + vt$), had the plate been away, is given by

$$Vt_0 = a + vt;$$

so that

$$\frac{Vt_0}{a} = 1 + \frac{\mu v/V}{1 + (x - 1)\mu v/V}. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Thus

$$\frac{V(t - t_0)}{a} = \frac{\mu(1 - v/V)}{1 + (x - 1)\mu v/V} - 1. \quad . \quad . \quad . \quad . \quad (4)$$

Substituting in this Fresnel's value of x, viz. $(1 - \mu^{-2})$, and neglecting as insensible the square of v/V, we find

$$V(t - t_0) = (\mu - 1)a(1 - v/V). \quad . \quad . \quad . \quad . \quad . \quad (5)$$

If we suppose that part of the original wave traverses the plate, and that part passes alongside, (5) gives the relative retardation—that is, the distance between the wave fronts which were originally in one plane. It would appear at first sight that this result would give us the means of rendering v evident. For the retardation, depending upon the sign of v/V, will be altered when the direction of the light is reversed, and this we have it in our power to bring about by simply turning our apparatus through 180°. A more careful examination will, however, lead us to a different conclusion.

The most obvious way of examining the retardation would be to use homogeneous light, and, by producing regular interference of the two portions, to observe the position of the fringes, and any displacement that might result from a shift of the apparatus relatively to the direction of the earth's motion. But if we employ for this purpose a terrestrial flame, *e.g.* that of a Bunsen's burner containing sodium, we have to take into

[1] An accusation of crudeness might fairly be brought against this phraseology; but an attempt to express the argument in more general language would probably fail, and would in any case be tedious.

[2] *Phil. Mag.*, xxviii., 1846, p. 76; xxix., 1846, p. 6.

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account the fact that the source is itself in motion. For it is evident that the waves which pass in a given time through any point towards which the source is moving are more numerous than had the source been at rest, and that the wave-lengths are correspondingly shortened. If v be the velocity of the source, the wave-length is changed from λ to $\lambda(1 - v/V)$. At a point behind, from which the source is retreating, the wave-length is $\lambda(1 + v/V)$. We shall have occasion to refer again to this principle, named after Döppler, as applied by Huggins and others to the investigation of the motion of the heavenly bodies in the line of sight.

Referring now to (5), we see that, although the *absolute* retardation is affected by v, yet that the retardation *as measured in wave-lengths* remains unaffected. If, then, there be, in the absence of v, an agreement of phase between the two interfering beams, the introduction of v will cause no disturbance. Consequently no shifting of the interference bands is to be expected when the apparatus is turned so that the direction of propagation makes in succession all possible angles with that of the earth's motion.

The experiment has been modified by Hoek,[1] who so arranged matters as to eliminate the part of the retardation independent of v. As before, of two parallel beams A and B, one, A, passes through a plate of refracting medium; the other, B, through air. The beams are then collected by a lens, at the principal focus of which is placed a mirror. After reflection by this mirror, the beams exchange paths, B returning through the plate, and A through air. Apart, therefore, from a possible effect of the motion, there would be complete compensation and no final difference of path. As to the effect of the motion, it would appear at first sight that it ought to be sensible. During the first passage, A is (on account of v) accelerated; on the return, B is retarded; and thus we might expect, upon the whole, a relative acceleration of A equal to $(\mu - 1)d \cdot 2v/V$. But here, again, we have to consider the fact that another part of the apparatus, viz. the mirror, partakes in the motion. In the act of reflection the original retardation of A is increased by twice the distance through which the mirror retreats in the interval between the arrival of the two waves. This distance is (with sufficient approximation) $(\mu - 1)d \cdot v/V$; so that the influence of the movement of the mirror just compensates the acceleration of A which would have resulted in the case of a fixed mirror. On the whole, then, and so long as the square of v/V may be neglected, no displacement of fringes is to be expected when the apparatus is turned. The fact that no displacement was observed by Hoek, nor in an analogous experiment by Mascart,[2] proves that if the stationary condition of the æther in terrestrial vacuous spaces be admitted, we are driven to accept Fresnel's law of the rate of propagation in moving refracting media.

What is virtually another form of the same experiment was tried by Maxwell,[3] with like negative results. In this case, prisms were used instead of plates; and the effect if existent, would have shown itself by a displacement of the image of a a spider-line when the instrument was turned into various azimuths.

On the basis of Fresnel's views it may, in fact, be proved generally that, so far as the first power of v/V is concerned, the earth's motion would not reveal itself in any phenomenon of terrestrial refraction, diffraction, or ordinary refraction. The more important special cases were examined by Fresnel himself, and the demonstration has been completed by Stokes.[4] Space will not allow of the reproduction of these investigations here, and this is the less necessary, as the experiment of Hoek, already examined, seems to raise the principal question at issue in the most direct manner.

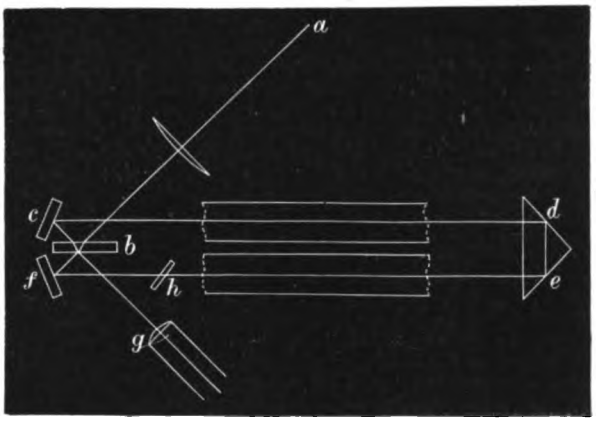
Another point remains to be touched upon. We have hitherto neglected *dispersion*, treating μ as constant. In stationary dispersing media, μ may be regarded indifferently as a function of the wave-length or of the periodic time. When, however, the medium is in motion, the distinction acquires significance; and the question arises, What value of μ are we to understand in the principal term V/μ of (1)? Mascart points out that the entirely negative results of such experiments as those above described indicates that, in spite of the difference of wave-length due to the motion, we must take the same value of μ as if the medium and the source had been at rest, or that μ is to be regarded as a function of the *period*.

Mascart has experimented also upon the influence of the earth's motion upon double refraction, with results which are entirely negative. The theoretical interpretation must remain somewhat ambiguous, so long as we remain in ignorance of the mechanical cause of double refraction.

Reference has already been made to the important experiments by Fizeau and by Michelson upon the velocity of light in moving media. The method, in its main features, is due to the former,[1] and is very ingeniously contrived for its purpose. Light issuing from a slit is rendered parallel by a collimating lens, and is then divided into two portions, which traverse tubes containing running water. After passing the tubes, the light falls upon a focussing lens and mirror (as in Hoek's experiment), the effect of which is to interchange the paths. Both rays traverse both tubes; and, consequently, when ultimately brought together, they are in a condition to produce interference bands. If now the water is allowed to flow through the tubes in opposite directions, one ray propagates itself throughout *with* the motion of the water, and the other *against* the motion of the water; and thus, if the motion has any effect upon the velocity of light, a shift of the bands is to be expected. This shift may be doubled by reversing the flow of water in the tubes.

Fizeau's investigation has recently been repeated in an improved form by Michelson.[2]

"Light from a source at *a* falls on a half-silvered surface, *b*, where it divides; one part following the path *b c d e f b g*, and the other the path *b f e d c b g*. This arrangement has



the following advantages: (1) it permits the use of an extended source of light, as a gas flame; (2) it allows any distance between the tubes which may be desired; (3) it was tried by a preliminary experiment, by placing an inclined plate of glass at *h*. The only effect was either to alter the width of the fringes, or to alter their inclination; *but in no case was the centre of the central white fringe affected.* Even holding a lighted match in the path had no effect on this point.

"The tubes containing the fluid were of brass, 28 mm. internal diameter; and in the first series of experiments, a little over 3 metres in length, and in the second series a little more than 6 metres."

Even with the longer tubes and the full velocity (about 8 metres per second) the displacement on reversal amounted to less than the width of a fringe. Nevertheless, fairly concordant results were arrived at; and they showed that the fraction (x) of the velocity of the water (v) by which the velocity of light is altered is ·434, with a possible error of ± ·02. The numerical value of the theoretical expression is

$$x = 1 - \mu^{-2} = \cdot 437,$$

in very close accordance.

"The experiment was also tried with air moving with a velocity of 25 metres per second. The displacement was about ·01 of a fringe; a quantity smaller than the probable error of

[1] *Archives Néerlandaises*, t. iii. p. 180 (1868); t. iv. p. 443 (1869).
[2] *Ann. de l'École Normale*, t. iii. (1874).
[3] Phil. Trans., 1868, p. 532.
[4] *Phil. Mag.*, xxviii. p. 76 (1846). See also Mascart, *Ann. de l'École Norm.*, t. i. (1872), t. iii. (1874); and Verdet, "Œuvres," t. iv., deuxième partie.

[1] *Ann. de Chimie*, III. lvii. (1859).
[2] *American Journal*, vol. xxxi. p. 377 (1886).

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observation. The value calculated from $(1 - \mu^{-2})$ would be ·0036."

We have seen that, so far as the first power of v/V is concerned, Fresnel's theory agrees with all the facts of the case. The question whether it is possible to contrive an experiment in which v^2/V^2 shall be sensible, has been considered by Michelson,[1] who, having arrived at an affirmative conclusion, proceeded to attack this very difficult experimental problem. In Michelson's apparatus interference is brought about between two rays, coming of course originally from the same source, one of which has traversed to and fro a distance, D, parallel to the earth's motion, and the other a like distance in the perpendicular direction. The phase of the latter ray is considered by Michelson to be unaffected by the earth's motion. As to the former, it is retarded by the amount

$$\frac{D}{V-v} + \frac{D}{V+v} - \frac{2D}{V} = \frac{2D}{V} \cdot \frac{v^2}{V^2},$$

or, reckoned in distance at velocity V,

$$2D\,v^2/V^2. \quad \ldots\ldots\ldots\ldots (6)$$

"Considering only the velocity of the earth in its orbit, the ratio $v/V = 10^{-4}$ approximately, and $v^2/V^2 = 10^{-8}$. If D = 1200 mm., or, in wave-lengths of yellow light, 2,000,000, then in terms of the same unit, $2D\,v^2/V^2 = ·04$.

"If, therefore, an apparatus is so constructed as to permit two pencils of light, which have travelled over paths at right angles to each other to interfere, the pencil which has travelled in the direction of the earth's motion, will in reality travel ·04 of a wave-length further than it would have done were the earth at rest. The other pencil, being at right angles to the motion, would not be affected.

"If now the apparatus be revolved through 90°, so that the second pencil is brought into the direction of the earth's motion, its path will be lengthened ·04 wave-length. The total change in the position of the interference bands would be ·08 of the distance between the bands, a quantity easily measurable."

In the actual experiment, the earth's velocity was not available to the full extent, and the displacement to be expected on this account was reduced to ·048; but Michelson considers that some addition to it should be made on account of the motion of the solar system as a whole. The displacement actually found was ·022; and when the apparatus was employed in such azimuths that the rotation should have had no effect in any case, ·034. These results are very small, and Michelson gives reasons for regarding them as partially systematic errors of experiment. He concludes that there is no real displacement of the bands, and that the hypothesis of a stationary æther is thus shown to be inconsistent with fact.

It has, however, been recently pointed out by Lorentz[2] that Michelson has over-estimated the effect to be expected according to Fresnel's views. The ray which travels perpendicularly to the earth's motion is not unaffected thereby, but is retarded to the amount represented by $D\,v^2/V^2$. The outstanding relative retardation is thus only $D\,v^2/V^2$, instead of the double of that quantity. Accepting this correction, we have to expect, according to Fresnel's views, a shift of only ·024 of a band in Michelson's experiment.

Under these circumstances Michelson's results can hardly be regarded as weighing heavily in the scale. It is much to be wished that the experiment should be repeated with such improvements as experience suggests. In observations spread over a year, the effects, if any, due to the earth's motion in its orbit, and to that of the solar system through space, would be separated.

On the whole, Fresnel's hypothesis of a stationary æther appears to be at the present time the more probable; but the question must be considered to be an open one. Further evidence would be most important; but it is difficult to see from what quarter anything essentially new can be expected. It might be worth while for astronomers to inquire whether it is really true, as is generally assumed, that stellar aberration is independent of the position upon the earth's surface from which the observation is made. Another question that might, perhaps, be submitted with advantage to an experimental examination is whether the propagation of light in air is affected by the rapid motion of heavy masses parallel to, and in the immediate neighbourhood of, the ray.

If we once admit the principle that, whatever the explanation may be, no ordinary[1] terrestrial observation is affected by the earth's motion, it is easy to give an account of what must happen when the light comes from an external source which may have a motion in the line of sight. Imagine, for example, a spectroscopic examination of a soda flame situated on a star and vibrating in identical periods with those of terrestrial soda flames. In accordance with Döppler's principle, the wave-lengths are altered by a relative motion in the line of sight, and the fact may be rendered evident by a comparison between the spectra of the star and of the terrestrial flame, held so as to be seen in the same direction. The simplest case is when the flame is entirely external to the apparatus, so that both lights are treated in precisely the same way. It is evident that, under these circumstances, the difference between the two cannot fail to become apparent; and this way of regarding the matter shows also that the apparent displacement of the bright lines in the stellar spectrum is dependent upon the relative, and not further upon the absolute, motions of the star and of the earth. The mean of observations, equally distributed over the year, would thus give data for determining the relative motion in the line of sight of the star and of the solar system.

If the external source be the sun itself, it might be thought that the spectra must agree almost perfectly, the eccentricity of the earth's orbit being so very small. But the sun is a revolving body, and consequently a distinction must be made according to the part of the sun from which the light proceeds. It is found, in fact, that a very sensible shift takes place in the position of the dark lines according as the light under observation comes from the advancing or from the retreating limb. This circumstance has been successfully employed by Thollon and Cornu to distinguish between lines having a solar and a terrestrial origin. In the latter case it is a matter of indifference from which part of the sun the light proceeds.

In general optical theory the finiteness of the velocity of light is usually disregarded. Velocities at least ten times greater than that of the earth in its orbit are, however, known to astronomers; and such must begin to exercise a sensible influence upon radiation. Moreover, in so wide a generalization as the theory of exchanges, the neglect of even a small quantity is unsatisfactory. Prof. Balfour Stewart has discussed the influence of the motion of a plate exercising selective absorption upon the equilibrium of radiation within an inclosure. He argues that a disturbance will ensue, involving a violation of the second law of thermodynamics, unless compensated by some other effect not hitherto recognized. It appears, however, more probable that the whole radiation coming *from* and *through* a plate would not be altered by its motion. Whatever effect (in accordance with Döppler's law) the motion has upon the radiation from the plate, a similar effect would probably be produced upon the absorbing power. On this view the only result of the motion would be to change the wave-length of the rays most powerfully emitted and absorbed, but without disturbing the balance required by the theory of exchanges. The moving plate would in fact be equivalent to a stationary one of slightly different quality. RAYLEIGH.

1887.

[1] *American Journal*, xxii. p. 120 (1881).

[2] "Over den invloed dien de beweging der aarde of de licht verschijnselen uitoefent." (Amsterdam, 1886.)

SOCIETIES AND ACADEMIES.

LONDON.

Mathematical Society, March 10.—Prof. Greenhill, F.R.S., President, in the chair.—The President and Mr. S. Roberts, F.R.S., spoke upon the loss the Society had sustained by the recent decease of Dr. Hirst, F.R.S., touching more especially upon the great services he had rendered to it in the early days of its existence.—The following paper was read:—The simplest equivalent of a given optical path, and the observations required to determine it, by Dr. J. Larmor. To specify an optical path through a heterogeneous medium like the atmosphere, or through an arrangement of refracting substances like an optical instrument, we require the geometrical curve followed by the filament of light, and also the character of the modification produced on a filament following this path across the medium.

[1] This qualification is inserted in order to exclude such an experiment as that of Michelson, just described, in which an attempt is made to render sensible an effect depending on v^2/V^2.

[2] B.A. Report, 1871.

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This division into filaments of waves, along whose course the energy of the radiation is propagated, is the true objective analysis of the light; and it also, on Sir W. R. Hamilton's principles, leads to more compact and comprehensive treatment than the ordinary analysis into linear rays. It is shown that the effect of the medium on any filament following the given path is exactly equivalent to that of a certain pair of thin astigmatic lenses with a common axis, on a beam passing across them; and a method is given for constructing these lenses from observations made at the two extremities of the actual optical path. It is shown, in continuation, that conjugate pairs of focal lines at the two ends of a filament are given by a 2 to 2 correspondence, whose general relations are exhibited graphically by the aid of a pair of conics, and various developments are made in this direction. There are, in general, no points which have conjugate foci; but, if a certain condition hold, there exist two transverse planes of points which have conjugate planes of focal points. This occurs only when the equivalent astigmatic pair of lenses have their principal sections parallel, so that the component refractions in these principal sections are independent of each other. It is only in this special case that the emergent filament can be constructed by means of rays, in Moebius's manner, by aid of the two conjugate pairs of focal planes. If a certain other condition is also satisfied, there is complete symmetry round the axis, except as regards a possible constant rotation of the filament; and then the optical path is equivalent to refraction by a single ordinary thin lens.—The President communicated a paper, by Prof. W. Burnside, on cases in which a hyper-elliptic integral of the first order can be expressed as the sum of two elliptic integrals.—Mr. Tucker read abstracts of the following notes:—On the analytical theory of the congruency, by Prof. Cayley, F.R.S.—On certain curves of the fourth order, and the porism of the inscribed and circumscribed polygon, by Mr. R. A. Roberts.—Notes on dualistic differential transformations, by Mr. E. B. Elliott, F.R.S. A perusal some time ago of De Morgan's paper in the Cambridge Transactions on the subject of the principle of duality in differential equations which bears his name led Mr. Elliott to notice a short note thereto, in which the author announced that after writing his paper he had found a note by Chasles, in which the method had been anticipated. Upon this, referring to Chasles's work ("Aperçu Historique," note xxx), he found that Chasles had stated and to a certain extent developed a theory on the subject of much wider generality. It occurred to Mr. Elliott that some further consideration might with advantage be given to Chasles's conclusions and their extension; and a portion of this paper is the result. It had previously occurred to the author that recent theories as to the transformation of differential expressions by interchange of dependent and independent variables, and in particular the theory of reciprocants, had a bearing on the more restricted or De Morgan duality, and even more on its simpler analogue as to ordinary differential equations, which had probably escaped notice. Another portion of these notes is devoted to the elucidation of this idea.—Prof. M. Hill made a few remarks on singular solutions; and the President spoke on the rectification of the Cartesian oval. It has been shown by Prof. Genocchi, of Turin (*Annali di Matematica*, 1864), and by Mr. Samuel Roberts (Proc. L.M.S. iii.), that the arc of a Cartesian oval can be expressed as the sum of three elliptic arcs. Taking a fixed oval (i.) and its conjugate oval (ii.) in a triconfocal system of Cartesians, then as a variable orthogonal oval traces out by its intersection with (i.) a certain arc, its conjugate oval traces out on (ii.) another arc; the sum of these arcs can be expressed by a single elliptic arc, while the difference is expressible as the sum of two elliptic arcs; thus leading to the theorems of Prof. Genocchi and Mr. S. Roberts.

Anthropological Institute, March 8.—Edward B. Tylor, F.R.S., President, in the chair.—Mr. J. Allen Brown read a paper on the continuity of the Palæolithic and Neolithic periods. The deductions of the author are based on the large number of flint implements of Palæolithic type which have been discovered during recent years at Eastbourne, East Dean, Cuckmere, and in other combes and dry valleys in England; at East Dean, &c., they are associated with compact aggregated deposits of flints and chalk rubble, evidently due to the erosion of the valleys and combes by underground water, as seen at Birling Gap, near Eastbourne. The valleys of Sussex have been subject to many changes during the concluding episodes (both glacial and subaërial) of the Quaternary period, and in many cases the older forms of flint implements have been covered up and preserved by the deposit of loam and chalk rubble resulting from the waste of the surface of the land. Intermixed or associated with the flint implements of older types are others of transition form, to which he desired to see the term "Mesolithic" applied. The East Dean Valley appeared to contain flint implements forming a series ranging from the late Palæolithic age to the polished stone period of true Neolithic. The old mining-shaft at Cissbury has furnished analogous specimens. Similar implements of the Palæolithic type have been found in chalk rubble far away from the sea-board, and associated with the bones of the mammoth, tichorine rhinoceros, hippopotamus, and other Quaternary Mammalia, as well as the remains of various animals of species still living, showing that man was present in Southern Britain not only in the plateaux and river-drift periods, but also continuously into the so-called Neolithic epoch. The author alluded to the evidence derived from caves and rock-shelters and peat-beds, both in this country and in France, which pointed in the same direction. A large series of flint implements of Palæolithic form from East Dean, &c., were exhibited, with specimens of corresponding forms from the river-drift; also a series showing the evolution of the axe or celt form from the simply chipped nodule of the plateaux drift, through the valley drift and transitional types to the highly finished celts of the Neolithic age, of which the forms were continued in the earliest stages of the age of copper and bronze. Other series were exhibited, showing in like manner the evolution of the spear-head and knife, &c.

Zoological Society, March 1.—Dr. A. Günther, F.R.S., Vice-President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of February 1892, and called attention to two Short-winged Tyrants (*Machetornis rixosa*) purchased February 15, being the first examples of this bird that have reached the Society, and to a female Beatrix Antelope (*Oryx beatrix*) from Arabia, presented by Lieut.-Colonel Talbot, February 18.—Mr. J. Graham Kerr gave a short account of the expedition up the Rio Pilcomayo in 1890–91, which he had accompanied as naturalist. Mr. Kerr made remarks on the animals met with on the banks of the Pilcomayo, and exhibited a series of photographs illustrating the vegetation of the district and its native Indian inhabitants.—Mr. G. F. Hampson read a paper on stridulation in certain Lepidoptera, and on the distortion of the hind-wings in the males of certain *Ommatophorinæ*. The author attributed the clicking sound described by Darwin as produced by various species of the South American genus of Butterflies, *Angerona*, and confirmed by Wallace and other observers, to the presence of a pair of strong corneous hooks on the thorax, which play on a pair of curved hooks with spatulate ends attached to the inner margin of the fore-wing close to the base, and surrounded by a membranous sac which acts as a sounding-board. An account was given of a similar sound produced by the males of a Burmese moth of the family Agaristidæ and of a buzzing sound in an allied Australian form, both of which have a patch of ribbed hyaline membrane below the costa of the fore-wing. The sound was attributed to the friction of spines, attached in the former to the first pair of legs, in the latter to the second pair, on the ribbed membrane. A description was then given of the transformation of the costal half of the hind-wing in the Noctuid genus *Patula* into a large scent-gland, and of the manner in which this had distorted the neuration. The still greater distortion of the neuration in the allied genus *Argida* was attributed to its once having possessed a similar scent-gland, now become rudimentary by disuse.—A communication was read from Prof. W. N. Parker, on the retention of functional gills in young Frogs (*Rana temporaria*), which he had succeeded in producing in specimens reared in his laboratory. Prof. Parker described the method employed with this object, and made remarks on the way in which the fore-limbs are protruded.—Prof. F. Jeffrey Bell read a paper entitled "A Contribution to the Classification of Ophiuroids," to which were added descriptions of some new and little-known forms of this group.—Mr. M. F. Woodward gave an account of an abnormal Earthworm (*Lumbricus terrestris*) possessing seven pairs of ovaries situated on the eighth and following somites to the fourteenth.

OXFORD.

University Junior Scientific Club, March 4.—Mr. J. A. Gardner, of Magdalen College, President, in the chair.—Some investigations of the action of dry hydrochloric acid gas on dry carbonates were brought forward by Mr. F. R. L. Wilson,

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Keble College, and Mr. R. E. Hughes, Jesus College. The authors showed that probably perfectly dry hydrochloric acid gas does not act on carbonates. Experiments were tried with the carbonates of calcium and barium.—Mr. J. L. Hatton, Hertford College, read a paper on some investigations, which he had been engaged upon, in conjunction with Mr. James Walker, on the motions of the nodal planes in a rotating bell. This work appeared in a recent number of the *Philosophical Magazine*.—This paper was followed by an account of the fixation of nitrogen by plants, by Mr. O. V. Darbishire, of Balliol College.

PARIS.

Academy of Sciences, March 14.—M. d'Abbadie in the chair.—The Secretary commented upon the loss sustained by the Academy by the death of M. Léon Lalanne.—On conical vascular branches, and on the inductions to which they lead with regard to the organization of the vascular blood system, by M. Ranvier.—Researches on samarium, by M. Lecoq de Boisbaudran. By passing an electric spark from a large induction coil, without condensers, through solutions rich in samarium, and viewing the spark spectroscopically, lines were obtained at the wave-lengths 466·2, 462·7, and 459·3, and a wide band having a well-defined edge at λ 611·2, and fading away to about λ 622. The samarium bands undergo very marked variations when the position of the spark with respect to the meniscus of liquid is altered. This fact is thought to be of interest from the point of view of the supposed complexity of samarium. It is not impossible that there is a relation between the band 611–622 and the narrow line which Prof. Crookes observed when using mixtures of samarium and yttrium *in vacuo*, and which he attributed to the presence of a new element. M. Boisbaudran has observed this line, or one very near it, with different substances, and finds that its position varies sensibly with the nature of the solution employed. The narrow line is accompanied with a less refrangible and weaker one. With lanthanum sulphate mixed with a compound of samarium, the wave-length of the stronger line was determined as 612·7, and of the weaker 619·6. Prof. Crookes obtained the wave-length 609.—On a remarkable prominence, by M. H. Deslandres. The prominence was observed on the east limb of the sun on March 3, as the large spot-group of February was coming round it.—On frictionless gearings, by M. A. Rateau.—On periodic heat maxima observed in spectra furnished by flint and crown glass, and rock-salt, by M. Aymonnet. The heat maxima observed are separated by equal wave-lengths in the case of each of the prisms used, and, for rock-salt, the maxima appear to correspond to the fundamental vibrations of 1, 2, 3 . . . *n* systems of cubical molecules.—On some well-defined alloys of sodium, by M. Joannis. By the action of lead, in excess, upon sodammonium, a compound having the formula $Pb_4Na_2NH_3$ was obtained. An alloy of lead and potassium, Pb_2K, was obtained by the action of potassammonium, in excess, upon lead; an alloy of bismuth and sodium, $BiNa_3$, by treating pure bismuth, in excess, with sodammonium, and an alloy of antimony and sodium, $SbNa_3$, have similarly been produced.—On the analysis of minerals containing antimony, by M. Ad. Carnot.—On the microscopic structure of oolitic iron from Lorraine, by M. Bleicher. From the investigation it appears that the ferruginous oolites which have been studied consist of a central mineral or organic nucleus, single or multiple, surrounded by regular concentric layers of a substance rich in silica and organic matter.—On the vegetation of the vine, by MM. L. Roos and E. Thomas. Conclusions are given respecting the amounts of sugars present and the acidity of various parts of the vine plant at different stages of its growth.—Citric acid, by M. G. Massol. The heat of formation, in the solid state, of potassium and sodium citrates is greater than that of the corresponding carballylates. The augmentation is analogous to that observed when comparing malonic and succinic acids with tartronic, tartaric, and malic acids, and is to be attributed to the alcoholic hydroxyl group.—On some reactions of the isomeric amido-benzoic acids, by M. Oechsner de Coninck.—Calculation of the temperatures of ebullition of compounds derived from the paraffins by terminal substitution, by M. G. Hinrichs.—On the pyrogenous hydrocarbons formed in the compressed gas industry, by M. A. Brochet. The author has isolated and identified the following:—

$CH_2 : CH . CH_2 . CH_3$;
$CH_2 : CH . CH_2 . CH_2 . CH_3$;
$CH_3 . CH : CH . CH_2 . CH_3$;
$CH_2 : CH . CH_2 . CH_2 . CH_2 . CH_3$;
$CH_2 : CH . CH : CH_2$;
$CH_2 : CH . CH_2 . CH : CH_2$.

—The specific gravity of silk, by M. Léo Vignon.—Glycolysis in the blood, by M. Maurice Arthus.—Are there inhibitory nerves?, by M. J. P. Morat.—On an anomaly in the great hypoglossal nerve, by M. Buffet-Delmas.—On the ovary and the egg of *Gobius minutus*, by M. Frédéric Guitel.—Note on the magnetic perturbations of March 11–13, 1892, by M. Th. Moureaux.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—Note-book of Agricultural Facts and Figures, 4th edition: P. McConnell (Lockwood).—A Year-book of Science, 1891: edited by Prof Bonney (Cassell).—Willing's British and Irish Press Guides, 1892 (Willing).—Sitzungsberichte der K.B. Gesellschaft der Wissenschaften, Math. Naturw. Classe, 1891 (Prag).—Abhandlungen der Mathematisch-Naturwissenschaftlichen Classe der K.B. Gesellschaft der Wissenschaften von den Jahren 1890-91. vii. Folge, 4 Band (Prag).—Health Springs of Germany and Austria, 2nd edition: F. O. Buckland (Allen).—The School Calendar, 1892 (Whittaker).—Silk Dyeing, Printing, and Finishing: G. H. Hurst (Bell).—Le Poil des Animaux et les Fourrures: Lacroix Danliard (Paris, Baillière).—Les Fleurs à Paris: P. L. de Vilmorin (Paris, Baillière).—Statistics of the Colony of Tasmania for the Year 1890 (Tasmania, Strutt).—Anatomie et Physiologie Comparées de la Pholade Dactyle: Dr. R. Dubois (Paris, Masson).

PAMPHLETS.—Neue Integrationsmethoden auf Grund der Potenzial—Logarithmal und Numeral-rechnung: Dr. J. Bergbohm (Stuttgart).—Neue Rechnungsmethoden der Höheren Mathematik, Dr. J. Bergbohm (Stuttgart).—O Theorii Ploch: E. Weyr (V. Praze).—Jahresbericht der K.B. Gesellschaft der Wissenschaften für das Jahr 1891 (Prag).

SERIALS.—Beiträge zur Kenntniss der Orchideen von H. G. Reichenbach fil fortgesetzt durch F. Kränzlin; Dritter Band, Fünftes Heft (Leipzig, Brockhaus).—Proceedings of the American Philosophical Society, vol. xxx. No. 136 (Philadelphia).—Bulletin de L'Académie Imperiale des Sciences de St. Pétersbourg, Nouvelle Série, II. xxxiv. (St. Pétersbourg).—Bulletin of the New York Mathematical Society, vol. i. No. 6 (New York).

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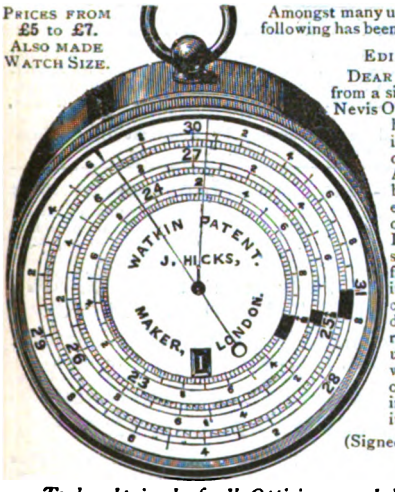

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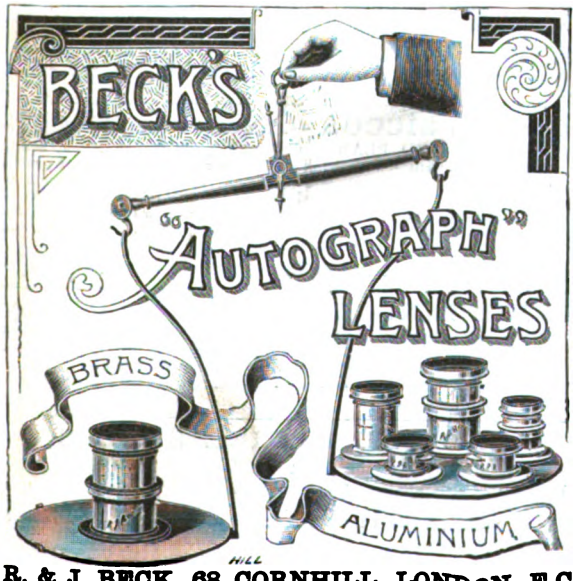

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THURSDAY, APRIL 28, 1892.

THEORETICAL CHEMISTRY.

Outlines of Theoretical Chemistry. By Lothar Meyer, Professor of Chemistry in the University of Tübingen. Translated by P. Phillips Bedson, D.Sc., and W. Carleton Williams, B.Sc. Pp. 220. (London: Longmans, Green, and Co., 1892.)

"GOOD wine needs no bush," but a well-known bush makes one look for good wine. The translation of Prof. Lothar Meyer's "Die Modernen Theorien der Chemie," made by Messrs. Bedson and Williams, is so well known and so appreciated by all English-speaking chemists, that everyone welcomes a new book by the author of "Modern Theories," and expects the book to be a good one. The "Outlines of Theoretical Chemistry" is a translation, by the translators of the "Modern Theories," of a book published in German in the course of last year. The translation is exceedingly well done; the English runs smoothly and lucidly; the book reads as if it were composed in English, rather than as a translation from another tongue.

The subject-matter of this book is very similar to that of "Modern Theories"; details are avoided wherever the author thought this could be done with advantage, and the treatment is made as general as possible. In his preface to the English translation the author says:—

"The general—I may say the philosophical—review of the subject has been my chief aim, to which the details should be subordinated."

The book is not divided into chapters, but runs on from paragraph to paragraph. Beginning with a statement of the province of chemistry, the author passes in review the stoïchiometric laws, sketches the atomic hypothesis, considers the various aspects of chemical equivalents, states and applies the law of Avogadro, refers to Prout's notions about the relations between the values of atomic weights, and states and briefly illustrates the periodic law; he then considers in several paragraphs the constitution of compounds in the light of the molecular and atomic theory, and, through a short discussion of physical isomerism, he passes to the consideration of such physical properties of bodies as melting and boiling point, capillarity, solubility, evaporation, &c., and the connexions between these and the molecular weights and constitutions of bodies. Finally the author devotes some fifty or sixty paragraphs to the treatment of the thermal and electrical aspects of chemical changes, and the subject of chemical affinity.

At the outset the essential character of chemical phenomena is emphasized:—

"Chemistry deals with the changes which affect the material nature of the substance. Chemistry, then, is the science which treats of matter and its changes" (p. 2).

It is to be wished that all writers of books, whether elementary or advanced books, on chemistry, and all who endeavour to help others to learn this science, would keep steadily before them the characteristic feature of all chemical events, viz. that they are those which occur when changes of composition accompany changes of properties in definite kinds of matter. If this were done we should not be deluged with those catalogues of the properties of innumerable disconnected substances which are frequently sold under the misleading name of text-books of chemistry.

The paragraphs on the determination of atomic weights from stoïchiometric values (pp. 11–13) seem to me extremely lucid and apposite, provided the reader will give his close attention to them. I do not think the subject of chemical equivalents is treated sufficiently fully to make it clear (pp. 13–16). Paragraph 13 does not make perfectly intelligible the process whereby atomic weights are determined from the crystallographic relations of compounds. I am much taken by the order in which the author arranges his treatment of combining weights, equivalents, thermic equivalents, crystallographic equivalents, &c., culminating in Avogadro's law. The determination of atomic weights by the application of the law of Avogadro is made very clear in a couple of paragraphs (pp. 39–42); and the author is especially to be congratulated, in my opinion, on paragraph 26, wherein he most skilfully and gracefully avoids the popular error of making a stumbling-block of so-called "abnormal vapour-densities."

Paragraph 28, which deals in about thirty lines with "nascent state," would much better have been omitted; the treatment is neither interesting nor accurate. It seems to me that paragraphs 34–40, which are supposed to give a clear general conception of the periodic law, quite fail to enable the student to grasp this all-important generalization. I think that much too little space is given to the periodic law, which comprises in itself all other schemes of chemical classification; and that too much space is devoted to valency, which, at the best, is a conception that is of very limited application. Anyone who turns from the study of Mendeleeff's great work on "The Principles of Chemistry" to the paragraph on p. 76 will be greatly astonished; the paragraph reads thus:—

"Formerly it was more or less explicitly assumed that a chemical compound was held together by the total attractive force of the affinities of all the atoms contained in it; but, as our knowledge increased, it was gradually recognized that the connexion is between atom and atom, and that the atoms are attached to each other like the links in a chain, the continuity ceasing if even a single link of the chain is removed."

This sentence seems to imply that no one now looks on a molecule as held together by the interactions between all the atoms; but if one says this view is held by none, one must make a few exceptions, such as Mendeleeff and the chemists of his school. The treatment of atomic linkage on pp. 80–83 seems to me to be very one-sided and unsatisfactory. We are told (p. 81) that such a formula as $H_2O . SO_3$ is inadmissible because it represents the compound as made up of atomic groups which are already saturated, and "therefore have no free affinities for mutual combination"; but on p. 107 we are informed that, in substances which crystallize with water, "every molecule is united with a definite number of molecules of water." But how can water molecules unite with, say, dehydrated alum, if the group H_2O is saturated and "has

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therefore no free affinities"? This example shows that the paragraph quoted above from p. 76 is much too dogmatic.

I do most strongly object to such a statement as that on p. 69, where, speaking of carbon monoxide, it is said:—

"The molecule of this compound is represented by the formula

$$*{=}C \,||\, O.$$

Here the asterisks are intended to show that two affinities are unsaturated; this is proved by the fact that the compound unites with two atoms of chlorine, forming phosgene gas,

$$\begin{matrix} Cl \\ Cl \end{matrix}\!\!\rangle C{=}O."$$

What is proved by the fact of combination with chlorine? No one can attach any clear meaning to the statement "two affinities are unsaturated." The only practical meaning these words have is, "The molecule CO can unite with two other atoms of certain kinds"; that is to say, the sentence quoted, when put into the speech of the plain man, asserts that the fact that CO does unite with 2Cl proves that CO can unite with 2Cl.

The later paragraphs, treating of the physical properties of bodies and the connexions between these and the constitutions of the same bodies, seem to me to be both very well done and very disappointing. They are well done because an earnest attempt is made to put the matter clearly, but they are disappointing because it is quite impossible to grapple with these very difficult matters in the space which is given to them in this book. I do not think that anyone will succeed in getting a grasp of Raoult's law from the pages which are grouped around paragraph 133. The application of Raoult's law to determine molecular weights, given on p. 137, is based on the constant ·62°, which has been shown by van 't Hoff and others to be erroneous.

But it is much easier to find fault than to compose such a book as this. A careful perusal of the work leaves the impression on my mind that, as a synopsis and suggestive remembrancer to the student who knows general chemistry well, this book will prove useful, but that it is too condensed and too slight to be of much service to him who is beginning the study of general chemistry. Most of the subjects dealt with cannot be made clear except by going into details, and illustrating them with considerable profusion. When one attempts to deal with these matters in a broad and general way, and at the same time to devote only a few pages to each section, one is almost obliged either to make statements so generalized that they are of very little use to the earnest student, or only to touch the fringe of each part of the subject. Chemistry is an abstract science to a much less degree than physics; hence such short statements as those which sum up and include in themselves whole provinces of physical knowledge cannot yet be made in chemistry. Where the "Outlines of Theoretical Chemistry" fails for the most part it fails because no book could succeed; it fails because it attempts to do that which cannot, at present, be done.

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THE TRAVELS OF A PAINTER OF FLOWERS.

Recollections of a Happy Life, being the Autobiography of Marianne North. Edited by her sister, Mrs. John Addington Symonds. In Two Volumes. (London: Macmillan and Co., 1892.)

MOST of the readers of NATURE will know without telling that Marianne North was a world-wide traveller, that she travelled in pursuit of nature, that she was an accomplished and faithful painter of plant and animal life, and that the results of a life's labour were presented by her to the nation, and now cover the walls of a building in Kew Gardens, erected at her expense Most persons, too, who knew her personally—and her acquaintances and friends are as numerous as her travels were wide—will be glad to know something more of her history, and especially something more of her travels, of her impressions of peoples, of places, and, above all, her impressions of the plant and animal life of the many countries she visited and to which she gave her life. All who had the pleasure of knowing her personally will remember her stately presence, her kind face, her charming manner, and her entertaining conversational powers—now relating the difficulties and delights of her experiences in foreign lands, now her appreciation of home comforts and genial society. She wrote as she talked, and she was a fertile letter-writer; and she has written her book in the same style.

In early life Miss North made various journeys in Europe, and also went up the Nile and visited Syria, and painted many flowers; but with the exception of the Sicilian *Papyrus*, and perhaps two or three other little pieces, none of this early work is in the gallery at Kew. Only 38 pages of her book are devoted to her early life, and it practically begins with her more distant travels; the first long trip being to Canada and the United States, and extended to Jamaica, whence she returned to England. Two months later she started for Brazil, where she made a long stay, and then returned direct to England. The next journey included Teneriffe, California, Japan, Singapore, Borneo, and Java, and then home again. Her paintings attracted attention, and she complied with a request to exhibit some 500 of them at Kensington. This matter being arranged, she proceeded to India, landing on the way at Lisbon, Gibraltar, Malta, and Galle; and India was traversed almost from east to west and north to south.

The narrative of this journey is perhaps the most interesting part of the whole work. On her return home there was an exhibition of the accumulated paintings in Conduit Street; and a visit to Mr. Darwin, which ended in a determination to go to Australia and paint the flowers of the fifth quarter of the globe. It should be mentioned that in the meantime Miss North had adopted a suggestion of the *Pall Mall Gazette* that her paintings should find their home at Kew, and her generous offer was accepted. So it was, that when Darwin told her that her collection of paintings would be an imperfect representation of the vegetation of the world without the Australian element, she took it as a "royal command," and prepared to go forthwith. This journey some of the old scenes were revisited, brief halts being made at Galle and Singapore, a longer stay with the Rajah and Rani

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Brooke in Borneo, and thence to Queensland. New South Wales, Victoria, West Australia, Tasmania, and New Zealand were successively visited; but incessant travelling, climatal changes, and continuous work had begun to tell on the constitution of this brave woman, who suffered much in the colder regions. Now, the great object was to make the collection of paintings as complete as possible, and she spared neither her pocket nor her person in trying to carry it out. Her book is so essentially the history of her gallery at Kew that one cannot dissociate them. The Australian journey was fruitful beyond all others, and the Australasian section of the gallery is perhaps the most attractive of all, being a marvellously complete representation of the varied and curious flora of that region. The homeward route was across the Pacific, calling at Honolulu, landing at San Francisco, and off at once to the redwood and mammoth-tree forests for more painting. Then across America by the southern route, and back to old haunts in the North-Eastern States, and home again to open the gallery, which had been built during this journey. Hanging the pictures was a most laborious task, from which Miss North took no rest. At this time the writer first made her acquaintance, and was engaged by her to botanize the paintings and compile a popular instructive catalogue. This occupied two or three months; and most interesting work it was, usually brightened by her presence.

No sooner was the opening of the gallery accomplished, than the terribly jaded donor of this munificent gift to the public began to think of visiting new regions to further enrich it. But I must be brief, for even to catalogue these journeys occupies much space. South Africa was next visited, and several months' uninterrupted work, much of it done under trying conditions of failing health, yielded so bountifully that it was determined to build a wing to the gallery, for the existing walls were already completely covered.

Miss North intended going from South Africa to Madagascar, but the means of communication were irregular and uncertain, and her health so bad that she returned home; but having to some extent recovered, she went the following year (1883) to the Seychelles, to paint the beautiful palms and screw pines of those islands. Even this did not satisfy her, and she started on her last journey in November 1884. Chili was her goal, and the principal object of this long journey was to paint the *Araucaria imbricata* in its home, as she had already painted the Brazilian and Australian species. She also succeeded in painting a considerable number of the characteristic types of the vegetation of that country. But this voyage, by way of the Straits of Magellan, tried her waning strength very much, and a less energetic person would have collapsed entirely. In the last chapter of her "Recollections" we read that all was enjoyment until they reached Bordeaux. "Then my nerves gave way again (if they were nerves), and the torture has continued more or less ever since." Beautiful Rio was touched on the outward voyage, and on the homeward route, by Panama, old friends were looked up in Jamaica. England was reached in the spring, and it cost another year to rearrange the gallery; the introduction of the South African, Seychelles, and Chilian paintings entailing renumbering throughout, in order to preserve the geographical order.

The foregoing is an outline of her journeyings, but the book should be got for the details, which are almost always interesting, often clever and quaint. Here and there one meets with uncompromising criticisms and descriptions of persons that might have been expunged with advantage. The descriptions of the vegetation of various regions, with particulars of the principal elements, are pleasant and instructive, often containing much original information; and will be greatly appreciated by those who frequent the gallery at Kew, of which the book, as already stated, contains the history.

After completing her work at Kew, Miss North took an old-fashioned house at Alderley, in Gloucestershire, where she formed a charming garden; but her constitution was broken, her sufferings increased, and she died in August 1890. W. B. H.

AMERICAN TOWN TREES.

Our Trees. By John Robinson. (Salem: Horton and Son, 1891.)

THIS short account of the trees of an American town and its neighbourhood consists of reprints of newspaper articles written in 1890–91 for the benefit of local readers: they have been re-compiled into book form at the request of the directors of the Essex Institute, and date from the Peabody Academy of Science, Salem.

Several points strike a careful reader of the book. The writer draws special attention to the fact that the articles, or chapters, are not intended as botanical essays; and the reader will probably decide that the remark was unnecessary, for a more unscientific work dealing with a scientific subject would be difficult to find; but there is a peculiar charm in a certain style of talks about natural objects—for instance, in some of the more chatty paragraphs of White's "Selborne," or Walton's "Angler," and even Evelyn's "Flora"—which attracts the most devoted student to refreshing looks around his subject-matter from every-day points of view, and this little work possesses that charm. Few facts of scientific importance are met with in such writings, and still fewer of the generalizations which make science what it is: the specialist may even deride the writing as "talkee-talkee"—gossip, if you will; and even the broadest thinker may be inclined to wonder why such articles are written; all this, and more, may be true, and yet—there is the charm, nevertheless, and it is very apt to seem appropriate where trees and flowers are concerned. Whether it is advisable that such writings should increase is a matter likely to settle itself, simply and certainly, because very few can produce them. A scientific work, then, this is decidedly not. It is a series of homely chats about trees, by one who knows and loves them. The latter fact leads to another—namely, that such a writer cannot help telling you something worth learning even though it be by the way, and merely incidental.

In the first place we gather some ideas as to what trees are common in the streets and gardens of a Massachusetts town, and the evidently thriving condition of magnolias, sumachs, maples, witchhazels, mulberries, hickories, gingkos, catalpas, sassafras, and many other beautiful trees, makes envious one who knows what difficulties are

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met with in this country in attempts to rear even presentable "specimens" of such favourites in our smoke-beladen and crowded cities and suburbs.

Then, again, the English reader gathers some information as to the Western popular names of trees, well known to him by very different ones; how many English people know what are the "cucumber-tree," the "yulan," the "buckeye," the "butter-nut," and the "button wood"?

Bits of history also occur, and incidental notes on the rates of growth of various trees, their ages, &c. So that, after all, there are some dry facts in this singularly quaint and simply written talk about trees. We must not claim much for the work in this respect, however; and perhaps the chief reason we like the writing is because of its contrast to the empty and inflated style of too many of our native newspaper articles on similar subjects.

OUR BOOK SHELF.

Synopsis of Non-Metallic Chemistry. By William Briggs, B.A., F.C.S. Pp. 90. (London: W. B. Clive and Co.)

THIS book is intended for students preparing for the Matriculation Examination of London University. After the contents of an ordinary text-book have been studied, the reader is here supposed to find the more important points which have to be remembered, and which serve to recall the less important. Interleaved note-paper is provided, whereon facts readily forgotten may be recorded.

As is usually the case with such cram-books, little can be said in favour of the quality of the information supplied.

Formulæ are stated to be "arrangements of letters representing *a molecule* of a compound," and this definition is illustrated by regarding Fe_3O_4 as denoting a molecule. The vapour-density of hydrofluoric acid is given as 10, and the solubility of hydrogen in water as "practically the same at all temperatures from 1° to 20°": neither statement is up to date. Such antiquated terms as *basylous* and *chlorous*, which are freely employed, might well be replaced; and to speak of *distilling* potassium perchlorate with strong sulphuric acid is inaccurate. The account given of fractional distillation is worthy of reproduction. The mixture of liquids is heated "up to the lowest of the boiling-points of the liquids present. *The whole* of that liquid (?) will be converted into vapour, and can be condensed in the usual way. On heating the remaining liquid up to the next boiling-point, we can separate *another* of the constituents, and so on until they are all separated out. The different liquids thus obtained must be redistilled to get them quite free from the others, small quantities of which *may* have been distilled over in the first process."

This last extract is typical of the bulk of the knowledge contained in the book, which, to say the least, savours more of the class-room than the laboratory.

A table of contents, a glossary, and three appendixes are provided. The last are concerned with the preparation and purification of substances and with the simpler chemical calculations. A list, with answers, of numerical examples set at the matriculation examinations is included.

Chemical Calculations. By R. Lloyd Whiteley, F.I.C. With a Preface by Prof. F. Clowes, D.Sc., F.I.C. (London: Longmans, Green, and Co., 1892.)

THIS is still another addition to the numerous manuals on chemical arithmetic, and the points wherein it differs from its predecessors are somewhat difficult to discover. Once more we have specific gravity observations in which corrections for temperature and air displacement are ignored; and although the author attempts to set right the prevalent misconception as to the meaning of density and specific gravity, it is questionable if he succeeds. According to him, specific gravity is always relative; it is the same magnitude as relative density. The use of absolute specific gravity—or, shortly, specific gravity with no temperatures of comparison attached—as denoting the weight of unit volume, is here overlooked. From a physical point of view, the definition of the absolute density of a gas as the mass of 11·16 litres is a needless complication. In ascertaining the percentage composition of a compound it is insisted that, first of all, the *molecular* weight must be calculated. The examples given to illustrate the rule include apatite, apophyllite, basic lead chromate, &c. The student is thus led to infer, here as elsewhere in the book, that the molecular weights of such bodies can be fixed.

The freezing-point and boiling-point methods of obtaining molecular weights are disposed of in two pages. No hint is given that the solutions must be dilute and non-electrolytic, if consistent results are to be obtained; or that, in general, the interpretation of the results of these methods is still subject to difference of opinion.

It is erroneous to state that "the *alteration* in the volume of a gas is proportional to the so-called absolute temperature," or to speak of "Dalton and Henry's law." Henry's law is distinct from Dalton's, and is the older by two years.

We have dwelt on some of the points which seem to call for criticism. On the other hand, the book has its good features. The problems are numerous, carefully selected, and well arranged. Contents, answers, and index are supplied. It seems to us, however, that, instead of being as good as several of its kind already in existence, it, as a new book, should have been better.

The Year-book of Science. Edited for 1891 by Prof. T. G. Bonney, D.Sc., LL.D., F.R.S. (London: Cassell and Co., 1892.)

ALL who have any sympathy with scientific pursuits will heartily welcome the appearance of this epitome of the more important results of the investigations which were published during the past year. Scientific inquiry now covers so much ground that all men of science must be more or less specialists, and it is difficult for them to keep in touch with the developments in other branches through the usual channels, although it frequently happens that an advance in one subject may throw light upon and induce investigations in another. There are also many engaged in practical pursuits who require a convenient means of determining how far contemporary researches may be technically applied.

With a well selected staff of contributors, the editor has attempted to meet the wants of all by the present volume, which is divested as far as possible of technicalities. The scope of the work is sufficiently defined by the following paragraph from the editorial:—

"It is almost needless to remark that this volume is not intended to be a record or catalogue of papers. The endeavour of its projectors and compilers has been to select those memoirs, in each several department, which appeared to be of somewhat exceptional interest, either by throwing light on special difficulties or by being suggestive of further advances."

In a work of this kind strict impartiality is essential, and we see no reason to suppose that the various contributors have abused the power vested in them. On the whole, the production is very satisfactory, and the improvements which the editor contemplates for the next volume will make it more so. One can only wonder that science has had to wait so long for a year-book of its own.

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Handy Atlas of Modern Geography. (London: Edward Stanford, 1892.)

IT would be difficult to obtain a small atlas more complete than this. Every place of any importance appears to be represented on one or more of the thirty coloured maps. The degrees of latitude and longitude are subdivided into parts of five minutes each, so that the positions of places, the names of which are not engraved, can be easily and accurately located by reference to the alphabetical list at the end. This list is a comprehensive one. It gives the latitude and longitude of the principal mountains, rivers, capes, bays, islands, towns, and villages, and forms an excellent supplement to a very good atlas.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Aurora.

A VERY brilliant display of aurora was seen here last night, the 25th inst. At about 9.25 p.m. a number of red streamers proceeded from a length of some 110° in azimuth along the northern horizon, and extended upwards for (on an average) 30°. The length of the streamers varied quickly, sometimes shooting upwards for 70° from the horizon. In the course of five minutes the red streamers gave way to white or yellowish white ones, narrower and more sharply defined than the red ones. At 9.40 p.m. there was a decrease in the brilliancy of the phenomenon, but at 9.45 p.m. long red streamers again appeared for a few minutes, which again shortly gave way to a brightness of the horizon only. Close to the horizon the colour was white, or nearly so, the whole time. The apparent point of convergence of the streamers was far south of the zenith, say 30°.

GEO. M. SEABROKE.

Temple Observatory, Rugby, April 26.

PROBABLY many of your readers witnessed the brilliant display of the northern lights between nine and twelve o'clock last night, the 25th, but it may be as well to call attention to it, as being the finest display seen here for many years. Appearing soon after nine o'clock, the luminous arc and the radiating beams, sometimes rose and orange coloured, presented a varied and beautiful spectacle until close upon midnight, when they faded away.

The most noteworthy features of this display were the vividness and height of the arc, which reached an angle of about 13° above the horizon, whilst the beams were visible up to 51°. The whole expanse of the arc from east to west was about 93°, and the duration of the phenomenon a little under three hours.

ARTHUR MARSHALL.

Cauldon Place, Long Row, Nottingham.

A FAIRLY distinct aurora was visible here on the northern horizon last night. I first observed it at 9.15, when the streamers appeared somewhat less bright than the Milky Way. Ten minutes later one streamer, about 15° west of north, brightened considerably, and appeared of a pale reddish-yellow tint. It fluctuated in intensity, and soon became less bright. The streamers, which inclined slightly to the west of the vertical, extended to about 30° to 40° above the horizon.

I watched them till 9.50, when they seemed fading in intensity, and when I looked again at 10.30 they had disappeared entirely.

ARTHUR E. BROWN.

Thought Cot, Brentwood, April 26.

Pigments of Lepidoptera.

THE appearance of Mr. F. Gowland Hopkins's letter on this subject in the last issue of NATURE (p. 581) demands a brief explanation from me—although it is not easy to reply satisfactorily within narrow limits—and the more so since Mr. Hopkins appears to have somewhat misunderstood my standpoint.

Let me first acknowledge the courteous tone of Mr. Hopkins's letter, and express my sense of the value to myself of criticism from his pen, and the more so since I have been labouring under the disadvantage of being practically entirely uncriticized so far—a disadvantage that I have not failed to appreciate.

Now, Mr. Hopkins remarks: "Mr. Coste's experiments are very useful as forming a method of classifying these pigments; but . . . they are of far too empirical a nature for any considerations as to the constitution of the bodies to be based upon them."

Here it is that Mr. Hopkins appears to have missed the point of my work. If he will do me the favour to refer to the detailed account of my experiments in the *Entomologist*, *passim*, I think that he will find it tolerably clearly emphasized that my interest in this work, so far, has been almost entirely *biological*. I stated expressly in my opening article that my object had been to discover, if possible, the genealogies of the colours, and to obtain evidence (so far as coloric characters could afford it) of the phylogenetic relations of allied species: and I may perhaps add that the results obtained have enabled me to predict several varieties of whose occurrence in the natural state I have since been informed. So that Mr. Hopkins is mildly reproaching me because my work does not tend in a direction at which it was not originally aimed, while he is at the same time good enough to admit that it is of some use for the end at which it was aimed.

However, it was only to be anticipated that one could not go very far without becoming involved in the further question as to the constitution of the pigments; but here I was met by three considerations. In the first place, I was anxious to obtain first of all as much as possible of what Mr. Hopkins designates "empirical" evidence as to the reactions and classification of the pigments before making any researches at all into their constitution; secondly, the amount of material at my command was far too scanty for any even approximate analysis; and in the third place, shortly after my experiments had been commenced, my attention was drawn to an abstract of a paper by Mr. Hopkins on the constitution of the yellow pigments. Finding that he was already in possession of the field here, I felt almost bound to leave this part of the subject alone, at least for the present; and I think that I may say that I have on the whole taken exaggerated care not to extend my experiments into that quarter where Mr. Hopkins was engaged, or to avail myself of the discoveries that he had already made, in order to trespass on his investigations. Putting aside my provisional suggestions as to the nature of the "reversion effect," it has only been at a comparatively recent stage of my work, and in consequence of experiments that have not yet been published, that I have at all turned my attention to the constitution of the pigments; these results being such as would have compelled me to consider the question even had I heard nothing of Mr. Hopkins's work. I hope that this explanation will put me right in Mr. Hopkins's eyes, and will satisfy him that he has considerably misunderstood the spirit of "some remarks [perhaps clumsily expressed by me] made at the close of the last article"; and that it will also satisfy him as to the question of priority. I had no thought of questioning Mr. Hopkins's priority in his own work, and the less so since I have throughout been under the impression that we were working mainly on different—though sometimes adjacent—lines.

I must not so far trespass upon your space as to criticize Mr. Hopkins's criticisms upon the "reversion effect"; but I will ask him kindly to examine the detailed accounts of the "reversion" experiments which I gave in the *Entomologist*, since his remarks appear to me somewhat to ignore the evidence there brought forward: and at the same time I may remark that his statements as to the constitution of the *yellow* pigments appear to me hardly to invalidate, but rather indirectly to confirm, the suggestions made by me as to the reversion reaction with *red* pigments. The new information that Mr. Hopkins promises in his closing paragraph I shall look forward to with great interest.

April 22. F. H. PERRY COSTE.

I WAS about to pen some remarks on Mr. Perry Coste's recent articles on this subject, when a letter from Mr. Gowland Hopkins in the last number of NATURE (p. 581) expressed substantially the same views as those which I had arrived at. I write now rather to support Mr. Hopkins in his strictures than to offer any fresh criticisms of my own. The articles on "Insect Colours" published in these columns are, as the author

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states, to be regarded in the light of an abstract of a series of more extended papers published in the *Entomologist*. The papers in the latter publication from their title led us to suppose that Mr. Coste had made some contribution to our knowledge of the chemistry of insect pigments. I read them from month to month in the hope of getting new light on this subject, which is of such general interest to both chemists and biologists: I regret to say that I have been grievously disappointed. The experiments thus far described amount simply to the fact — not altogether astonishing — that strong chemical reagents modify the colours of Lepidopterous pigments or in some cases dissolve them out of the wings. The bearing of these observations on the chemistry of the pigments is so remote as to be practically useless until we know something of the chemical nature of these pigments. The methods adopted by Mr. Coste are not likely to advance our knowledge in this direction very much, and it is certainly remarkable that in treating of yellows he makes no reference[1] to the only real contribution to the chemistry of Lepidopterous pigments, viz. the experiments made by Mr. Hopkins, and published in the Proceedings of the Chemical Society in 1889. Mr. Coste is no doubt acquainted with those South American *Papilios* with a large red spot on the hind wing, which spot loses its red colour and becomes of a brilliant metallic bluish green when the wing is tilted so that the incident and reflected rays form a very wide angle. The colour is in this case doubtless a mixed result, partly due to pigment and partly to interference. Now, anyone who has observed this and other similar colour phenomena in insects might describe his observations as contributions to the physics of insect colours. If he thought proper to adopt this course, he would be misleading physicists. The observation of the bare facts is as much a contribution to the physics of insect colours as the statement that a rainbow can be seen in the sky is a contribution to the physics of illuminated water-drops. It seems to me that Mr. Coste's experiments bear the same relationship to the chemistry of insect colours that the mere observation of interference colours in insects bears to the physics of insect colours.

Quite independent of the facts recorded by Mr. Coste is the interpretation which he puts upon them. Here I must decidedly express dissent. It cannot be admitted, because by the action of certain reagents green is changed into yellow or red into yellow, that this indicates the evolution of green or red from yellow. There is no evidence that this result is a reversion effect at all. The analogy between the action of strong acids in modifying the colour of an animal pigment and the effect of true reversion is forced, and has no parallel in natural processes. Hot water is a chemical reagent; by its action on the brown pigment of the lobster the latter becomes red. If from this observation I drew the inference that the ancestral lobster was red, and that the hot water produced a reversion effect, I do not think that Mr. Coste would agree with me.

R. MELDOLA.

Oxford, April 24.

Eozoon.

MR. GREGORY has, I fear, slightly mistaken the meaning of my remarks, which were intended rather to excuse than to blame him. The specimen of Eozoon collected by the late Mr. Vennor at Tudor was figured in connection with my paper of 1867 as a type specimen, in so far as macroscopical characters are concerned; but it does not follow that slices from specimens less perfect in that respect, and now in my collection, may not be more instructive as showing minute structures. I may refer in this connection to the three specimens from Tudor and Madoc (Madoc being in the same formation with Tudor) figured by Dr. Carpenter in our original paper in the Journal of the Geological Society, vol. xxiii., pl. xii., Fig. 1. If anyone will take the trouble to compare these with the figures in Mr. Gregory's paper in the same Journal, vol. xvii., he will have a singular and impressive illustration of the different ways in which things supposed to be the same may appear to observers of different types.

Mr. Gregory is in error in supposing that he could see in the cases of the Peter Redpath Museum my specimens from Tudor and Madoc. I have not yet been able to place there any portion of my microscopic cabinet of Eozoon; but only a few hand specimens sufficient to show students the ordinary types of the fossil.

As to the Laurentian age attributed to the Tudor beds, I have already explained that this I subsequently regarded as an error, and so stated not long after the date of the paper of 1867. I now regard them as less ancient, though of pre-Cambrian age.

I shall be happy to show to anyone my little collection from Tudor and Madoc, including specimens in which Carpenter detected the canal system; but of these particular specimens I have unfortunately no duplicates for distribution, and would prefer to exhibit the slices in the modes I have found best suited for the development of the structures; as otherwise there might be some doubt whether the resulting impressions would more resemble Mr. Gregory's figures or Dr. Carpenter's.

Montreal, April 6. J. WILLIAM DAWSON.

The Theory of Solutions.

I AM glad to see that as to the main point, the character of the "gaseous laws" of solutions, there seem to exist no more differences between Mr. Rodger and me. For Mr. Rodger, in his letter on p. 487 of NATURE, limits his remarks to some dialectical expressions, to cover an honourable retreat. I wish not to follow him on this way, because it is an endless one.

As to the application of van der Waals's formula on solutions, Mr. Rodger is now forced to confess that this application is not so "meaningless" as he has formerly written; but he asserts that, shortly spoken, the form of application given in my book is so. To say the truth, if I have to choose, as in this case, between the agreement of a formula with Mr. Rodger's opinion, and the agreement of this same formula with experiment, I prefer the latter. W. OSTWALD.

Leipzig, April 12.

Physiological Action of Diminished Atmospheric Pressure.

WITH reference to the effect of diminished atmospheric pressure on the vital powers, alluded to in Prof. Bonney's review of Mr. Whymper's "Travels among the Great Andes of the Equator" (NATURE, April 14, p. 561), I do not know whether it is worth while recalling the well-known fact that numerous passes in the Himalayas, ranging from 17,000 to 19,000 feet, are habitually traversed by the hillmen, in the summer, with their flocks of sheep and goats carrying borax, &c. The highest pass is said to exceed 20,400 feet. In the same mountains Messrs. Schlagintweit reached an altitude of about 22,200 feet (Proc. As. Soc. Bengal, January 1866), while Mr. W. W. Graham ascended to 23,500 feet in 1883 (NATURE, September 11, 1884). I have myself, on several occasions, been to elevations of 17,000 to 19,000 feet, and beyond shortness of breath when climbing, never experienced any ill effects except once, when I, the four plainsmen with me, and three out of a considerable number of hillmen, felt severe headache during the evening after crossing a high pass. My companion on one trip, however, almost invariably suffered very severely from mountain sickness under similar circumstances. F. R. MALLET.

18, The Common, Ealing.

Sensitive Water Jets.

A FORM of this effect lately presented itself, which seemed in some ways new. A thin jet, 5 feet high and arched so as to be 3 feet at the base, was falling in a feathery spray. At 13 feet distance a small Wimshurst machine was set going: not instantly, but after two minutes, the spray gathered itself up almost into one clear line: although the jet was turned up and down and the machine was discharged the falling water would not resolve itself again into spray for fifteen or twenty minutes. It is difficult to imagine the medium for this action: it is too indefinite, perhaps, to suppose that an indicator is found for the trembling of a disturbed ether while it is dying down.

The well-known experiment is not known enough, for it is not often described in books. Take a glass rod, electrified ever so little, to a certain point; at once the jet collects itself; a slight move away brings back the old disorder, while an inch nearer makes things much worse. It is a striking illustration to help one to imagine what the electrical forces of the air may do We can perhaps understand those thick thundery rain-drops, that almost allow us to pass between them while they are giving friendly warning of what will come. W. B. CROFT.

Winchester College, April 14.

[1] At least in NATURE: I have not the *Entomologist* at hand where I am writing.

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Double Orange.

THE abnormality in a Maltese orange described in NATURE of April 7 (p. 534) would appear of common occurrence in the Queensland or South Australian fruit. A friend assures me that in a case recently received from Australia, 80 per cent. of the contents showed small oranges, more or less perfect, either embedded in the pulp or in the rind. The quality of the fruit I observed was in no way affected. It would, however, be interesting to obtain further testimony. Although the small oranges may not affect the commercial value of the fruit, their presence must be undesirable in the groves where perfection is sought.

GERALD B. FRANCIS.

Katrine, Surbiton.

ON THE LINE SPECTRA OF THE ELEMENTS.

THE distribution of the lines in the spectra of the elements is by no means so irregular as it might seem at first sight. Since Lecoq de Boisbaudran, in 1869, discovered the general plan in the spectra of the alkali metals, a number of interesting facts have been brought to light, which will probably one of these days find their mechanical explanation, and will then greatly advance our knowledge of the molecules.

Mechanical explanations of some of the facts have been attempted already. Lecoq de Boisbaudran explains the fact that the rays of the alkali metals are, on the whole, less refrangible the greater the atomic weight, by observing that the oscillations of a body suspended in a given elastic medium will become less frequent when the mass of the body is increased. This explanation, however, seems to me to remain rather vague and unsatisfactory as long as it does not lead to any numerical results that agree with the observations. Taken literally, it makes the oscillation-frequency inversely proportional to the square root of the atomic weight, which is far from being the case.

A second well-established fact has received different explanations by Julius [1] and by Johnstone Stoney.[2] It has long been observed by Hartley that in the spectrum of several elements a number of doublets or triplets of lines appear, the oscillation-frequencies in each doublet or triplet differing by the same amount. Recent measurements by Prof. Kayser and myself have confirmed this observation. Julius believes that this phenomenon is due to a cause analogous to the combination tones in the theory of sound.

If two rays, with oscillation-frequencies α, β, combine with other rays, p, q, r, s, to oscillation-frequencies

$$\begin{array}{cccc} p+\alpha & q+\alpha & r+\alpha & +\alpha \\ p+\beta & q+\beta & r+\beta & s+\beta, \end{array}$$

the same difference $\alpha - \beta$ will occur several times. That the doublets under consideration are in many cases remarkably strong is accounted for by the fact that the intensity of the combination tone is proportional to the product of the intensities of the primary tones, so that it must become very strong when the amplitude of the primary tones is sufficiently increased.

Johnstone Stoney gives a different explanation of the doublets. He supposes that the path of the molecule from which light emanates is an ellipse, which by disturbing forces is gradually changed, and he shows that on this supposition, instead of one ray, two rays or more would originate, and the oscillation-frequencies of these rays would differ by an amount depending on the rate of change of the ellipse. If now, instead of the ellipse, the path of the molecule is any other curve, it can be considered as consisting of a number of superposed ellipses, all of which change in the same way on account of the disturbing forces. To each of the ellipses a doublet of lines corresponds, and the oscillation-frequencies of each doublet differ by the same amount. In this explanation I do not understand the decomposition of the arbitrary curve in a series of superposed ellipses. For the movement is supposed not to be periodical, and Fourier's theorem then would not apply, at least the periods of the superposed ellipses would not be definite, as long as there are no data except the arbitrary curve itself.

Besides, both Johnstone Stoney and Julius only try to explain one of a number of regularities that have been observed in the spectra of the elements. A plausible suggestion about the movement of the molecules ought to explain more than one of the observed phenomena. I think it may be useful to point out the other regularities that have been observed in the distribution of lines, and for which as yet no mechanical explanation has been attempted.

(1) The doublets and triplets existing in the spectrum of an element can be arranged in series which show an appearance of great regularity. These series seem to be analogous to the over-tones of a vibrating body. But they possess a remarkable peculiarity, which, as far as I know, is without analogy in the theory of sound. The difference of two consecutive oscillation-frequencies decreases as these increase, and there seems to exist a finite limit to the oscillation-frequencies of a series. If n represents integer numbers, the oscillation-frequencies of a series may with great accuracy be represented by the formula—

$$A - Bn^{-2} - Cn^{-4},$$

where A, B, C are positive constants. B has nearly the same value for all the series of the different spectra. A is the limit towards which the oscillation-frequency tends, when n increases.

(2) For elements that are chemically related, the series are distinctly homologous, both in appearance of the lines and in the values of A, B, C, and with increasing atomic weight shift towards the less refrangible end of the spectrum. Homologous series have been observed in the following groups of elements :—

Lithium, sodium, potassium, rubidium, cæsium ;
Copper, silver ;
Magnesium, calcium, strontium ;
Zinc, cadmium, mercury ;
Aluminium, indium, thallium.

In the first two and in the last group the series consist of doublets,[1] while in the remaining two groups they consist of triplets. Thus we may say that the spectrum shows a relationship between the elements similar to that between their chemical properties. It is interesting to note that magnesium forms a group with calcium and strontium, and appears more nearly related to them than to zinc, cadmium, and mercury.

(3) The doublets and triplets in each group broaden as the atomic weight increases. In the first group the difference of oscillation-frequencies is nearly proportional to the square of the atomic weight. The constant difference of the oscillation-frequencies in the doublets and triplets may also be noted in the values of A, B, C. For a series of doublets or triplets we have two or three different values of A, but only one value of B and one value of C.

(4) In each of the spectra of sodium, potassium, rubidium, and cæsium, a series of doublets has been observed, in which the oscillation-frequencies do not differ by a constant amount, the difference diminishing inversely proportional to n^4. For these series A and B have only one value each. The least refrangible doublet of the series has the same difference of oscillation-frequencies as the doublets in the other series of the same element. In the spectrum of lithium there is a homologous series of single lines. All the lines of these series have the same

[1] Julius, *Annales de l'École Polytechnique de Delft*, tome v. (1889).
[2] Stoney, Trans. of the Roy. Dublin Soc., vol. iv. (1891).

[1] Lithium has here to be excepted, whose lines are all single.

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character; they are strong and easily reversed, and in all of them the first doublet is situated on the less refrangible side of the spectrum, and all the others in the violet and ultra-violet. The series shift towards the less refrangible side with increasing atomic weight.

For further details the reader is referred to the following memoirs:—Kayser and Runge, "Ueber die Spectren der Elemente," *Abhandl. der Berl. Akademie*, 1890–92; Rydberg, "Recherches sur la constitution des spectres d'émission des élements chimiques," *Kongl. Svenska Vetenskaps-Akademiens Handlingar*, Bandet 23, No. 11, 1890.

C. RUNGE.

ABERRANT FOSSIL UNGULATES OF SOUTH AMERICA.

TILL within the last few years palæontologists and zoologists were being continually startled by the discovery of strange forms of extinct Ungulates which rewarded the researches conducted in the Tertiary rocks of the United States. The animals thus brought to the notice of the scientific world have, to a very large extent, modified our conceptions of the relationships of the various groups of hoofed or Ungulate Mammals to one another; and have led to the very general adoption of the view of the ordinal unity of all these multifarious types. Several of them, indeed, so far as we may judge from their mere skeletons, indicate signs of a transition between the Perissodactyle and Proboscidean modifications of Ungulate structure; but none of them tend in the least degree to break down the hard and fast line of demarcation between the Perissodactyle (odd-toed) and Artiodactyle (even-toed) modifications, which is maintained throughout all the known Tertiary deposits of the Old World. Moreover, after a little "shaking down," the whole of these North American Ungulates, with the exception of the curious Rodent-like *Tillotherium*, fall fairly well into their places in the Ungulate order; although some of the earlier and smaller types present indications of close affinity with the common stock from which we may presume both Ungulates and Carnivores to have taken origin.

At the present time the wave of discovery of new forms appears to be passing from the northern to the southern half of the New World; so that while the palæontologists of the United States are to a great extent engaged in the important task of revising and completing the preliminary work of the last twenty years, their *confrères* in Argentina are almost flooding scientific literature with descriptions—sometimes, it is to be feared, rather crude and hasty ones—of a number of new or hitherto imperfectly known forms of extinct mammals. This descriptive work has been mainly undertaken by Messrs. Ameghino, Burmeister, and Moreno. Unfortunately, however, the greater part of it is still in the form of preliminary notices, unaccompanied by illustrations; while on several points the three describers above mentioned are by no means in accord, and it is quite clear that unnecessary names have frequently been published. There is, indeed, one large illustrated work published by Dr. Ameghino; but since, so far as we are aware, there is only a single copy (in the Natural History Museum) in England, palæontologists have not the opportunity of paying it that attention in private study which its importance demands.

In spite, however, of these drawbacks, the information at present before us—imperfect though it be—introduces us to several groups of extinct Ungulates totally unlike any found in all the rest of the world put together, and which are of especial interest as tending to a certain extent to break down the distinction between Perissodactyles and Artiodactyles. It should be observed, before proceeding further, that the explorations conducted in Patagonia and various parts of Argentina have shown that the deposits containing mammalian remains, instead of being exclusively of Pleistocene age, comprise a large portion of the Tertiary period, probably extending down at least as far as the Oligocene; although the exact correlation of the different beds with European deposits is probably premature.

With these preliminary observations, and asking our readers at the same time to bear in mind that a considerable part of our knowledge is still in a very imperfect and crude condition, we propose to glance at some of the peculiarities presented by the more remarkable forms of Ungulates described from the deposits in question.

Since the date of the publication of the results of Darwin's voyage in the *Beagle*, we have been gradually acquiring a knowledge of the structure of that remarkable South American Ungulate known as *Macrauchenia*, of which the complete osteology has been described by Burmeister. This animal, which had the general proportions and size of a horse, conforms in several respects—more especially in having three-toed feet, in which the middle (third) digit is symmetrical in itself—so markedly with the Perissodactyles, that by common consent it has been generally regarded as an extremely aberrant member of that group. The molar teeth are, indeed, more like those of the Rhinoceros and Palæotherium than of any other Old World Ungulates, while the infolding of the enamel of the crowns of the incisors is a character known elsewhere only in the horses. The absence of any gap in the dental series, and the nearly even height of the teeth, are characters in which *Macrauchenia* agrees with the Old World *Anoplotherium*. Perissodactyle affinities are indicated by the presence of a third trochanter on the femur; but in certain peculiarities in the ankle-joint this animal differs from all typical Perissodactyles, and agrees with the Artiodactyles. Moreover, a certain peculiarity of structure in the vertebræ of the neck is repeated elsewhere only in the camels and llamas, which form an isolated group of Artiodactyles. In the complete closure of the orbit by bone, *Macrauchenia* resembles the horses and many Artiodactyles; but in the narial aperture being situated on the top of the skull between the orbits (whence the nostrils were probably produced in the form of a proboscis), it is absolutely peculiar.

There are thus many indications that, while *Macrauchenia* is a specialized form that can in no sense be regarded as the ancestral type from which Perissodactyles and Artiodactyles have originated, it retains certain generalized features which were probably directly derived from such ancestral stock.

Among the Ungulates discovered in Patagonia is one named *Proterotherium*, which was at one time referred to the Artiodactyles, but subsequently placed among the Perissodactyles. In the skull, so far as can be gathered from Ameghino's description, the orbit is closed, as in *Macrauchenia*, but the narial aperture appears to have had the normal position. The molar teeth are so like those of true Perissodactyles that they were originally described under the name of *Anchitherium*; but the rest of the dentition is very peculiar. Thus, in the upper jaw there appears to have been only a single pair of incisors in the premaxillæ, these being pyramidal and obliquely truncated like the canines of the pigs; and as there were no canines, it may be inferred that there was a long toothless interval in the jaw. In the lower jaw there were two pairs of incisors, and no canines. The lower molar teeth were inserted by four distinct roots—a feature unknown in any existing Perissodactyle, although occurring in the pig. In the limbs, both the front and hind feet were furnished with three complete toes, much resembling those of *Hipparion*; the ankle-joint is, however, said to resemble that of the Artiodactyles. We have no information as to the third trochanter of the femur. On the whole, this genus appears to indicate a

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Perissodactyle-like Ungulate, somewhat more specialized as regards its dentition than *Macrauchenia*, but exhibiting strongly-marked Artiodactyle affinities in the ankle-joint.

Still more remarkable are the generalized affinities displayed by the group known as the Toxodonts, of which the first representative was also discovered during Darwin's memorable voyage. These Ungulates cannot be included in either the Perissodactyla or Artiodactyla, and, therefore, come nearer the original generalized Ungulate stock than the animals already noticed. *Toxodon*, from the Pleistocene of Argentina, was of the approximate size of a Hippopotamus, and its osteology is tolerably well known. It takes its name from the curvature of the molar teeth, which approximate in structure to those of the Rhinoceros, and, like the incisors, have ever-growing roots. The front teeth are separated from the cheek-teeth by a considerable interval; the upper dental series being reduced in number by the loss of the outermost incisors and the canines, and the lower by the disappearance of the first premolars; the lower canine is, moreover, rudimentary. The feet conform to the Perissodactyle type in having three toes, of nearly equal size, and also in the interlocking of the bones of the upper and lower rows of the wrist- and ankle-joints. In the absence of a third trochanter to the femur, and also in the articulation of the fibula with the calcaneal bone of the ankle, as well as in the structure of the palatal and tympanic regions of the skull, *Toxodon* is, however, constructed on a decided Artiodactyle type; so that its characters are to a great extent intermediate between the existing members of the two groups.

Going back to the earlier Tertiaries of Argentina and Patagonia, a number of Ungulates allied to *Toxodon*, but with much more generalized characters, have been brought to light. The skulls from Patagonia brought back by Darwin, and named *Nesodon*, also belong to this same generalized group. In *Nesodon* there is the full complement of 44 teeth; and the same formula also obtains in the recently described *Protoxodon*, in which the feet are known to have been tridactylous in both limbs, although retaining rudiments of the metacarpals of the first and second digits, and being of a longer and more slender type than in *Toxodon*. The allied animals described as *Acrotherium*, some of which were about the size of a pig, present a peculiarity totally unknown among other Ungulates; and, indeed, in any Eutherian Mammals except some individuals of the small African long-eared fox (*Otocyon*). This peculiarity consists in the presence of eight cheek-teeth on either side of each jaw; the constancy of this character being proved by its occurrence in a considerable number of specimens. Whereas, however, in *Otocyon* the eight cheek-teeth are reckoned as four premolars and four true molars, in *Acrotherium* there are said to be five premolars and three true molars. If this interpretation be correct, it is difficult to point out a probable derivation for this most remarkable type of dentition, since no other heterodont mammals are definitely known to have more than four premolars.

If, however, the cheek-teeth really prove to be four premolars and four true molars, there might be a possibility of direct inheritance of the fourth molar of the Marsupials, although even then there is the difficulty that none of the Lower Eocene Ungulates of the United States are known to have possessed more than three of these teeth. And the probability accordingly suggests itself that the additional tooth may be an acquired redundancy. There are a number of other more or less closely allied types which have received distinct generic names, such as *Colpodon* and *Adinotherium*, but it is at present somewhat difficult to realize all their distinctive features and peculiarities. One genus, however, if the specimen on which it was established is normal, is so remarkable as to call for special notice; and taken together with *Acrotherium* seems to show that these South American Ungulates ran riot in the disregard of all rules as to the number and arrangement of their teeth. The genus in question is *Trigodon*, founded upon the lower jaw of an animal about the size of a pig, but evidently related in the structure of its cheek-teeth to *Toxodon*. In this mandible the middle of the extremity of the long and narrow symphysis is occupied by a single cylindrical incisor tooth, flanked by a pair of larger incisors, and these, again, by the still larger triangular canines. If normal (and from Dr. Moreno's description and figure it would seem to be so) this single median incisor is totally unique in the whole mammalian class.

A still more remarkable and puzzling group is typically represented by the long-known *Typotherium* from some of the Tertiaries of Argentina, which, while presenting many dental characters connecting it with the Toxodonts, has upper incisors resembling those of the Rodents; with most of which it also agrees in the presence of clavicles, which are invariably absent in all true Ungulates. The number of the teeth is similar to that obtaining in many Rodents, with the exception that there are two pairs of lower incisors. An allied type has, however, three pairs of these teeth, thus departing further from the Rodent type; and the skull of both genera is constructed on the Ungulate plan. All the teeth are rootless. From other beds in Argentina we have the genus described as *Hegetotherium*, which, while having rootless teeth, differs from *Typotherium* in possessing the whole typical series of 44, without any marked interval between them. Here, then, we have almost entirely lost the Rodent features which are so marked in *Typotherium*, and thus revert nearer to a normal Ungulate type; it is unknown whether clavicles were present. Still more generalized is an allied group typified by *Interatherium*, in which the dentition is always complete, the anterior premolars have distinct roots, and the incisors conical roots. This genus and the allied *Protypotherium* thus appear to be connected both with *Typotherium* and the Toxodonts; the specific name *rodens* applied to one of the species of *Protypotherium* apparently indicating the existence of Rodent-like upper incisors.

The existence of these intermediate forms renders it exceedingly difficult to come to any satisfactory conclusion as to whether *Typotherium* really has any genetic affinity with the Rodents (among which it was placed by the late Mr. Alston); for if there be such relationship it would seem to imply the descent of all Rodents from a form more or less closely allied to *Interatherium*—a view which can scarcely be maintained.

That these Typotheroids were, however, in some manner connected with the Toxodonts is tolerably clear; and there are nearly equally clear indications of a more or less distant connection between the Toxodonts and the Macrauchenias. The most probable explanation of the latter relationship is that both groups took origin from generalized Ungulates allied to those found in the Eocene of the United States, and known as the Condylarthra, which appear to have been the common ancestral stock of both the Artiodactyle and Perissodactyle modifications of the order. On this view the retention of characters common to both the groups last-mentioned by the Toxodonts and Macrauchenias is readily accounted for; the Macrauchenias having acquired sufficiently well-marked Perissodactyle characters to admit of their inclusion in that group, while the Toxodonts cannot be placed in either of the two existing divisions of typical Ungulates. Having thus diverged at an early epoch (perhaps in the neighbourhood of Central America) from the original generalized Ungulate stock, the ancestral Toxodonts and Macrauchenias become the dominant forms in South America, where they appear to have developed into such numerous and unexpected modifications of struc-

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ture, as to render the task of deciphering their mutual relationships and determining their exact systematic positions an exceedingly difficult, if not an impossible one. At the same time, however, it does not appear to us that the existence of these puzzling and aberrant types need interfere in the least degree with the commonly-accepted classification of the Ungulates, although there may be legitimate doubt as to the propriety of including the Macrauchenias among the Perissodactyles, instead of retaining them with the Toxodonts as a special group, exhibiting on the one hand many generalized features, coupled with extreme specialization in other respects.

R. L.

THE CHANGEFULNESS OF TEMPERATURE AS AN ELEMENT OF CLIMATE.

ONE of the features in which the climates of great continents most contrast with those of oceanic islands, and those of higher latitudes with the climates of the tropics, is the greater range through which the temperature varies between night and day, and between winter and summer. Another, perhaps not less important, is the greater changefulness of the temperature from day to day. Both of these are comprised under the general expression *variability of temperature*,[1] and they are similar in their effects on living organisms, but they depend on very different causes, and in their local association are often manifested in very different degrees; places with a great annual and diurnal range of temperature, displaying great constancy of climate at any given season of the year, while others, at which the former variations are moderate in amount, are, nevertheless, subject to irregular vicissitudes of considerable magnitnde. The Punjab and Sind may be cited as examples of the former class, Western and Central Europe of the latter.

Now, although from a sanitary point of view these two kinds of variation are of equal importance, the degrees in which they have respectively engaged the attention of climatologists and others are strikingly different. While the daily and annual range of temperature of all the more important and many minor places that have furnished meteorological registers are now well known, or are easily ascertainable from published records, the first systematic inquiry into the changefulness of temperature as an element of climate was that made by Prof. Hann in a memoir published in the *Sitzungsberichte* of the Vienna Academy of Sciences in 1875. In this paper, Dr. Hann tabulated the results of ninety stations, seven of which are situated in the southern hemisphere, and the remainder chiefly in Europe, Siberia, Canada, and the United States. The extraction of the data was not a little laborious, since it consisted in taking out from the daily registers, of generally from five to ten years, the differences of the mean temperatures of every pair of successive days throughout the whole period; then classifying them according to algebraic sign, as rises or falls of temperature, and also, in certain cases, according to their incremental values. The means of these different categories were then taken month by month, and the results are given in numerous tables in the memoir. The changefulness of temperature at any given place is the general mean of all changes during the period considered, irrespective of their being rise or fall. As instances of these, I take the following three stations, representing respectively the climates of Siberia, England, and Canada. They show the average change of any two consecutive days on the Fahrenheit scale.

	Barnaul.	Oxford.	Toronto.
January	8·8	3·4	6·8
February	8·5	3·1	6·8
March	7·2	2·9	4·9
April	4·7	2·9	4·0
May	5·6	3·1	3·8
June	4·3	2·7	3·8
July	3·4	2·3	3·6
August	3·2	2·5	3·2
September... ...	4·5	2·5	4·5
October	5·6	3·4	4·1
November	9·0	3·6	4·5
December	10·1	3·8	6·7
Year ...	6·3	3·1	4·7

These three stations serve to illustrate the fact, amply confirmed by the general tables, that temperature is subject to greater and more rapid changes in the winter than in the summer; either December or January being, as a rule, the month of greatest variability.

Since the publication of this memoir, the inquiry thus started by Dr. Hann has been followed up by several writers with especial reference to particular countries. Prof. O. Döring, for instance, has thus discussed the statistics of the Argentine Republic; Herr E. Wahlén, those of 18 stations in Russia; Dr. V. Kremer, those of 57 stations in Northern Germany; and Mr. Robert Scott, those of 7 observatories in the British Isles, at which the temperature has been recorded by thermographs since 1869. These are Valentia, Armagh, Glasgow, Aberdeen, Falmouth, Stonyhurst, and Kew. At all these stations the variation was found to be less than at Oxford; but this may be partly due to the longer period (15 years) over which the records extend, and partly also to the fact that the daily means compared are those of the twenty-four hourly measurements of the thermograph curve, whereas the Oxford register was for 10 years only, and the observations less numerous. On the general average of the year, it was greatest at Kew (2°·7), and least at Falmouth and Valencia (1°·9).

Finally, Dr. Hann has resumed the subject in a memoir published in the Transactions of the Vienna Academy,[1] in which he discusses the temperature records of 66 stations in the Austrian Empire and the adjacent territories, of which one-half extend over from 10 to 20 years, and the majority of the remainder over at least five years; all, however, are corrected to the period 1871–80. In the case of Vienna, not less than 91 years have been included in the reckoning, and this register affords the means of comparing the results of any decade with those of a long period.

The first point that stands out in the results of this discussion is that even a period of ten years is insufficient to give more than an approximate value. The general mean change at Vienna, between any two consecutive days, is 3°·4 F., but in the decade 1861–70 it was only 3°·26, whereas in the decades 1801–10 and 1871–80 it averaged 3°·53. The means of the individual months show much greater variation; that of December especially, ranging between 3°·2 and 4°·3 in different decennia, or through 30 per cent. of the general mean for the month. It is evident that when computed from shorter periods than ten years the discrepancies will

1 The term "variability" of temperature, adopted by Mr. Scott for the element now in question, has been already used in so many different senses, that in this paper I have adopted in preference the term "changefulness," which is not open to the same objection.

1 "Die Veränderlichkeit der Temperatur in Oesterreich," von J. Hann, W.M.K. Akad., aus dem lviii. Bande der *Mat. Naturwiss. Classe der k. Akad. d. Wissenschaften.*

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be still greater. In order, therefore, to obtain comparable values, even for neighbouring stations, it is essential that the data compared should be those of the same interval.

Both as regards season and amount, the changefulness of temperature depends very greatly on local geographical circumstances, so that neighbouring places very often differ greatly from each other. In Europe it increases from west to east and from south to north, in both cases towards the interior of the continent. It increases also on the whole with altitude, but very irregularly, being great on exposed plateaux, and comparatively small on mountain peaks. Places situated in valleys show very great differences, according to their exposure. Among the Austrian stations, those on the southern slopes of the Alps have the greatest vicissitudes, owing to the warmth they acquire in sunny weather and the consequent greater fall of temperature when a change of weather sets in. In general the changes of temperature at high elevations are greater than at low altitudes in summer, but less in the winter season. In the high mountain valleys in spring the changes are much smaller than on the neighbouring plains.

In the British Isles, Mr. Scott found that the number of rises exceeding 5° between any two consecutive days was greater than the number of falls of the same amount, and also that the mean value of the rises exceeds that of the falls. In Austria, also, except in the Southern Tyrol and on the coasts of the Adriatic, rapid rises are greater than rapid falls in the winter, and less in the summer; but on the whole the former preponderate. In the south, however, rapid falls are greater than rapid rises at all times of year, and therefore also on the mean of the year. This peculiarity is a still more marked characteristic of lower latitudes, since in Northern India it was found that rapid falls are about three times as numerous as rapid rises, and on the whole greater in amount.

The duration of rises of temperature is somewhat greater than that of falls, and both are rather greater at mountain stations than at low levels. Thus the passage of a wave of temperature, on the mean of the two stations Klagenfurt and Salzburg, occupies, on an average, 4·56 days, on the Sonnblick 4·93 days; or, in other words, 6½ waves pass within the month at the higher and 7 at the lower stations. The longest period of continuous cooling that occurred at any station was ten days at the mountain observatory of Hoch Obir, and the longest continuous rise of temperature ten days at Klagenfurt. There is a marked annual periodicity in the length of the temperature waves, with two epochs of maximum, viz. in March and September, and two of minimum, in July and December. From the data afforded by certain stations in Austria and Saxony, Dr. Hann computes the following formula for their annual variation in Central Europe—

$$4{\cdot}813 + 0{\cdot}138 \sin(26^\circ 45' + x) + 0{\cdot}164 \sin(318^\circ 27' + 2x).$$

The last subject investigated in Dr. Hann's memoir is the question whether the inter-diurnal changefulness of temperature shows any periodical variation during the sun-spot period; for which purpose he takes the 90 years' registers of Vienna, Wilna, and Warsaw. He finds that on the mean of these stations a certain minute variation is indeed apparent, but it is one of two maxima and two minima, and the whole range is so small that it is doubtful whether it is other than fortuitous.

In the foregoing paragraphs only a few of the more important results of Prof. Hann's investigation have been noticed. His memoir contains many others of interest, well worthy of study, and forming important contributions to general climatology; and like the original memoir, published seventeen years ago, it will doubtless stimulate others to prosecute the subject. It is especially important from a medical point of view that the statistics of all health resorts should be analyzed in the manner of which Prof. Hann has here given so admirable an example.

H. F. B.

FORESTRY IN AMERICA.[1]

IT cannot be said that, as far as the issue of reports and pamphlets on forestry is concerned the Agricultural Department at Washington has been idle; if only this activity would resolve itself into the establishment of a State Forest Service, and the formation of State forests out of the wreck of the former forest wealth of North America!

An important series of papers on forest matters has come to hand, and though they date as far back as 1889, they are probably new to many of the readers of NATURE.

The first paper is by Dr. James, Professor of Public Finance and Administration in the University of Pennsylvania, and is entitled "The Government in its Relation to Forests." The Professor has evidently studied his subject thoroughly, and the remedy he proposes is the exact counterpart of that which has been so successfully applied to the forests of India. He commences by stating that the forests of any large country not only constitute a large portion of its wealth, but form the indispensable basis of a flourishing manufacturing and commercial industry. They are also one of the most important elements in determining the climatic conditions of a region, and, through these, the distribution of the population, of industrial pursuits, and of disease and health. He states that the value of the forest crop in the United States in 1880, the census year, was 700,000,000 dollars (= £140,000,000), and that if the value of the total annual output of the mines, quarries, and petroleum wells were added to the estimated value of all steamboats and other craft on American waters, it would still be less than the value of the forest crop, by a sum sufficient to purchase all the canals, telegraph companies, and construct and equip all the telephone lines in the States.

He then shows how Government has fostered agriculture by offering land on easy terms, by establishing model farms and agricultural schools, by improving the breed of stock, by free distribution of seed, and in many other ways; it has also assisted manufactures by the protective tariff, bounties, and exhibitions, &c.; and that vast sums have been spent by the State on improving rivers and harbours, and on the general means of communication—railroads and roads. Game and fish are also protected by the State, but although from their forests the Americans have been drawing more natural wealth than from all other sources together, yet practically nothing has been done to preserve them from the devastations of selfish people. Besides the great demands on the forests for timber, three-fifths of the people in the States use wood for ordinary domestic fuel, and the value of the wood fuel annually consumed is placed at 325,000,000 dollars.

Prof. James then treats at length of the vast indirect value of forests in maintaining a steady supply of water in rivers, and preventing floods. He shows that the maintenance of a system of factories and mills dependent on a watercourse becomes impossible when the stream is converted into a mountain torrent for one quarter of the year and is all but dry during another quarter; and instances the River Schuylkill, from which Philadelphia draws its water-supply, where the current has become too shallow and sluggish to carry off the ever-increasing

1 "Department of Agriculture, Forestry Division. Bulletin No. 2.—Report on the Forest Condition of the Rocky Mountains, and other Papers." With a Map showing location of Forest Areas. Second Edition. (Washington: Government Press, 1889.)

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quantity of impurities which pour into it, and consequently the quality of its water is steadily deteriorating.

The Professor considers it proved by European experience that a certain percentage of forest land is indispensable for any civilized country, and that when the forest area sinks below that percentage, through carelessness, or a selfish desire to get all the advantages from the resources of a country for the present generation, regardless of the interests of posterity, the result can only be an impaired industry and declining prosperity. He asserts that in the United States nothing is being done to cultivate forests, whilst vast areas, besides those which fall under the axe, are being wasted by fires and by unregulated grazing; so that, to put it mildly, the Americans are using up their forests at a much greater rate than they are replacing them, and are changing the character of their streams, soil, and local climate. Emphasis is laid on the fact that *tree-planting* is not *forest-culture*, and based on the experience taken from European countries, Mr. James insists that only the State can insure the preservation of the forests of America, and that private enterprise is powerless to prevent their eventual destruction.

His proposals to remedy matters are therefore that the Federal and State Governments should remove timber lands from the list of lands for sale, and after a thorough examination as to what forests are of climatic and industrial importance, should retain them under the control of Government. He also advocates the establishment of a School of Forestry, where men could be trained to manage the extensive tracts of forest lands in the ownership either of private individuals or of the State; and calls for further legislation, and active enforcement of existing laws to protect forests from fires and browsing animals. Here we have in a nutshell a proper forest policy sketched out for the United States; and it remains to be seen whether there is sufficient patriotism in the leading men to carry it out, or whether the great power of the timber trade, which has always insisted on non-interference with their business on the part of the State, will still obstruct the road to progress.

There is not space for much more than mere reference to the other papers contained in the Bulletin, the first being a most comprehensive report, by Colonel E. T. Ensign, on the forest conditions of the Rocky Mountains, showing the estimated area of forest still existing in each county of the States of Idaho, Montana, Wyoming, Colorado, and New Mexico, in 1887. A coloured map of the area shows the position and extent of the forest tracts. This report concludes with a most useful tabular statement, giving the area of forest in each county and for each State, as well as the character of the forest growth, the uses made of the timber, the principal causes of destruction of the forests, chiefly fires. Measures are suggested for the adequate protection of the forest growth, and any noticeable changes in the flow and volume of water in streams are noted. Under this head, we find that the streams have diminished in volume and their flow has become more intermittent in one-quarter of the ninety-one counties referred to, which altogether comprise an area of 555,081 square miles, still containing 83,460 square miles of forests in 1887.

The other papers are: "The Forest Flora of the Rocky Mountain Region," by G. B. Sudworth, and "On the Climate of Colorado and its Effects on Trees," by G. B. Parsons. The latter ascribes the barrenness of the eastern slopes of the Rocky Mountains to the extremes of temperature, and to the desiccating power of the north and north-west winds, which are frequently powerful enough to bark young trees by pelting them with gravel.

The Bulletin closes with a valuable paper on "Snow Slides and Avalanches," by B. E. Fernow, the present Chief of the Forestry Division of the Washington Agricultural Department.

W. R. FISHER.

NOTES.

THE following are the members of the Royal Commission appointed to investigate the question of a Teaching University for London:—Lord Cowper (Chairman), Lord Reay, Bishop Barry, Sir Lyon Playfair, Sir William Scovell Savory, Sir George Murray Humphry, Mr. George G. Ramsay, Rev. Canon Browne, Mr. Henry Sidgwick, Mr. John Scott Burdon Sanderson, Mr. James Anstie, Mr. Ralph Charlton Palmer, and Mr. Gerald Henry Rendall. No one who has devoted serious attention to the subject is likely to be of opinion that the choice of Commissioners is satisfactory. It shows that the Government has not grasped the problem.

THE International Congress of Chemical Nomenclature at Geneva has been attended by many representatives from various European countries. The representatives from England are Prof. H. E. Armstrong, F.R.S., Dr. J. H. Gladstone, F.R.S., and Prof. W. Ramsay, F.R.S.

PROF. A. CHAUVEAU has been elected to the presidency of the Société de Biologie, in place of Prof. Brown-Séquard, whose term has expired. The Société de Biologie was founded by Claude Bernard and a group of friends. Claude Bernard and Paul Bert were Presidents before M. Brown-Séquard.

A COMMITTEE has been formed to make preparations for the erection of a monument to Prof. de Quatrefages in his native village, Vallerangue (Gard).

WE regret to have to announce the death of Prof. Annibale de Gasparis, Director of the Observatory at Naples, which took place on the 21st of this month. Born in Bugnara, in the province of Aquila, on November 9, 1819, he passed the first few years of his youth in Tocco Casuria, where he studied classics. Going thence to Naples in 1838 he began the study of mathematics under Prof. Tucci, dealing specially with the problems relating to bridges and rivers. Afterwards he devoted himself to astronomy, in which he soon gained great celebrity. In 1840 he was appointed assistant at the Capodimonte Observatory, where he became a diligent observer and an industrious calculator. His discovery of the three minor planets—Hygieia, Parthenope, and Egeria—created a great stir in the scientific world, and secured for him the Royal Astronomical Society's medal. Nominated as Director of the Observatory in 1864, owing to the death of Capocci, he worked incessantly for the advancement of practical astronomy, and followed up his observations for the capture of small planets. Eunomia, Psyche, Massilia, Themis, Ausonia, and Beatrix were all discovered owing to his ever careful scrutiny. His theoretical labours included many on pure mathematics, while those on astronomy related principally to the best methods of determining the orbits of comets. The investigations he carried on from time to time were numerous, and the results appeared in many periodicals, of which we may mention the *Atti della R. Accademia delle Scienze Fisiche e Mathematiche de Napoli* and the *Astronomische Nachrichten*. De Gasparis was naturally robust, and enjoyed good health until he was attacked by the maladies which killed him. His powers of work were tremendous; he was always making either some calculation or observation. Being taken ill rather suddenly, he went away to recruit, but he became worse and worse, until at last he could not move. The sad days of the last year of his life he spent in reading the classics which he loved best, until his sight failed him.

MR. JOHN HARTNUP, the Astronomer to the Mersey Docks and Harbour Board, met with a fatal accident on the 21st while performing one of his Observatory duties. It seems that he was accustomed to examine occasionally the anemometers

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situated on the flat roof of the building, the roof being skirted by a low wall about 20 inches in height. Being near the wall, and looking up at the anemometers, he was seized with a fit of giddiness, such as he had lately been accustomed to, and fell to the ground, breaking his neck. His sister-in-law, who saw the sad accident, had previously been cautioned by him not to go too near the wall when on the roof, for he considered it a dangerously low one. Mr. Hartnup was a member of the Royal Astronomical and Liverpool Astronomical Societies, and a Fellow of the Meteorological Society. He had succeeded his father in 1885, so that he was thoroughly familiar with the Observatory in which he had to work.

MISS AMELIA B. EDWARDS, whose death we have already recorded, has in her will endowed a Chair of Egyptology. Her library, which is very valuable, she has bequeathed to Somerville Hall, Oxford.

WE regret to hear that the venerable Prof. Svén Lovén has been compelled, as a result of the influenza, to retire from his position as Senior Keeper in the State Museum of Natural History, at Stockholm, where he has been active for fifty-one years. Prof. Lovén is now seeing through the press two important works on Echinoderm morphology, one dealing with the young stages of Echinoidea, the other with the Cystidea. We trust he may long be spared to enrich the world with these and other fruits of his wide knowledge and deep thought.

THE twelfth annual exhibition of natural history objects of the South London Natural History Society will be held on May 5 and 6 at the Bridge House Hotel, London Bridge, the whole of which building has been secured for the occasion. These exhibitions are growing in popularity, and several thousand visitors have each year taken lively interest in the exhibits. This year they will be exceptionally varied and novel. Lectures will be delivered by Mr. F. Enock on "The Life-history of the British Trapdoor Spider," by Mr. Step on "Edible and Poisonous Fungi," and by Mr. George Day on "Various Natural History Subjects."

AN Exhibition which will be interesting from a scientific as well as from a popular point of view will be held this year in the open ground near the Earl's Court railway station. It will illustrate the development of horticulture, and as Mr. H. E. Milner, F.L.S., is the chairman of the executive committee, we may expect that the scheme will be admirably carried out. There are to be examples of the gardens of all ages, including restorations of the ancient gardens of Egypt, Greece, and Rome; copies of those in China and Japan, and types of the Baronial, Italian, Tudor, Jacobean, Georgian, and Victorian eras. A large sub-tropical garden will be laid out, and there will be representations of the tea gardens of India and Ceylon. Various foreign countries—especially Belgium, Holland, France, Italy, and Germany—will co-operate to show the progress they have made in horticulture.

IN September a splendid Exhibition of fruit will be held in a temporary building, which is to be erected on a site on the Thames Embankment, near Blackfriars, lent for the purpose by the City Corporation. The Exhibition will be held under the auspices of the Fruiterers' and Gardeners' Companies, the Royal Horticultural Society, the British Fruit Growers' Association, and other kindred societies, and will last at least a week. In connection with the Exhibition lectures and object-lessons will be given on subjects relating to fruit culture and the planting of fruit trees.

THE Technical Instruction Committee of the Essex County Council appointed last year an organizing joint committee, consisting of six members of their own body, and six members of the Essex Field Club. By means of the funds voted to this joint committee, peripatetic courses of lectures on various scientific and technical subjects have been carried on in different rural centres with considerable success during the past year. The principle on which the joint committee has carried on this work has been to employ only thoroughly qualified lecturers, and to insist upon the instruction being made as practical as possible. In some cases the lectures have been followed by practical work, in which the students have been taught how to use the microscope, and to dissect plants, as a means of acquiring a good working knowledge of vegetable physiology. This practical work has been so much appreciated in the rural centres that there has been an actual competition to gain admission to the class, the number of students being necessarily limited by the supply of apparatus and material. One of the most popular courses given under the auspices of the joint committee has been that on general science, a kind of elementary introductory course showing the advantage of acquiring scientific knowledge in its applications to daily life. There has been such a demand for this subject that four lecturers have been engaged to meet the wants of different parts of the county. Special courses on marketable fish and oyster culture will shortly be commenced for the benefit of the maritime centres. The organizing joint committee has, we are informed, not been reappointed by the new County Council, but that its labours have been appreciated is shown by the fact that the Council has decided to merge the joint committee in the main Technical Instruction Committee. The latter will thus secure directly the co-operation of the six representatives of the Essex Field Club, among whom are Sir Henry Roscoe, Prof. Meldola, and Mr. G. J. Symons. Essex is to be congratulated upon the wisdom which its Councillors have displayed in securing the services of such well-known scientific advisers.

A VERY beautiful aurora was visible from Westgate-on-Sea on Monday evening last. When it was first observed, about 9.30 p.m., the sky was brilliantly illuminated to a height of about 30° above the horizon, extending laterally quite 50°. It seemed to be decidedly of a pinkish colour, but to all appearance this tint gradually disappeared. About ten minutes later, two fine streamers were thrown out, their approximate positions on the celestial sphere corresponding to the lines joining the stars π^2 Cygni, ϵ Draconis, and α Lacertæ, π Cephei. Their light was considerably more intense than the aurora itself, the beams reminding one rather of those produced by a strong search light. East and west of these, two more beautiful bright streaks were shot out, extending to a height not quite so great as the former two. The west one became especially fine, its light exceeding that of any of the others. Their positions, as near as could be gathered, lay between the stars ρ Cygni and o Draconis for the west one, and for the east one o Andromedæ and τ Cassiopeiæ. Five minutes later these vanished, and the two central ones merged into one and also disappeared. At 12.30, only one streamer was visible, while its light and that of the aurora itself was of a very feeble nature.

THE weather during the past week has become on the whole much more seasonable all over the country. Westerly winds have prevailed during the greater part of the period, and the air has been mild and genial; but the temperature, although high for the season, was lower than at the commencement of the month. Rain has fallen very generally within the last few days in all parts of the kingdom, and thunder, lightning, and hail have occurred in many places. A brilliant aurora, to which reference is made in the preceding note and in several letters, was observed in Scotland and in several parts of England during Monday night. The weather report of the Meteorological Office for the week ending April 23, showed that bright

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sunshine, although less than in preceding weeks, still exceeded the mean value in nearly all districts.

LAST week California was visited by the most severe earthquake which has been known in that region since 1868. On April 19 shocks were felt over a distance of 200 miles, the intensity varying at different points. In San Francisco a number of large buildings trembled perceptibly, but only one was damaged—an old church building until lately occupied by the Academy of Science. The front wall gave way, tearing away the balconies. The centre of the disturbance was Vacaville, where a number of brick buildings both in the town and in the vicinity were destroyed or damaged. Many walls also fell into the streets. At Winters and Dixon serious damage was done to buildings. On April 21 further shocks occurred at San Francisco, and were felt in the surrounding districts. A number of buildings were demolished at Winters, and several persons received injuries. Eight distinct vibrations were felt. At Biggs clocks stopped, and plaster fell from the ceilings of the houses. At Woodville several brick buildings were damaged, while at Vacaville some walls which had been cracked by the previous shocks were demolished, and the ceilings in most of the houses were cracked. At Sacramento also some damage was caused, but the place which seems to have suffered most severely was Dixon, where extensive damage had already been caused by the shocks of April 19.

DR. A. C. OUDEMANS, Director of the Zoological Gardens at the Hague, has for some years made the sea-serpent a subject of special study, and now he is about to issue a book in which he will present his conclusions. He states in a prospectus that he was attracted to the question by "an account of the appearance of a sea-serpent published in NATURE of November 8, 1880." As NATURE was not published on November 8, 1880, a good many people may be tempted to think that this reference (due, of course, to a slip of the pen or to a misprint) is very suitable to the nature of the animal to which it relates. Dr. Oudemans has placed side by side "all the accounts, tales, and reports of this great unknown animal," and has convinced himself that "through all the reports there runs only *one* red thread, that there must be *one* single animal species which has given rise to *all* the reports." The author has chosen to write in English, because it is a language "known to all navigators, as well as to all zoologists, and other men of education." The full title of the work will be "The Great Sea-Serpent. An Historical and Critical Treatise. With the Reports of 166 Appearances, the Suppositions and Suggestions of Scientific and non-Scientific Persons, and the Author's Conclusions." There will be 82 illustrations.

WE are glad to hear that Mr. J. J. Wild, Ph.D., who accompanied the *Challenger* as secretary to the Director (Sir Wyville Thomson) and artist to the Expedition, is settled in Melbourne, Victoria, where he is engaged in producing plates illustrative of the zoology and palæontology of the colony, under the direction of Sir Frederick MacCoy, F.R.S. He is at present figuring the fossil remains of Acanthodian fishes discovered in the Old Red Sandstone of Mansfield, Victoria.

THE programme of the first series of summer excursions of the Manchester Field Naturalists' Society has just been issued. Mr. Leo Grindon, the founder of the Society thirty-two years since, has been compelled, by advancing years and impaired health, to resign the presidential chair, which Mr. Chas. Bailey has undertaken to occupy. Mr. Grindon retains the office of botanical referee, with the assistance of Mr. W. Gee, who is engaged in teaching natural history subjects under the Science and Art Department. A special study, appropriate to the season and locality, is appointed for each meeting; and a field card, directing observation in each of the seven sections of the Society, is in preparation. A course of lessons, treating in detail of the leading natural families of the Manchester Flora, is being given in the city.

AT the meeting of the Royal Statistical Society on Tuesday, Mr. R. Henry Rew read a valuable paper on the statistics of the production and consumption of milk and milk products in Great Britain. The subject, as he pointed out, is one of extreme complexity. The effective production of milk by a single cow ranges from *nil* (in the case of a cow which only rears her calf) to 1200 gallons or more. The number of cows and heifers returned in 1890, the year taken as a basis for calculation, was 3,956,220, of which it is reckoned that 3,544,575 are productive. Returns now collected from a large number of districts, together with other data, justified an estimate of 400 gallons per cow, making the total quantity of milk available for consumption in various forms in the United Kingdom 1417 million gallons. The number of cows has decreased in proportion to population. In Great Britain there were in the period 1866-70 82·1 cows per 1000 of population, while in 1886-90 there were only 77·9. The absolute number of cows had increased, but not sufficiently to keep pace with the growth of population. The latest return (for 1891) is more encouraging, showing as it does the largest number of cows on record. In Ireland the period 1886-90 showed a higher proportion (290·8) of cows to population than in any of the four preceding quinquennial periods, but this was due not to an increase in the number of cattle, but to the decrease of population. The 1417 million gallons of milk produced in the United Kingdom is thus accounted for:—Consumed as milk 570 million gallons; butter, 617 million gallons (representing 105,000 tons of butter); cheese, 224 million gallons (representing 100,000 tons of cheese); miscellaneous (condensed milk, &c.), 6 million gallons. Mr. Rew admitted that the results arrived at were only tentative. He expressed a hope that before long some official help might be given in the solution of a problem which was of the deepest interest to statisticians, agriculturists, and social economists.

THE Bath and West of England Society decided some time ago to appoint a research chemist to make investigations upon the making of cheddar cheese. Mr. Fred. Jas. Lloyd was chosen to fill this post, and he has recently presented his report of the work done in August, September, and October of last year. The results obtained, though by no means complete, are both valuable and interesting, and it is to be hoped that the Society will continue and extend the work. The experiments were made at the Society's Dairy School, near Frome, and it was found possible to make a cheese in such a way as to be guided in judging the condition of the curd by determinations of acidity alone. The product was a decided success in every respect. The average acidity of the mixed milk before adding rennet was ·24 per cent., but on setting the whey only showed ·16 per cent. of lactic acid. It was proved, by continued experiment, that when the whey showed a percentage of acidity slightly greater than that in the milk before renneting the process was sufficiently advanced to draw off the whey and pile the curd. Determinations of acidity in the later stages of manufacture have yielded similar results, and it appears to be certain that the careful development of definite amounts of lactic acid at definite steps in the process is essential to success. The bacteriological observations show that, although very many organisms are liable to get into the milk, the majority of them are not able to exist in an acid material, consequently by insuring a proper development of acidity in the curd we destroy their activity, which would otherwise spoil the cheese. Not only does the *Bacillus acidi lactici* play the most important part in the making of the cheese, but it is also the chief agent in the ripening process.

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IT is well known that serious loss is caused in the various Australian colonies by the ravages of the rust fungus in wheat. An Intercolonial Conference met to consider the subject in 1890, and this body has since held two other meetings, the third having taken place at Melbourne last month. Many experiments have been made, and it has been clearly shown that there are several varieties of wheat which, except under very unusual circumstances, are never seriously attacked by rust. It has also been shown that in many districts early sown wheats of a rust-liable kind generally escape damage by rust, when the same wheats sown late suffer seriously. In view of these facts the Conference has directed attention mainly to encouraging the growth of varieties less liable to be attacked by rust, and also to early sowing. At the March meeting it was recommended that a practical system for the production and distribution of rust-resisting wheats suitable to different districts should be immediately established, and that this system should, subject to modifications needed by each colony, be conducted on the following lines:—A central station for each colony for the preliminary testing of new wheats introduced into the colony; for the production of new varieties by cross-fertilization and by selection; and for the distribution of suitable wheats thus obtained to representative districts of the colony, to be there subjected to a sufficient test, and, if necessary, fixed in their characters by farmers and others competent for the work; and that such wheats as pass satisfactorily this test should then be distributed to the farmers around in such a manner and by such agency as would be most suitable to the conditions of each colony. A committee was appointed to take steps for the proper naming of the different varieties of wheat.

THE U.S. Department of Agriculture has received information to the effect that *Vedalia cardinalis* has been successfully colonized at the Cape of Good Hope. Last autumn Mr. Thomas A. J. Louw, a special commissioner from the Legislative Assembly of the Cape, went to Washington charged with the task of collecting and taking back from America a supply of the useful little lady-bird mentioned. He was furnished with letters to the California agents of the Department, and took away from that State two parcels of *Vedalia*, one lot being shipped on ice and the other kept open and fed *en route*. Both were alive when he arrived at the Cape, and he writes that the experiment has been so successful that various parts of the colony have been supplied with the insect, which, no doubt, will be as useful in clearing off the Cottony Cushion Scale there as it has been in California and the Hawaiian Islands. Mr. Louw's letter, dated Malmesbury, Cape of Good Hope, March 5, 1892, to the Hon. Edwin Willits, Assistant Secretary of Agriculture, says: "While thanking you again for the kindness displayed towards me, may I request you also to convey to Prof. C. V. Riley my extreme obligations for the service rendered by him to me, and which I assure him will ever be appreciated by me."

DR. HYADES, as we noted last week, was impressed, while in Tierra del Fuego, with the resemblance between the Yahgan and the Botocudos of Brazil. Of the latter people an interesting account appeared lately in the Washington *Evening Star*, and is reproduced in the April number of *Goldthwaite's Geographical Magazine*. The colour of the Botocudos is described as of a light yellowish-brown. When brought into contact with Europeans they manifest not the slightest embarrassment on account of their lack of clothing. From certain seeds and fruits they obtain brilliant dyes, with which they adorn their bodies; and a common custom is to paint the face above the mouth a bright red, the upper half of the body being stained black, and a red stripe encircling the waist. A warrior thus decorated, with lip and ear ornaments, is said to present "a most demoniacal expression." The colours employed are mixed in the upper shell of a turtle, and carried in joints of the bamboo. The arms of the Botocudos consist of the bow and arrow. For calling one another in the forest they have speaking trumpets made of the skin of the tail of the great armadillo. While travelling through the woods, they build for themselves temporary shelters of palm-leaves, sticking the stems into the ground in a half-circle, so that the tips of the fronds arch together and form a sort of roof. When encamping for a considerable time in one place, they construct houses often big enough to hold several families. The fire is placed in the middle of the dwelling, and the beds are made of bark fibre. Gourds are used for drinking purposes and in the preparation of food. The Botocudos have been harshly dealt with by the Portuguese, and are rapidly dying out.

CAPTAIN BOWER, of the Indian Staff Corps, has arrived at Simla from China, after a very remarkable journey across the Tibet tableland. He had with him Dr. Thorold, a sub-surveyor, one Pathan orderly, a Hindustani cook, six caravan drivers, and forty-seven ponies and mules. The Calcutta correspondent of the *Times*, who gives an account of the journey, says that Captain Bower, leaving Leh on June 14, crossed the Lanakma Pass on July 3, avoiding the Tibetan outpost placed further south. Journeying due east, he passed a chain of salt lakes, one of which, called Hor-Ba-Too, is probably the highest lake in the world, being 17,930 feet above the sea. Gradually working to the south-east, the explorer saw to the north a magnificent snowy range, with a lofty peak in longitude 83° and latitude 35°. After many weeks' travel over uplands exceeding 15,000 feet in height, where water was scarce and no inhabitants were to be seen, the party on September 3 reached Gya-Kin-Linchin, on the northern shore of Tengri Nor Lake, in longitude 91° and latitude 31°. This is within a few marches of Lhassa, and two officials from the Devi Jong, or temporal governor of Lhassa, met him here and peremptorily ordered him to go back. But he refused to return, and a compromise was effected, guides and ponies being provided on his agreeing to make a detour to the north in order to reach the frontier of Western China. He reached Chiamdo on December 31, only just succeeding in getting off the tableland before winter set in. He struck Bonvalot's route for a few miles when marching to Chiamdo. The country about this town is very fertile and well wooded. Three thousand of the monks of Chiamdo, who lived in fine monasteries, threatened to attack the party, but were deterred on learning that they carried breechloaders. Captain Bower arrived at Tarchindo, an outpost on the Chinese frontier, on February 10. The distance covered from Lanakma to Tarchindo was over 2000 miles, all of which, save a few miles, has now been explored for the first time. The route for thirteen consecutive days lay over a tableland 17,000 feet high. Captain Bower is engaged in writing a report and completing his maps.

SPLENDID specimens of mica are to be sent to the Chicago Exhibition from Idaho, where the supply is said to be practically inexhaustible. Mica is to be used in the Idaho building as a substitute for glass in the windows. The *Photographic News* suggests that, if the reports as to the quality of the material prove to be accurate, it may become a rival to glass in photographic plate-making.

AT the meeting of the Paris Geographical Society on April 1, Lieutenant Vedel read an interesting paper on the Polynesians, whom he has had constant opportunities of studying during the last seven years. Referring to the Maoris, he said it was impossible not to be struck with the extraordinary resemblance which exists between their myths and those of the ancient Greeks.

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THE Southport Society of Natural Science has issued its first report, from which we learn that the Society, although still very young, is doing good work as a local centre of scientific inquiry. The report includes a presidential address by Dr. H. H. Vernon, on the material and educational utility of natural science.

MR. EDWARD STANFORD has issued "The Hand-book of Jamaica for 1892." This is the twelfth year of publication. The work has been compiled from official and other trustworthy sources by S. P. Musson and T. Laurence Roxburgh. It comprises all necessary historical, statistical, and general information relating to the island.

THE latest instalment of the Transactions of the Royal Society of Victoria (vol. ii. part 2) opens with a paper on the occurrence of the genus Belonostomus in the rolling downs formation (Cretaceous) of Central Queensland, by R. Etheridge, Jun., and A. S. Woodward. There are also papers on the structure of *Ceratella fusca* (Gray), by Prof. W. Baldwin Spencer; additional observations on the Victorian land planarians, by Dr. A. Dendy; and land planarians from Lord Howe Island, by Prof. W. Balwin Spencer. Each of the papers is illustrated.

PROF. S. H. GAGE, of the Cornell University, has reprinted an interesting paper contributed by him to the *American Naturalist* on the life-history of the vermilion-spotted newt (*Diemyctylus viridescens*, Raf.). He has added to it a valuable annotated bibliography.

A REPORT on the geology and mineral resources of the central mineral region of Texas, by T. B. Comstock, was included in the second Annual Report of the Geological Survey of that State, and has now been issued separately. It ought to be of good service to practical men as well as to students of science. The author has a valuable note in which he shows how the prospector, the capitalist, or the property-holder may most advantageously use the report.

AN excellent essay on aboriginal skin-dressing, by Otis T. Mason, has been reprinted from the Report of the U.S. National Museum for 1888-89. It is based on material collected in the Museum, and includes an account of skin-dressing among the Eskimo and the Indians. There are many illustrations.

THE following science lectures will be given at the Royal Victoria Hall during May:—May 3, "Flying Bullets," by C. Vernon Boys; May 10, "Travels in Java and Sumatra," by Wm. Hancock; May 17, "The Wonders of the Rocky Mountains," by Wm. Carruthers; May 24, "Mines and Mining," by Bennett H. Brough; May 31, "The Alps in Winter," by C. T. Dent.

THE results of an investigation, concerning the conditions of silent combination of the hydrogen and oxygen contained in the detonating mixture of these gases obtained by the electrolysis of water, are communicated to the current number of *Liebig's Annalen* by Prof. Victor Meyer and Herr Askenasy. The object of the experiments was to ascertain whether any regular connection existed between the duration of time, during which such a mixture of the two gases is maintained at a temperature at which silent combination slowly proceeds, and the amount of water produced. The main result of the experiments has been to afford a direct negative to this question, the amount of combination under precisely the same conditions of temperature, pressure, and time varying most irregularly. Although this is the case, however, some interesting phenomena have been observed during the course of the experiments. It was found that, when a quantity of the pure dry mixture of two volumes of hydrogen and one volume of oxygen was sealed up in a glass bulb and heated in a bath of the vapour of phosphorus pentasulphide, the temperature of which (518°) is such that the bulb becomes faintly luminous, no explosion occurred, but a small proportion of the gases silently combined, with production of water. Upon immersing the bulb, however, in a bath of boiling stannous chloride (606°), explosive combination instantly occurred. It was surmised, therefore, that the temperature at which explosion occurs lies somewhere between 518° and 606°. But upon modifying the experiment in such a manner that the bulb was open, a slow stream of the gaseous mixture being allowed to pass continuously through, it was found that no explosion ensued under these conditions at the temperature of boiling stannous chloride, although at this temperature the bulb glows with a cherry red heat, and the glass is quite soft. It appears likely, therefore, that the extra pressure of the gases in the closed vessel determines the explosion at a lower temperature. The irregularity in the rate of silent combination would appear to be due to the different condition of the inner surfaces of the vessels containing the gaseous mixture; probably largely owing to the different amount of etching action by the water vapour at these high temperatures. In order to eliminate this disturbing element, the experiments have been repeated with bulbs whose inner surfaces have been equally etched beforehand and with bulbs whose interior walls have been silvered, the results, however, showing in both cases the same irregularity. In connection, however, with the experiments with silvered bulbs, another striking fact has been brought to light. It was found that in these bulbs the silent formation of water occurs at temperatures several hundred degrees lower than in unsilvered glass bulbs. Complete combination had occurred in two hours' time at the temperatures of boiling phosphorus pentasulphide (518°), sulphur (448°), diphenylamine (310°), and naphthalene (218°); 70 per cent. of the mixed gases had combined at the temperature of boiling aniline (183°), and a small amount of combination had occurred even at 155°. Finally, it was found that bright July sunshine is incapable of inducing the combination of hydrogen and oxygen, even when it is concentrated upon a bulb traversed by the gaseous mixture and heated to 606° in a bath of boiling stannous chloride.

THE additions to the Zoological Society's Gardens during the past week include a Patas Monkey (*Cercopithecus patas*) from West Africa, presented by Mr. W. S. Hewby; two Orinoco Geese (*Chenalopex jubata*) from South America, presented by Mr. Everard F. im Thurn, C.M.Z.S.; two Mute Swans (*Cygnus olor*), European, presented by Mrs. Melville; a Herring Gull (*Larus argentatus*), British, presented by Miss Lota Bower; two Chinese White-eyes (*Zosterops simplex*) from China; an Egyptian Goose (*Chenalopex ægyptiaca*) from Africa, deposited; a Cheer Pheasant (*Phasianus wallichii* ♂) from Northern India, a Swinhoe's Pheasant (*Euplocamus swinhoii* ♀) from Formosa, a Common Pheasant (*Phasianus colchicus* ♀), British, six Wigeon (*Mareca penelope*, females), twelve Common Teal (*Querquedula crecca* 6 ♂ 6 ♀), European, purchased; a Crested Porcupine (*Hystrix cristata*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

SPECTRUM OF NOVA AURIGÆ.—On February 22, Mr. E. W. Maunder obtained a photograph of the spectrum of Nova Aurigæ with an exposure of seventy minutes. The photographic magnitude of the star was then 4·78, and its visual magnitude was about 5·7. Bright lines were observed upon the plate at the following wave-lengths:—4919, 4860 (F), 4629, 4580, 4547, 4510, 4472, 4340 (G), 4229, 4174, 4101 (h), 3968 (H), 3933 (K), 3887·5 (α), 3834 (β). And dark lines had their positions located as follows:—4316 (G), 4212, 4155, 4085 (h), 3953 (H), 3913 (K). Measures of the displacement

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of the dark lines relatively to the corresponding bright ones gave a mean of 18·3 tenth-metres. According to this, the relative motion of the two bodies engaged was about 820 miles a second. Mr. Maunder also observed the visual spectrum of the Nova. Three bright lines were seen, and estimated to be in the positions of C, D, and F of the solar spectrum. A line was detected "not far from E," another "near *b*, but further towards the blue," and another "very near the chief nebular line." The line measured on the photograph as at λ 4919 was also made out.

PHOTOGRAPHS OF THE REGION OF NOVA CYGNI.—At the March meeting of the Royal Astronomical Society, Mr. Roberts stated the results of a comparison of Drs. Copeland and Lohse's catalogue and chart of the region of Nova Cygni with two photographs of the same part of the heavens taken in September 1891. It appears that the brightness of some of the stars has undergone changes since 1878, when the chart was made. Changes of this character may, of course, be due to the well-known difference between visual and photographic magnitudes; but there are other differences, which are not so easily explained. Several stars, single on the chart, are seen to be double on the photographs, and some changes in relative position seem to have occurred. Although the Nova is not given on the chart, it appears on the photographs as a star of about magnitude 13. It will be interesting to compare Mr. Roberts's pictures with others taken under similar conditions at some future date, in order to determine definitely whether the changes are real, or due to errors in observation or cataloguing.

WINNECKE'S COMET.—Dr. G. F. Haerdtl gives the following ephemeris in *Astronomische Nachrichten*, No. 3083:—

1892.	R.A.	Decl.	Brightness.
	h. m. s.	° ′ ″	
April 29 ...	11 36 4·49 ...	+43 55 41·3	
,, 30 ...	34 22·64 ...	44 2 13·6	
May 1 ...	32 42·41 ...	8 8·3 ...	2·38
,, 2 ...	31 3·89 ...	13 25·6	
,, 3 ...	29 27·21 ...	18 6·3	
,, 4 ...	27 52·46 ...	22 11·9	
,, 5 ...	26 19·62 ...	25 43·4 ...	2·71
,, 6 ...	24 48·66 ...	28 42·1	
,, 7 ...	23 10·55 ...	31 8·6	
,, 8 ...	21 52·27 ...	33 3·6	
,, 9 ...	20 26·81 ...	34 28·1 ...	3·10
,, 10 ...	19 3·16 ...	35 23·3	
,, 11 ...	17 41·31 ...	35 50·0	
,, 12 ...	16 21·18 ...	35 49·6	
,, 13 ...	15 2·62 ...	35 22·5 ...	3·58
,, 14 ...	13 45·59 ...	34 29·4	
,, 15 ...	12 29·98 ...	33 11·2	
,, 16 ...	11 15·72 ...	31 28·7	
,, 17 ...	10 2·63 ...	29 22·9 ...	4·17
,, 18 ...	8 50·51 ...	26 55·0	
,, 19 ...	7 39·29 ...	24 5·2	
,, 20 ...	6 28·92 ...	20 53·7	
,, 21 ...	5 10·08 ...	17 21·4 ...	4·91
,, 22 ...	4 9·48 ...	13 29·0	
,, 23 ...	2 59·99 ...	9 17·4	
,, 24 ...	1 50·50 ...	4 47·8	
,, 25 ...	11 0 40·59 ...	44 0 0·3 ...	5·84
,, 26 ...	10 59 29·91 ...	43 54 55·3	
,, 27 ...	58 18·15 ...	49 33·4	
,, 28 ...	57 5·05 ...	43 55·1	
,, 29 ...	55 50·13 ...	38 0·7 ...	7·04
,, 30 ...	54 33·03 ...	31 50·9	
,, 31 ...	53 13·21 ...	25 25·2	

PERSONAL EQUATIONS IN TRANSIT OBSERVATIONS.—An accurate determination of an observer's personal equation is to-day of as much importance as an observation itself, when such small quantities, as we now deal with, have to be measured. The variation in the latitude, of which we have heard so much of late, amounts to a quantity only a few times larger than that of a moderate personal equation, showing that no small regard must be paid to its estimation. In observing an N.P.D., the star has to be bisected by the horizontal wire, while the nadir point has also to be observed: in both these cases an error can arise from personality, for the best observers cannot make a really true bisection. In the taking of transits another personality exists, but this is rather of a different kind, for, using the eye and ear method, the clock beats have to be taken into account simultaneously with the relative positions of the star and certain wires. In the April number of the *Bulletin Astronomique*, an account is given of some experimental researches on such transit determinations in which both methods, the eye and ear and the chronograph, were used. The observations were made with an apparatus similar to that designed by Wolf, who, to obtain artificial transits, employed a small truck to carry the plates, on which punctures of different sizes were made. From 115 observations made with both methods, it was found that equally accurate results were obtained, the eye and ear method, if any, proving a little inferior, while the degree of lighting of the field made no appreciable variation on the personal equation. For planets the electrical method showed that personality varied considerably, according to whether the preceding or following side was observed: the resulting personal equation for the centre of a planet turned out to be −0·046s., while that for a star under the same conditions was +0·023s. It would be interesting to find out whether this occurs when the eye and ear method is employed. The tendency of an observer, adopting the eye and ear method, to choose certain tenths of a second in preference to others, seems to have its analogy in the chronograph method, in the linear measurement from the second impressions. A comparison given here shows that the most favourable tenth is the *zero*, while the *nine* is very considerably left out in the cold. Another very curious fact is that the tenths, one, two, three, four, chosen in the chronograph methods, are all *less* than the corresponding ones in the other methods, while the opposite occurs for the tenths five to nine.

Tenths	0	1	2	3	4	5	6	7	8	9	Total
Eye and Ear . .	157	97	134	134	100	94	82	81	74	57	1000
Chronograph . .	164	79	96	90	94	129	104	94	81	69	1000

THE SIRIUS SYSTEM.—Dr. A. Auwers contributes to the *Astronomische Nachrichten*, Nos. 3084 and 3085, a long discussion with reference to our "Knowledge of the Sirius System." The problem which he undertook was to investigate whether the measures of the companion obtained during the period extending from 1862 to 1890 would satisfy an ellipse with a 49·4 year revolution; to determine the most probable value of the place elements for every measurement on the assumption of the revolution; and to inquire whether the observations of the principal star could be represented by means of the so determined elements. The author divides the discussion into the following three parts:—(1) A summation and sifting of all the measures that have been made of this companion for the above-mentioned period. (2) The derivation of the normal places, and the correction of the elements. (3) A comparison of the meridian observations of Sirius with the elements derived from the measurements of the companion. The result of the discussion is that a slight correction is necessary to reduce the right ascension and declination of the bright star to the centre of gravity of the two bodies (the masses of the chief star and of the companion being taken as 2·20⊙ and 1·04⊙). The table showing these corrections indicates that the right ascension between the years 1850 and 1890 has to be increased by a quantity which reaches to 0·232s., while between 1890 and 1896·5 a diminution takes place. The greatest correction for the declination is + 2″·268, which occurs in 1882·0, and this correction becomes negative also about 1893·5.

THE ANCIENT CIVILIZATION OF CENTRAL AMERICA.

IN Central America there are abundant traces of the existence of a great race which must at one time have attained to a comparatively high state of culture. It was undoubtedly a race of American Indians, and as undoubtedly closely connected with the present Indian inhabitants of the country.

No trace, however, of the ancient culture and knowledge can be found amongst the Indians of to-day, and the numerous ruins which lie scattered over the country are the remains of towns which have neither names nor history attached to them.

Very little information can be gathered from the published writings of the Spaniards who overran the country at the close of the fifteenth and beginning of the sixteenth centuries; but, apart from their bearings on Spanish history and biography, these writings have received very imperfect examination and criticism.

The Spaniards have been severely censured for their remiss-

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ness in omitting to record the wonders of the Indian civilization which they are supposed to have met with, and especially for having failed to tell us about the towns and highly decorated buildings the ruins of which have been frequently described by modern travellers; but this censure appears to be to a great extent unmerited, for their writings, if carefully searched, do reveal a considerable amount of information about the Indians as they found them, and they failed to describe the ancient buildings because, as I hope to prove later, in many cases these buildings were even then as deeply buried in the recesses of the forests as they are at the present day.

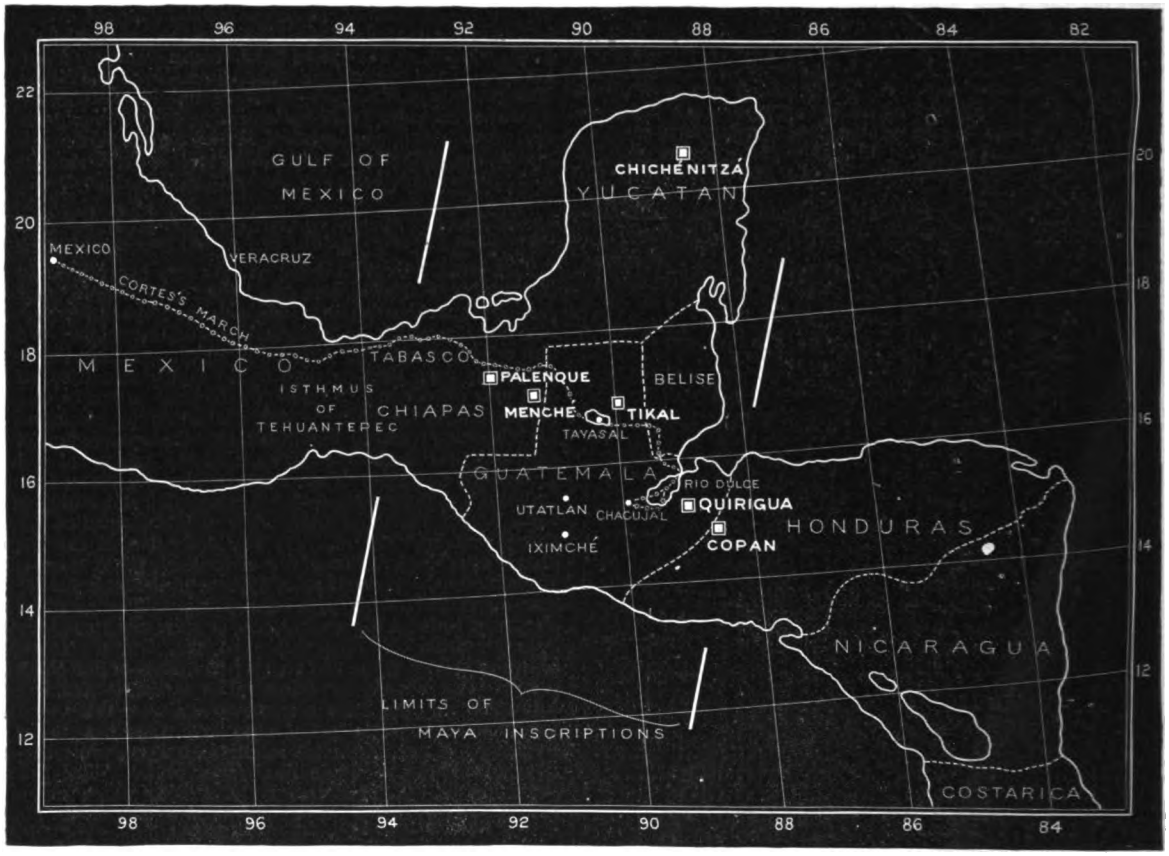
We naturally want to know more about this lost civilization, and there are many ways of attacking the problem. First of all, there is a large amount of correspondence, and a great number of reports written by the soldiers, officials, ecclesiastics, and other early settlers in the New World, which, not bearing on the main historical events of the conquest, have escaped publication, but which, if carefully examined, may afford valuable information regarding the native Indians. Then a study of the customs, languages, and folk-lore of the living races, may throw much light upon the condition and belief of their forerunners; and, above all, a careful examination of the burial-places and of the architectural and monumental remains, and their numerous hieroglyphic inscriptions, which lie hidden away in the vast forests, may reveal something of the history of the people who raised them.

It has been my good fortune to be able to devote my time during seven winters to the collection of materials which I trust may enable the study of Central American archæology to be pursued with greater ease and success than has hitherto been possible.

All the moulds of inscriptions and other sculptures made during my expeditions have been handed over by me to the authorities of the South Kensington Museum, and casts have already been taken from the greater number of them, which, as well as some original stone carvings, are now exhibited in the Architectural Court of the Museum. Maps, plans, photographs, and drawings, are in course of publication in the archæologi a section of the "Biologia Centrali-Americana."

The Editor of this journal has asked me to give some general account of the work on which I have been engaged, and its results, and this I will now attempt to do; but I must ask the reader to bear in mind that I started on the work almost by chance, and without any previous training or archæological knowledge, that I am but little acquainted with the literature of the subject, and have almost entirely confined my efforts to the collection of accurate copies of sculptures and inscriptions, in hope that some students may be found willing to make use of them. The following notes must therefore not be looked on as anything more than an attempt to clear the ground before an attack which I hope some day to see made on a difficult problem.

The remains of the more civilized races of North America



can be traced from the Isthmus of Panama as far north as the ruined Pueblos in the Cañons of Colorado. This great extent of country can again be roughly divided into three portions—one extending from Colorado to the Isthmus of Tehuantepec, a second from Tehuantepec to a line running nearly along the western frontiers of Honduras and Salvador, which may be called the Maya district, and a third from this line to the Isthmus of Panama.

So far as I know, no remains of stone buildings have been found in this last district, but much pottery is found—some of which is distinguished by great beauty of form, as well as excellence of decoration.

It is in the centre province, which includes Guatemala, Chiapas, Tabasco, and Yucatan, that my collections have been made, and the accompanying map shows the most important ruins visited.

It is impossible within the compass of an article, and

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without the aid of numerous plans and drawings, to give an adequate account of the ruins as they can now be seen, but the following short summary gives the characteristics of the principal groups found to the south of Yucatan :—

Quirigua.—Thirteen monoliths covered with elaborate carved decoration and inscriptions. These are of two classes, upright stelæ, of which six still stand erect, the tallest measuring 25 feet high from the ground, 5 feet across back and front, and 4 feet across the sides, and large rounded blocks of stone cut into the form of some grotesque animal, the largest of them weighing about 20 tons. No buildings remain standing, but there are numerous mounds, only one of which has been dug into, and was found to contain on its summit the ground-work of a stone-built temple.

Copan.—Sixteen stelæ averaging 12 feet in height, of which eight are now standing, and numerous other sculptured monuments. Both figures and inscriptions are carved in higher relief than at Quirigua. Numerous stone-faced mounds, which can be ascended by well-laid stone stairways. There is no sign of a wall either of house or temple above ground, but the lower parts of both temples and houses can be found by digging into the masses of broken masonry on the tops of the mounds and terraces. Broken stone ornaments, which once decorated these buildings, are found lying in profusion at the foot of the mounds.

Menché.—A town built on stone-faced terraces rising one above the other from the banks of the River Usumacinta. There are many mounds of stone, and there are a few stone-roofed houses and temples still standing with carved stone lintels over the doorways. No separate carved monolithic monuments of importance.

Tikal.—Five cell-like temples with enormously thick walls, raised on pyramidal foundations of great height. The measurement of the largest, from the ground to the top of the temple, is about 160 ft., the base of the foundation measuring about 280 ft. square. These temples had beautifully carved wooden lintels over the doorways. Some of this carved wood is now preserved in the Museum at Basle, and some (a few smaller pieces) in the British Museum. There are several other smaller temples and numerous houses with stone roofs still standing. All these buildings had wooden lintels over the doorways, and some of the wooden beams are in a perfect state of preservation. There are seven or eight small stelæ, usually flat slabs of stone with carving on the front and sides only, all unfortunately much damaged and weather-worn.

Palenque.—One group of stone-roofed houses, commonly known as the palace, raised on a high stone-faced foundation. Four separate temples on similar foundations, and numerous other temples, houses, and tombs, some half-ruined, and others mere heaps of stone and rubbish. Only one carved monolith has been found which stood apart, but several large stone slabs beautifully carved with figures and inscriptions in low relief were let into the interior walls of the temples, and almost all the buildings have been lavishly ornamented with figures and inscriptions moulded in a hard and durable stucco.

The principal fact ascertained from the examination of the remains throughout this district (including Yucatan) is that the art as exemplified both by monuments and buildings is one and the same, and that the inscriptions are all carved in the same characters.

The chief difference to be noted is that whereas in the ruins which I assume to be of earlier date the art and workmanship is lavished on the decoration of large monoliths, whilst the temples and other buildings are comparatively insignificant, as time went on the elaborate carving of separate stone monuments was neglected, and the whole efforts of the artists were devoted to the erection and adornment of larger and more imposing buildings, and the carved stone glyphs of the monoliths gradually gave way to stucco and painted inscriptions on the walls of the temples and to manuscript books.

The age to be ascribed to these remains is purely a matter of conjecture ; but there are some historical facts which bear on the subject which I have already called attention to in another publication, but which may with advantage be here repeated.

Hernando Cortes, after the conquest of Mexico, started from that city in the year 1525, accompanied by some hundreds of Spaniards and a large number of Indians, with the intention of marching direct to Honduras. When Señor Don Pascual de Gayangos, in the year 1867, translated for publication by the Hakluyt Society the letter written by Cortes to Philip II. of Spain, giving an account of this expedition, he states in the preface that :—"To determine the spots visited by him in this extraordinary march through almost impenetrable forests, swampy plains, or lofty mountains, has by some writers been pronounced a hopeless task ; and though we possess the narrative of the stout-hearted and sturdy soldier, Bernal Diaz, who formed part of the expedition and carefully noted down its principal events ; though the various provinces traversed by the devoted army have since been more or less explored by travellers of all nations, few are the indications—and those very slight—of the route they followed. He must have passed near the ruins of Palenque, since the small village of Las Tres Cruzes is said to derive its name from three wooden crosses left in that locality."

A comparison of the recent and more accurate maps of Tabasco published by the Mexican Government, and of my own surveys in the region of the head waters of the Sarstoon and Mopan Rivers, with certain old maps and documents which have recently been brought to light from the Archives of the Indies at Seville, now enables us to trace Cortes's line of march with some degree of accuracy.

After passing the Isthmus of Tehuantepec, he found himself involved in the intricate waterways of the delta of the Tabasco and Grijalva Rivers. He and his followers suffered the greatest hardships, but after cutting their way through the tangled vegetation of the swamps, and with infinite patience and labour building bridges over the almost innumerable streams and lagoons, he crossed the River Usumacinta, somewhere in the neighbourhood of Tenosique.

There can be no doubt that towards the end of this part of this march, at a time when Cortes and his followers, lost in the forests of the delta, were suffering the last extremities of hunger, and were eagerly searching for a track which might lead them to an Indian settlement—they were traversing a plain actually overlooked by the temples of Palenque, and not more, and probably much less, than twenty miles distant from them. If Palenque had then been the great centre which it at one time must have been, and if the foot-hills of the Sierra on which it stands had then been as thickly peopled as the numerous remains indicate, it would have been impossible for a body of men as numerous and as much on the alert as were the followers of Cortes, to have missed the discovery of the many tracks which must have led thither.

Moreover, Cortes had been furnished with a map of the country, prepared by the Indian chiefs at Guacacualco ; and although it has been suggested that the chiefs systematically deceived him so as to prevent his visiting their richest and most sacred towns, such deception was not likely to have been successful with him, and it is still less likely to have imposed upon the large number of Mexican Indians who accompanied him. Yet, if Palenque was then inhabited, we are compelled either to believe that Cortes and his followers were indeed successfully imposed upon, or to give credit to the still more unlikely alternative that the Indian auxiliaries preferred to suffer such extremities of hunger that they were driven to eat the bodies of their companions who had died by the way, rather than give any information which would have been of service to their foreign leaders.

It hardly appears possible, therefore, to resist the conclusion that, in the year 1525, Palenque was already abandoned, and lost in the forest.

But if the information afforded to Cortes is to be relied on, then the same fate must also have overtaken the town of Menché on the Usumacinta, for Cortes was strongly advised by the natives not to continue his march along the banks of the river (and if he had done so he must have passed near the site of the ruins of Menché), as the country in that direction was uninhabited.

Accepting this advice, Cortes took the road by Acalá and Peten, and thence through part of what is now British Honduras, to the mouth of the Rio Dulce.

The inhabitants of Acalá appear to have been more civilized than any others whom Cortes met with during his long march. He states that the country was thickly peopled, and that the towns were large and full of mosques or idol-houses, yet no important ruins have ever been found in that district, and neither Cortes nor Bernal Diaz gives us any description which would lead us to suppose that they ever met with such imposing buildings as those still standing at Palenque or Menché.

From Acalá the expedition marched through a very thinly-peopled country until they arrived at the Lake of Peten.

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Cortes visited the town of Tayasal, built on a small island in the lake, which, we are told, was the chief town of the district, and which was doubtless then, as it was later, the stronghold of the warlike Itzaes. Now, fortunately, we know something of the subsequent history of this town, for Tayasal was visited by missionaries from Yucatan in 1618, 1619, and 1623. This last missionary expedition ended disastrously, as the missionary and his followers were murdered by the natives; and we then have but scanty information about the Itzaes until the country was invaded by the Spaniards from Yucatan, and Tayasal captured in 1697. A curious story shows us that Tayasal is not likely to have suffered any serious disturbance between Cortes's visit and the year 1618.

In his letter to the King he states that, "At this village, or, rather, at the plantations that were close to the lake, I was obliged to leave one of my horses, owing to his having got a splinter in his foot. The Chief promised to take care of the animal and cure him, but I do not know if he will succeed, or what he will do with him."

On the day after the arrival of the missionary fathers Fuensalida and Orbita, in 1618, the Chief of the Itzaes showed them round the town, "in the middle of which, on the rising ground, were numerous and large buildings, 'cues' or oratories of their devilish and false gods. Entering into one of them, they saw in the centre of it a large idol in the form of a horse, well modelled in stone and plaster. It was seated on the ground, on its haunches.

"These barbarians reverenced it as the God of Thunder, and called it Tzimindiac, which means 'the horse of thunder and lightning.'"

This sight was too much for the religious zeal of Padre Orbita, who, seizing a great stone, jumped on to the idol and hammered it to pieces. It is hardly necessary to add that the Chief had the greatest difficulty in saving the lives of the missionaries from his infuriated people, and that they were compelled to leave the island at once.

It was afterwards learnt from the natives that they had thought the horse to be the god of thunder and lightning because they had seen the Spaniards firing their guns from horseback, and that when they found the horse to be ill, "they gave it to eat fowls and other meat, and presented it with garlands of flowers, as it was their custom to do when their own chiefs were ailing," and that, on its death, a council of chieftains was called, and it resolved to make an image of the horse in stone.

In the year 1700, the historian Villagutieres published a detailed account of the conquest of Itza by the Spaniards, and a description of the town of Tayasal, stating that "it was full of houses, some with stone walls more than a yard high, and, above these, wooden beams and roofs of thatch, and others of wood and thatch only"; and "of the twenty-one oratories which General Ursua found in the island, the principal and largest was that of the high priest Quincanek, cousin of the king Canek; this was rectangular (*cuadrada*), with a beautiful breastwork (*pretil*) and nine handsome steps, and each front was about twenty yards long and very high."

Speaking from memory, I should say that the island is not more than 500 yards across, and there are no signs whatever at the present time of any ancient foundations. It is now covered with poorly-built adobe houses, and in the centre is a church, which probably occupies the site of the ancient 'cues.' Now, within a day's walk from the north shore of the lake are the very remarkable ruins of Tikal, of which a short description has already been given; yet nothing whatever is told us either by Cortes, by the missionaries, or by Villagutieres, of the existence of a town on this site, and the ruins were unknown to the Spaniards until the year 1848.

The missionaries, on their journeys from the Spanish outpost at Tipu to Tayasal must have passed within a few miles of the site of the ruins; and it is impossible to believe that, so long as Tikal was inhabited, Tayasal could have been the chief town of the district, or, indeed, that Tikal could have been inhabited at all without the fact coming to the knowledge of the Spaniards.

If any further evidence were needed to show that the great structures raised during the epoch of higher civilization had already been deserted at the time of the Spanish conquest, it can be found in what Cortes himself states with regard to the town in Guatemala which he calls Chacujal.

When, after having crossed the base of the peninsula of Yucatan, the starving army arrived at the mouth of the Rio Dulce, it was only to find the Spanish colony it had come in search of reduced to a similar extremity of famine.

The scanty Indian population in the neighbourhood had been rendered hostile by the exactions of the settlers, and it was immediately necessary to scour the country for long distances in search of food. The most important of these raids, and, indeed, the only successful one, was led by Cortes himself, who landed on the south side of the Golfo Dulce, and marched about two leagues inland (when he must have been within about twelve to fifteen miles of the site of the ruins of Quirigua), and then turned along the mountain-range to the south of the Rio Polochic, and finally succeeded in reaching Chacujal, which is situated between two small streams which run into the Polochic. The inhabitants had all fled, but Cortes was fortunate in finding a large store of Indian corn, and other food.

Cortes writes of the town as follows:—"Marching through the place we arrived at the great square, where they had their mosques and houses of worship, and as we saw the mosques and buildings round them just in the manner and form of those at Culua" (on the coast of Mexico), "we were more overawed and astonished than we had been hitherto, since nowhere since we had left Acalá had we seen such signs of policy and power. . . . On the following morning I sent out several parties of men to explore the village, which was well designed, the houses well built, and close to each other." I can find no record whatever of Chacujal subsequent to the date of Cortes's visit; but in 1884 I myself visited the ruins of the town, guided by Cortes's own description of the site. The ruins are now completely buried in the forest, but there was little difficulty in tracing the general plan of the town, and making out the foundations of the principal buildings.

It is easy to understand how Cortes may have been favourably impressed with the flourishing appearance of the place after his terrible and tedious journey through the forest, yet it is quite clear from the ruins that the structures themselves could never have been of any considerable importance. The walls of the principal buildings had only been built of stone to half their height, and the superstructure and roof must have been made of some perishable material—a great contrast to the thick stone walls and heavy stone roofs at Palenque, Tikal, Menché, and Copan. Another point of importance is that the plan and method of construction of the buildings at Chacujal is similar to that of the ruins on the hill-tops a little further inland near San Jeronimo, Rabinal, and Cubulco, some of which I have visited. These were undoubtedly the strongholds of those Indians of the Tierra de Guerra to whom no high culture has ever been attributed, and who were induced by the Padre Las Casas to leave their fastnesses and settle in the plain of Rabinal in the year 1537.

It can therefore now be stated without doubt that, although Cortes and his followers on his march from Mexico to Honduras passed within a short distance of several of the sites of the most important ruins in Central America, they heard nothing of their existence as living cities.

Let us now consider the case of the often-described ruins of Copan on the northern frontier of Honduras.

The earliest information dates back to the year 1576, when the ruins were visited by Palacio del Rio, who described them in a letter written to King Philip II. of Spain. After giving an account of the sculptured monoliths, he mentions the numerous mounds which could be ascended by stone stairways, but he says nothing whatever about houses or temples, which such a careful observer as Palacio could not have omitted to mention had they then been in existence. He further states in his letter that it was impossible to believe that the scanty Indian population of the districts could have raised such monuments as he found at Copan, and that his efforts to elicit information from the leaders of the Indians dwelling in the neighbourhood only showed that all knowledge of the people who had raised these monuments was lost in the mists of tradition.

Enough has now been said to show that the most important ruins in the whole of this Maya district (outside of Yucatan) were never known to the Spaniards as the sites of inhabited towns, and it now remains to say only a few more words about those towns in which the conquerors actually found the people dwelling. The descriptions already quoted from early writers, or given from my own observations of the ruins in the cases of Tayasal and Chacujal, give some idea of what these towns were like; and the correctness of these descriptions is strengthened by the results of a careful examination which I have made of the sites

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of the towns of Utatlan and Iximché, the capitals of the Quichés and Cachiquels, who were the most powerful tribes in Guatemala when Alvarado conquered the country. Although the remains of these towns, which were known for certain to have been inhabited at the time of the Spanish conquest, bear some similarity in plan and arrangement to the older ruins, there is the great distinction to be observed that in no instance is there any indication of the former existence of stone-roofed buildings, that there are only a few stones which show any trace of ornamental carving, and that of the roughest description, and that there are no remains of any carved inscriptions.

It may be as well to say a word of warning against the exaggerated accounts of the magnificence of the Indian towns of Guatemala at the time of the conquest which have found their way into the histories of the country by Fuentes, Juarros, and others, and are still alluded to and sometimes accepted as facts by modern travellers. To give only one instance. In describing the palace of the Quiché kings at Utatlan, dimensions are given for this palace which exceed the whole extent of the land on which any building is possible, for the site of the town is most clearly defined, and limited by the great "barranca" or rift, some hundreds of feet deep, which almost encircles it. It was no doubt this peculiar situation, that of an almost inaccessible peninsula in the middle of an undulating plain, which gave the site so much value in the eyes of the Quichés.

There is, then, a clearly marked difference between the remains of the towns of which we have some historical knowledge and the more ancient ruins.

But when one considers the fair state of preservation of some of the buildings at Palenque and Menché, and the presence of sound wooden beams in the temples and houses at Tikal, it is hardly possible to ascribe even to these ruins any very great antiquity.

From my own observation of the state of the ruins themselves, and the style of art displayed in the carved ornaments and inscriptions, I should feel inclined to give to Quirigua the earliest date, Copan the next, then Menché, Palenque, and Tikal, in the order named.

We must now turn our attention to the province of Yucatan.

The central portion of the peninsula has always been more or less a *terra incognita*. The Spaniards never really brought its inhabitants into complete subjection, and to this day it is peopled by hostile Indians, and no Spaniard dares to enter it.

If this country contains traces of the old civilization, nothing definite is known of them. The northern portion of the peninsula was brought completely under Spanish control, and is known to be studded with the remains of groups of ancient stone buildings.

It was on the north-east coast of Yucatan that the Spaniards first came into contact with Indians who used stone as a building material, and there can be but little doubt that some of the many ruined structures now to be seen were inhabited by the natives at the time of the conquest.

I am myself inclined to the opinion that the north of Yucatan was the last stronghold of the more cultivated branch of the Maya race after that race had either been driven out of, or under the stress of unknown adverse circumstances had retrograded in, the country to the south. But it does not follow that the Indians of Yucatan were at the height of their power and prosperity when the Spaniards came amongst them. In fact, their conquerors learnt from them that for some time previously the country had been troubled with civil wars and dissensions, and that Mayapan, once the chief town, had been destroyed and abandoned. It seems quite probable that this statement may be enlarged on to a considerable extent, and that we may consider the country to have been in a state of decadence, and that not one but many of the chief centres of population had been more or less abandoned. However, the temples and sacred edifices appear still to have been held in reverence after the population had moved away, and were visited during festivals, and may have been kept in some sort of repair by the priests; much in the same way as I believe the ruined dagobas and temples at Pollonarua and Anuradhapura are reverenced and visited by the people of Ceylon.

This appears to me to have been most probably the case with regard to the important buildings which still mark the site of what must have once been the large town of Chichén Itzá.

It has, I know, been stated that Chichén was inhabited at the time of Francisco de Montejo's first abortive effort to conquer Yucatan, and that the Spaniards were for some considerable time encamped in the town; but this statement does not appear to me to be supported by any sufficient evidence. Nevertheless, religious ceremonies had been so recently observed in Chichén Itza, that, in answer to a despatch from Spain, a committee of the settlers in the neighbouring town of Valladolid were able to give some account of them in the year 1579.

My personal experience of the ruins in Yucatan is limited to a hasty visit to Labna and Uxmal, and a residence of five months in one of the ruined temples of Chichén Itzá. At Chichén my clearings and surveys extended over an area of nearly a mile square, and although this appeared to include all the principal edifices, it was impossible to walk into the bush in any direction from the edge of this area without coming on the traces of stone buildings.

The surface of the ground, even in the centre of the town, although generally level, was in some places composed of cavernous and broken limestone rock, and these portions had apparently been walled off as unfit for buildings. But, wherever the ground was suitable, there were numerous traces of slightly constructed buildings in addition to the more solid structures.

The hieroglyphic inscriptions at Chichén are few in number, and with one small exception very poorly carved, but there is enough to show that they did not differ in character from those in Guatemala and Chiapas. There is, however, one great distinction between the sculptures in Yucatan and the country to the south which must not be overlooked. In the latter there is an almost entire absence of weapons of war, and the figures of women occupy a prominent position. In Yucatan the change is complete: there are no women represented in the sculptures, and every man is a warrior armed with spears and throwing-stick.

Whether the Maya civilization extended to Yucatan during the time that it flourished at Copan or Palenque, it is at present impossible to determine; but I strongly incline to the opinion that all the buildings now standing in Yucatan are of a later date. It may be perhaps allowable to state the case somewhat as follows:—

That the civilized portion of the Maya race have at some time occupied all the country lying between the Isthmus of Tehuantepec and the western frontiers of Honduras and Salvador (excepting perhaps a strip of country along the Pacific sea-board); that this people spoke the same or nearly allied languages, which they wrote or carved in the same script; that they were followers of the same religion, and built stone-roofed temples and houses decorated with the same class of design and ornament.

That at the time of the Spanish conquest they had entirely abandoned all their towns and religious centres in the country to the south of Yucatan, although the good state of preservation of many of the buildings at the present time precludes the idea that this desertion of their towns could have ante-dated the arrival of the Spaniards by very many years. That the people whom the Spaniards encountered in this part of the country, although they may have been allied in blood to the Mayas, were undoubtedly in a lower state of culture, and that an examination of the sites of their principal towns yields no signs of the artistic culture which is universally found in the older ruins.

That in Yucatan, where the Spaniards found a dense population of Maya Indians, and encountered a fierce and stubborn resistance, there are still to be seen numerous remains of ancient buildings, both larger and in better preservation than those in Guatemala and Chiapas, but built in the same manner, decorated with the same ornaments, and with inscriptions carved in the same hieroglyphic script. That there is evidence, from the early Spanish writings, that some at least of those buildings were still occupied at the time of the conquest; but that both the observations of the Spaniards themselves, as well as the reports subsequently gathered by them from the Indians, point to the conclusion that the country was in a state of decadence, and that many of the larger centres of population had already been abandoned, although the more important religious edifices may still have been reverenced and kept in repair.

The early Spanish writers make frequent allusion to the large number of books written and preserved by the natives of Yucatan. These books were written in hieroglyphic characters in the Maya language, which, it must be remembered, is still spoken by the whole of the Indian population of Yucatan, as well as by nearly all the half-breeds and Spaniards.

Unfortunately, every effort was made by the Spanish priests to destroy this literature, which they looked on as the work of the devil; and it is very doubtful whether a single fragment of hieroglyphic manuscript is now in existence in the whole peninsula.

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One of the chief of the iconoclasts was Archbishop Diego de Landa; but, luckily, his zeal was tempered by a considerable appreciation of the ingenuity of the Indians, and an interest in their manners and customs, which induced him to make some notes on their method of writing and recording events.

It is to this that we owe what is commonly known as "Landa's alphabet"; but, as this was an attempt to make an alphabet of a language which in all probability was not written alphabetically but syllabically, it was a signal failure, and has proved, to the few scholars who have attempted to employ it, about as puzzling as the hieroglyphics themselves. However, it may ultimately be of some use, and it was accompanied by an explanation of the calendar system, and a list of the signs for the days and months, with their names, which is of the greatest value.

Although no Maya books are known to exist in America, three examples of what are undoubtedly genuine Maya manuscripts have turned up in Europe.

No information whatever is forthcoming as to how they got here, but it is not unlikely that they were sent over as curiosities at the time of the Spanish conquest, and were afterwards lost sight of.

They are the "Codex Troano," now preserved in the Archæological Museum at Madrid, a chromolithographic copy of which was published by the Abbé Brasseur de Bourbourg; the "Dresden Codex," preserved in the Royal Library at Dresden, of which a beautiful photolithographic copy has been published under the direction of Prof. Förstemann; and the "Codex Peresianus," in the Bibliothèque Nationale at Paris. Another manuscript at Madrid, which has been called the "Codex Cortesianus," appears to be only a detached portion of the "Codex Troano."

An examination which I have made of the two first-mentioned Codices leaves no doubt on my mind about the similarity of the written to the carved inscriptions. Many of the glyphs are identical, and others only vary as much as might be expected by the change from carving on stone to writing on paper. In addition to this evidence of the eyes, there is the distinct statement of Cogolludo, the historian of Yucatan, that the Indians had "characters by which they could understand one another in writing, such as those yet seen in great numbers on the ruins of their buildings."

So that we arrive at the important conclusion that the language of the carved inscriptions of Copan, Quirigua, and Palenque is still a living tongue, although it has doubtless been much changed in the course of years.

ALFRED P. MAUDSLAY.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Mr. C. E. Ashford, B.A. of Trinity College, has been appointed Assistant Demonstrator of Physics in the Cavendish Laboratory.

Dr. William Ewart and Mr. Frederick Treves have been appointed additional examiners in Medicine and Surgery respectively.

The Cavendish Professor announces a course of lectures on Electrolysis and Solution, to be given by Mr. W. C. D. Whetham on Thursdays and Saturdays during the present term.

Seventeen candidates were approved for the diploma in Public Health at the extra examination held at the beginning of the month.

T. Clifford Allbutt, M.D., F.R.S., the newly appointed Regius Professor of Physic, has been elected to a Fellowship at Gonville and Caius College.

The Shuttleworth Scholarship in Botany has been awarded to J. H. Burkill, B.A., Assistant Curator of the Herbarium.

The memorial in Westminster Abbey to the late Prof. J. C. Adams, will be placed in the sill of the window on the north side, nearest to the monument of Newton. A large and very influential committee has been formed for the purpose of establishing the memorial.

SCIENTIFIC SERIALS.

American Journal of Science, March.—Mount St. Elias and its glaciers, by Israel C. Russell. Account is given of the country explored by two parties sent to Alaska by the National Geographic Survey, in connection with the U.S. Geological Survey, in 1890 and 1891.—Hudson River "Fiord," by Dr. Arthur M. Edwards.—Contributions to mineralogy, No. 52, by F. A. Genth; with crystallographic notes by Samuel L. Penfield. The minerals described are hübnerite, hessite, bismutite, and natrolite.—Tschermak's theory of the chlorite group and its alternative, by F. W. Clarke.—Recent fossils near Boston, by Warren Upham. Fossil marine shells of the post-Glacial epoch have been lately discovered near Boston, indicating slight recent changes in the relative levels of land and sea, and proving considerable changes in the temperature of the sea there.—The highest old shore line on Mackinac Island, by F. B. Taylor.—On the nature of colloid solutions, by C. E. Linebarger. It is generally believed that solutions of colloid substances, such as albumen or silicic acid, differ in their nature from solutions of crystalloid substances. The author's experiments indicate that colloid solutions are solutions in the ordinary acceptation of the term, and not "suspensions."—Observations upon the structural relations of the Upper Huronian, Lower Huronian, and Basement Complex on the north shore of Lake Huron, by Raphael Pumpelly and C. R. Van Hise.—A phasemeter, by John Trowbridge. The phasemeter is an instrument devised for the investigation of questions of the phase of alternating electric currents in transformers and in branch circuits. Two telephone diaphragms have mirrors fixed upon them. A spot of light reflected from one of the mirrors is given a horizontal movement when the diaphragm is vibrating, while the other mirror, when its diaphragm moves, gives a spot of light a vertical motion. By the combination of the two motions, figures are obtained similar to those of Lissajous in the case of tuning-forks; and from these, the difference in phase of the currents which set the diaphragms in motion can be found.—Preliminary report of observations at the Deep Well, Wheeling, West Virginia, by William Hallock.—Mount Bob, Mount Ida, or Snake Hill, by T. W. Harris.

April.—On the action of vacuum discharge streamers upon each other, by Dr. M. I. Pupin. The experiments described show that two electric current filaments in a rarefied gas may repel each other in cases where electrodynamic action would produce an attraction. The repulsion does not appear to be due to electrostatic action, but rather to "a strain in the vacuum produced by the peculiar distribution of the gas pressure resulting from the peculiar distribution of temperature."—On a melilite-bearing rock (Alnoite) from Ste. Anne de Bellevue, near Montreal, Canada, by Frank D. Adams.—On an azure-blue pyroxenic rock from the Middle Gila, New Mexico, by George P. Merrill and R. L. Packard.—On the correlation of moraines with raised beaches of Lake Erie, by Frank Leverett.—Magnesium as a source of light, by Frederick J. Rogers. The results of this investigation are summed up as follow:—(1) The spectrum of burning magnesium approaches much more nearly that of sunlight than does the spectrum of any other artificial illuminant. (2) The temperature of the magnesium flame, about 1340° C., lies between that of the Bunsen burner and that of the air-blast lamp, although the character of its spectrum is such as would correspond to a temperature of nearly 5000° C. were its light due to ordinary incandescence. (3) The "radiant efficiency" is 13½ per cent., a value higher than that for any other artificial illuminant, excepting, perhaps, the light of the electric discharge *in vacuo* for which Dr. Staub, of Zürich, has found an efficiency of about 34 per cent. (4) The radiant energy emitted by burning magnesium is about 4630 calories per gram of the metal burned, or 75 per cent. of the total heat of combustion, as compared with 15 per cent. to 20 per cent. in the case of illuminating gas. (5) The thermal equivalent of one candle-power-minute of magnesium light is about 2·4 lesser calories, as against 3·5 to 4·0 for other artificial illuminants. (6) The total efficiency of the magnesium light is about 10 per cent., as compared with 0·25 per cent. for illuminating gas. (7) Taking into consideration the greater average luminosity of the rays of the visible spectrum of the magnesium flame, it is certain that *per unit of energy expended, the light-giving power of burning magnesium is from fifty to sixty times greater than that of gas.*—A method for the quantitative separation of barium from calcium by the action of amyl alcohol on the nitrates by P. E. Browning.—On plicated cleavage foliation, by T. Nelson Dale.—Geological age of the Saganaga syenite, by A. R. C. Selwyn.—A third occurrence of peridotite in Central New York, by C. H. Smith.—A fulgurite from Waterville, Maine, by W. S. Bayley.—Mineralogical notes on brookite, octahedrite, quartz, and ruby, by G. F. Kunz.—Recent polydactyle horses, by O. C. Marsh.

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SOCIETIES AND ACADEMIES.

LONDON.

Entomological Society, April 13.—Mr. Henry John Elwes, Vice-President, in the chair.—Mr. R. McLachlan, F.R.S., exhibited specimens of a Caddis-fly remarkable for the abbreviated wings of the male, the female having fully developed wings. He alluded to the *Perlidæ* as including species in which the males were frequently semi-apterous. Dr. Sharp, F.R.S., inquired if Mr. McLachlan was aware of any order of insects, except the Neuroptera, in which the organs of flight were less developed in the male than in the female. Mr. C. G. Barrett and Mr. H. J. Elwes cited instances amongst the *Bombycidæ* in which the wings of the male were inferior in size and development to those of the female.—Dr. Sharp exhibited specimens of both sexes of an apparently nondescript Phasmid insect allied to *Orobia*, obtained by Mr. J. J. Lister in the Seychelles Islands, together with *Phyllium gelonus*. He also exhibited specimens of both sexes of an insect remarkable for its great general resemblance to the *Phasmidæ*, though without resemblance, so far as is known, to any particular species. In reference to the *Phyllium*, Dr. Sharp called attention to the fact that the similarity of appearance of parts of their organization to portions of the vegetable kingdom was accompanied by a similarity, amounting almost to identity, of minute structure. He said that it had been stated that the colouring-matter is indistinguishable from chlorophyll, and that Mr. Lister had informed him that when in want of food a specimen of the *Phyllium* would eat portions of the foliaceous expansions of its fellows, although the *Phasmidæ* are phytophagous insects. The resemblance to vegetable products reached its maximum of development in the egg; and M. Henneguy had observed that when sections of the external envelope of the egg of *Phyllium* are placed under the microscope no competent botanist would hesitate to pronounce them to belong to the vegetable kingdom.—Mr. Barrett exhibited, for Major J. N. Still, a specimen of *Notodonta bicolora*, which had been captured in a wood near Exeter. Major Still had stated that the captor of the specimen was unaware of the great rarity of the species. Mr. Barrett also exhibited, for Mr. Sydney Webb, some remarkable varieties of *Argynnis adippe* and *Cœnonympha pamphilus*; also two specimens of *Apatura iris*, and two of *Limenitis sybilla* in which the white bands were entirely absent.—The Hon. Walter Rothschild exhibited, and contributed preliminary notes on, some hundreds of Lepidoptera, representative of a collection of about 5000 specimens recently made by Mr. W. Doherty, in the south-west of Celebes. Many of the species were new, and others very rare. Mr. Elwes, Colonel Swinhoe, and Mr. S. Stevens commented on the interesting nature of this collection.—Mr. E. B. Poulton, F.R.S., gave a lecture "On the Denudation of the Scales in certain Species of Lepidoptera," and illustrated it by a large number of photographs shown by means of the oxy-hydrogen lantern. Mr. G. F. Hampson, Mr. Elwes, and Mr. Poulton took part in the discussion which ensued.

Royal Meteorological Society, April 20.—Dr. C. Theodore Williams, President, in the chair.—Reference was made to the death of Dr. J. W. Tripe, who had held the office of Council Secretary for the last twenty years, and a resolution of sympathy with the family was passed by the meeting.—The following papers were read:—Anemometer comparisons, by Mr. W. H. Dines. This was a report on a valuable series of experiments which have been carried out at the request of the Council of the Society, with the view of obtaining a direct comparison of the various anemometers in common use, so that some opinion might be formed as to which type of instrument is the most suitable for general purposes. The Meteorological Council have defrayed the cost of the work. The anemometers which were compared were—(1) Kew pattern Robinson; (2) self-adjusting helicoid; (3) air meter; (4) circular pressure plate (one foot in diameter); and (5) a special modification of tube anemometer. Most of these instruments are of the author's own invention, as well as the apparatus for obtaining automatic and simultaneous records from all the instruments upon the same sheet of paper. It appears that the factor of the Kew pattern Robinson is practically constant, and must lie between 2·00 and 2·20. The helicoid anemometer is quite independent of friction for all excepting light winds, and different sizes read alike, but it is not so simple in construction as the cup form. The air meter consists of a single screw blade formed of thin aluminium, and made as nearly as possible into the exact shape of a portion of a helicoid. A similar instrument with a larger blade, and with the dial protected from the weather, would probably form a useful and correct anemometer. It would be light, and offer a very trifling resistance to the wind. The oscillations of the pressure plate must have been considerably damped by the action of the floating weight, but as it was, they were sufficiently violent. It seems probable that the remarkably high values sometimes given by the Osler pressure plate may be due to the inertia of the moving parts. The tube anemometer appears to possess numerous advantages. The head is simple in construction, and so strong that it is practically indestructible by the most violent hurricane. The recording apparatus can be placed at any reasonable distance from the head, and the connecting pipes may go round several sharp corners without harm. The power is conveyed from the head without loss by friction, and hence the instrument may be made sensitive to very low velocities without impairing its ability to resist the most severe gale.—The hurricane over the West Indies, August 18–27, 1891, by Mr. F. Watts. The author has collected a number of observations on this violent hurricane, which on August 18 swept from the Atlantic into the Caribbean Sea, and moved in a north-north-westerly direction over San Domingo, and thence northward and eastward. At Martinique the barometer, which at 5.30 p.m. stood at 29·80 inches, fell to 28·38 inches at 8.15 p.m., during the passing of the centre of the cyclone.

Chemical Society, March 30.—Annual General Meeting.—Prof. A. Crum Brown, F.R.S., President, in the chair.—The President delivered an address, in the first part of which he referred to the favourable position of the Society. In the remainder of his address he dwelt chiefly on the work which is being done on the border-lines of chemistry proper, referring both to that by which an approach is gradually being made towards understanding the chemistry of Nature's organic laboratory, and to the solution of chemical problems by the application of mathematical and physical methods of inquiry. A vote of thanks to the President was carried by acclamation.—After the usual reports by the officers of the Society had been presented, a ballot was taken for the election of officers and Council for the ensuing year. The following were subsequently declared elected:—President: A. Crum Brown, F.R.S. Vice-Presidents who have filled the office of President: Sir F. Abel, F.R.S.; W. Crookes, F.R.S.; E. Frankland, F.R.S; J. H. Gilbert, F.R.S; J. H. Gladstone, F.R.S.; A. W. Hofmann, F.R.S.; H. Müller, F.R.S.; W. Odling, F.R.S.; W. H. Perkin, F.R.S.; Sir L. Playfair, F.R.S.; Sir H. E. Roscoe, F.R.S.; W. J. Russell, F.R.S.; A. W. Williamson, F.R.S. Vice-Presidents: A. V. Harcourt, F.R.S.; W. N. Hartley, F.R.S.; J. Pattinson; W. Ramsay, F.R.S.; W. A. Tilden, F.R.S.; R. Warington, F.R.S. Secretaries: H. E. Armstrong, F.R.S.; J. M. Thomson. Foreign Secretary: R. Meldola, F.R.S. Treasurer: T. E. Thorpe, F.R.S. Ordinary members of Council: H. Bassett; N. Collie: H. Dixon, F.R.S.; J. Ferguson; R. J. Friswell; J. Heron; M. M. P. Muir; F. J. M. Page; W. H. Perkin, Jun. F.R.S.; S. U. Pickering; J. A. Voelcker; W. P. Wynne.—Correction of a note on a new acid from camphoric acid, by W. H. Perkin, Jun. The author desires to express regret that he had overlooked a previous paper by Damsky, in which an account is given of the acid recently described by him as new.

Mathematical Society, April 14.—Prof. Greenhill, F.R.S., President, in the chair.—The following six foreign mathematicians were elected Honorary Members of the Society, viz. Messrs. Poincaré, Hertz, Schwarz, Mittag-Leffler, Beltrami, and Willard Gibbs.—The following short communications were made:—Second note on a quaternary group of 51,840 linear substitutions, by Dr. Morrice.—Note on the skew surfaces applicable upon a given skew surface, by Prof. Cayley, F.R.S.—Mr. A. B. Kempe, F.R.S., made an *impromptu* communication on regular graphs.—Mr. J. J. Walker, F.R.S., Dr. M. J. M. Hill, Lieut.-Colonel Cunningham, and the President joined in the discussion on the above communications.

EDINBURGH.

Royal Society, April 4.—Sir Arthur Mitchell, Vice-President, in the chair.—Dr. Thomas Muir read a paper on a problem of Sylvester's in elimination, and also a note on Prof. Cayley's proof that a triangle and its reciprocal are in

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perspective.—Prof. Blackie read a paper on the most recent phases of Greek literary style. The style of the educated Greek and the popular style were brought into closer correspondence than previously at the commencement of the present century, chiefly through the influence of Coraes. In this paper Prof. Blackie investigates the result of that amalgamation. Since 1830, the development of the Greek language has been most marked. The higher classical style has been constantly gaining ground, so that popular and literary Greek now differs as little from ancient classical Greek as Scotch does from English; while, previous to the time of Coraes, they were as distinct as present-day English is from the English of Chaucer. The author gives examples of the deviations of the literary and popular Greek of various epochs from ancient Greek, which prove a rapid return to the ancient purity of language. Thus, while in twelve lines of Romaic Greek eighteen or twenty deviations from the pure style may be found, in twelve lines of modern Greek only two or three such deviations appear. In the first five verses of the second chapter of Luke, nineteen deviations occur in the Romaic New Testament, while in the same passage in the English Bible Society's version of 1890 only four are found. In two pages of a recent number of a Greek newspaper only two deviations occur.—Dr. Berry Haycraft communicated a contribution, by Mr. F. E. Beddard, to the anatomy of *Sutroa*.

PARIS.

Academy of Sciences, April 19.—M. d'Abbadie in the chair.—Calculation of the diminution which is experienced by the mean pressure on a fixed horizontal plane, in the interior of a heavy liquid filling a basin and agitated by certain wave motions, by M. J. Boussinesq.—Note by M. Faye accompanying the presentation of celestial photographs obtained at Heidelberg by Dr. Max Wolf, Director of the Observatory. The photographs commented upon by M. Faye are those recently taken of a part of Cygnus, and that on which the trail of a new asteroid was detected; also a picture showing a shooting-star which crossed the field of observation during exposure. The photographs were taken by means of a portrait-lens 2½ inches in diameter.—On the optical measure of high temperatures, by M. A. Crova.—Researches on the formation of planets and satellites; memoir by M. E. Roger, presented by M. Jordan. The author has developed a complex relation connecting the distances of planets from the sun, and also one connecting the distances of planets from their satellites.—Observations of Swift's comet (1892 March 6), made with the Brunner equatorial of Lyons Observatory, by M. G. Le Cadet. Observations for position were made on April 3, 4, 8, 9, 11, and 15.—On differential invariants of a surface with respect to conformable transformations of space, by M. Arthur Tresse.—On the accuracy of comparisons of a *mètre à bouts* with a *mètre à traits*, by M. Bosscha.—Researches on the secondary wood of Apetales, by M. C. Houlbert.—On the relations existing between the form and nature of the beds of andalusite at Ariège, by M. A. Lacroix. It appears that at Ariège the form of the andalusite is characteristic in each bed to such an extent that, given a geological map of the region, it is possible to indicate *a priori* where the mineral would be found, and conversely, given a specimen of andalusite, the geological nature of the bed from which it was taken could be stated with very little chance of error. The facts described by M. Lacroix are thus as useful to the geologist as to the mineralogist.—On the loess of Turkestan, by M. Guillaume Capus.

BRUSSELS.

Academy of Sciences, March 5.—The following communications were read:—The male of certain Caligides, and a new species of this family, by M. P. J. Van Beneden. The author describes (1) the male of *Pandarus Cranchii*; (2) the male and female *Pandarus affinis*, n. sp.; (3) a new species, *Chlamys incisus*; and (4) the male of *Dinematoura elongata*.—Theoretical determination of the radius of the sphere of molecular activity of liquids in general, by M. P. De Heen. The conclusion is arrived at that the radius of the sphere of activity is proportional to the product of surface tension into molecular volume.—On the curve in conic sections, by M. Cl. Servais.—Researches on the physiology of respiratory centres, by Dr. Alfred Bienfait. The author adduces evidence to show that a single respiratory centre, isolated by two transverse sections from the accessory respiratory centres, controls the movements of the glottis.—On a new ptomaine obtained by the culture of *Bacterium Allii*, by Dr. A. B. Griffiths. In a former paper Dr. Griffiths described and named *Bacterium Allii*—a micro-organism found by him. This Bacteria produces a green pigment, soluble in alcohol, and possessing a particular absorption spectrum. In the presence of albuminoids, *Bacterium Allii* gives rise to a crystallizable ptomaine, which furnishes a chloroplatinate, having the formula, according to analyses, $(C_{10}H_{17}NHCl)Pt_2Cl_4$. The analysis of the base gave the formula $C_{10}H_{17}N$, which corresponds to that of chloroplatinate.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

BOOKS.—The Apodidæ: H. M. Bernard (Macmillan).—Tanganyika: E. C. Hore (Stanford).—Epidemics, Plagues, and Fevers: Hon. Rollo Russell (Stanford).—Hand-book of Jamaica, 1892 (Stanford).—A Treatise on Physical Optics: A. B. Basset (Bell).—The Landfall of Lief Erikson, A.D. 1000: E. N. Horsford (Boston, Damrell and Upham).—A Guide to Electric Lighting: S. Bottone (Whittaker).—Elementary Lessons in Heat: S. E. Tillman, 2nd edition (Gay and Bird).—Les Altérations de la Personnalité: A. Binet (Paris, Alcan).—Thermodynamische Studien: J. W. Gibbs, translated by W. Ostwald (Leipzig, Engelmann).—English Botany, supplement to the 3rd edition, Part 1: N. E. Brown (Bell).—Progressive Mathematical Exercises, 2nd series: A. T. Richardson (Macmillan).

PAMPHLET.—The Wheat Plant, how it Feeds and Grows: W. Carruthers (also 8 diagrams) (W. and A. K. Johnston).

SERIALS.—Proceedings of the Rochester Academy of Science, vol. i. Brochures 1 and 2 (Rochester, N.Y.).—Brain, Part 57 (Macmillan).—Journal of the Bombay Natural History Society, No. 4 vol. vi. (Bombay).—Journal of the Institution of Electrical Engineers, No. 97, vol. xxi. (Spon).—Bulletin of the New York Mathematical Society, vol. i. No. 7 (New York).—Physical Society of London, Proceedings, vol. xi. Part 3 (Taylor and Francis).—Proceedings of the Geologists' Association, vol. xii. Part 7 (Stanford).—Journal of the Royal Microscopical Society, April (Williams and Norgate).—A Manual of Orchidaceous Plants, Part 8 (Veitch).—Notes from the Leyden Museum, vol. xiv. Nos. 1 and 2 (Leyden, Brill).—American Journal of Mathematics, vol. xiv. No. 2 (Baltimore).—Transactions of the Royal Society of Victoria, vol. ii. Part 2. 1891 (Melbourne).—Report of the Geological Survey of India, vol. xxv. Part 1 (Calcutta).

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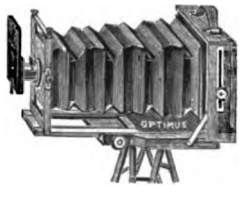

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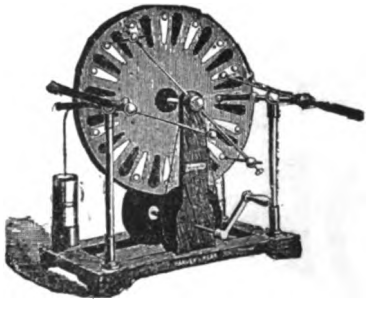

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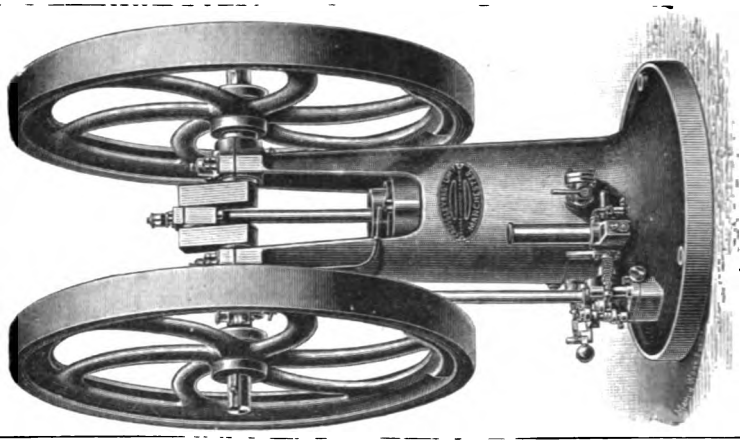

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